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Permitting Creativity in Science

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To every thing there is a season,
and a time to every purpose . . .
A time to plant
and a time to pluck up that which is planted . . .
A time to break down,
and a time to build up. (Ecclesiastes 3:1-3)

This chapter is based on one assumption, that the best-known part of scientific research—the published report—is the visible fruition of a much longer process, the beginnings of which are different from its final form. This longer process can be differentiated into stages, and the activities appropriate at the later stages are premature and infelicitous at the beginning. The critical and analytical way of thinking that is vital in the final stages leads to the certain death of creativity in the early stages. The time to plant or build up comes *before* the time to pluck up or break down.

Most of us know this, although we do not usually say so in methods books or classes. This discrepancy between what is said and what is done can handicap the new researcher fatally. I recall the plot, although unfortunately not the author, of a science-fiction short story:

It is decades in the future, and the U.S. has been isolated from the outside world for 50 years, since a plague killed off everyone over the age of 10, and the surviving children sealed the borders. Unex-

pectedly, after 50 years, the child-government summoned a delegation from Europe for “consultation.” The curious Europeans landed and were greeted by men and women in their fifties, dressed in cowboy suits, little-girl dresses, and other childish outfits. Without much diplomatic delay, these old children got to the point: We asked you to come because, in all this time, there have been no babies. We have done everything our parents told us; we have looked under cabbage leaves, encouraged the storks, and watched birds and bees. And we have been good boys and girls. But there have been no babies.

If we tried to create new ideas following what is taught formally about research, we would be like those sad old children, looking under statistics instead of cabbage leaves. Research too is a generative process, full of passion, fumbling and disorderly. I have never encountered a methods course or text with any of these characteristics.

What is written about the creative process? The choice is usually an unpalatable one, between a cold caste system of “creative” versus “uncreative” persons and, on the other hand, a fuzzy romanticism of inspiration in dreams, in bathtubs, and while gazing into fires. An alternative view is both egalitarian and demystifying: *Creativity in the early stages of science is a way of thinking that can be learned and practiced.* It has a logic of its own and requires mental discipline and effort of reasoning just as the later stages do. Presumptuous as this is, I will try to propose what that logic may be, based much less on reading philosophers of science than on doing, watching, supervising, and talking to researchers. As talking to a wide spectrum of other scientists has confirmed, little of what follows is original. In my case, it came from the experimental tradition in which I was privileged to be trained, one that can be traced back through Alex Bavelas to Kurt Lewin and Max Wertheimer (not their Gestalt theories but their way of working). I assume that I am simply writing down my experience and adaptation of this tradition.

An initial distinction must be made and maintained between an idea that follows from earlier work and one that has no known predecessor, that is, between research that comes *after* an original idea has entered the field and the first entrance of such an idea. There is an important role for the former; indeed, if no one followed up on original ideas, we would be buried in fresh starts going nowhere. However, the “how to” of this kind of research is fairly well known, because this is what journal articles and methods

Author's Note: I would like to thank my teachers and my students, especially the Psychology and Sociology graduate students in our two recent research seminars. Tamara Dembo, Lee Sechrest, John Furedy, and David Rosenhan contributed material and/or comments on an earlier version.

books describe: The researcher starts with the idea and sets out to test it. But new ideas have to enter the field somehow, and the focus of this paper will be on how to “get” them or, more important, how to keep from killing them.

Anyone who has been attracted to a field probably began noticing and thinking about it long before graduate or even undergraduate training. (For example, most psychologists have been people-watchers from an early age.) These continuing observations are the source of new ideas. The reshuffling of ideas already in the field must, by definition, be limited to whatever such a recombination can achieve, and the result can be at best a new permutation of old ideas. New ideas by definition must come from outside those familiar confines. It is possible that they might be transplanted, by analogy, from another field, but I think that the best source is in the phenomena the field aims to study, for example, in observations of human behavior. I happen to have the good fortune to be interested in verbal and nonverbal communication, which are fully observable. Therefore, unlike colleagues specializing in cognition and physiology, I can observe directly—in the supermarket, in meetings, while waiting in airports, or wherever two or more individuals are together. But no matter how technical the process of observation becomes, even if by implantation of microelectrodes by the neurophysiologist or the production of new mutations in maize, as long as there is access to the phenomenon of interest, new observations can be made.

The Care and Feeding of Hunches

The fate of those new observations is another matter, which will be described first. It is often a grim fate: Miscarriage or abortion is the rule. Usually we immediately apply to a novel observation the same standards that should be applied to a fully mature hypothesis, and of course a newborn observation cannot withstand such treatment. The first category of “how not to” reactions can be summarized as “Get rid of it.” Several of the most common methods will be described and proscribed.

Don't Dismiss It

First, there is the simple expedient of saying, “It didn't happen.” We notice an intriguing bit of human behavior (in a coffee shop,

in the newspaper, in a novel), some fascinating tidbit that is striking, for some reason. As will be seen, we must learn to trust that reason—but that would be getting ahead of the story, because what *not* to do matters most at this stage. Right now, it is sufficient to point out that casual observations can lead to solid discoveries.

Example: Lewin noticed in a coffee shop that the waiter could remember everyone in his group and what each person ordered. However, he forgot everyone completely as soon as the bill was paid and his task completed. This observation eventually yielded the classic “Zeigarnik Effect” (Zeigarnik, 1938), in which interrupted tasks are remembered better than completed ones. (Tamara Dembo, personal communication, May 1985)

Unfortunately, having seen something interesting, too often we shake our heads and say it didn't happen: I must have misperceived, misunderstood, or imagined it.

A more sophisticated variation on this is the invocation of “chance” to dismiss novelty: It was just a fluke, just a coincidence, a one-time-only random event. Notice how early in the game the null hypothesis is being used, how the model of statistical inference that serves us well if applied precisely later on is being used here quite sloppily, without any idea of the probabilities operating in this particular instance. Dismissal at this point can only be prejudice, because it is too early for anything else. It is certainly true that this little observation may never occur again, or it may turn out to have been a mere coincidence. Either outcome is totally irrelevant, because the role of the observation is to seed thinking, not to provide evidence for a formal hypothesis. That kind of evidence is essential later and must be given its due then, but to bring all that machinery to bear now can have only one effect, that of eliminating new observations.

Example: A friend of mine, who is a neurophysiologist with a knack for upsetting dogma, recently found an axon that conducts both slow and fast potentials, one a sodium-dependent action potential and the other a calcium “spike.” This is the first known instance of two types of impulse propagation in a single nerve fiber, and how important it is ultimately depends on whether or not it is a singular oddity. Even now, though, it raises an interesting question: With all those neurophysiologists hooking up neurons to recording devices, why did no one notice this before? My favorite hunch is that many of them did—and then kicked their oscilloscopes or filtered out the

second potential, because everyone knows there is only one kind per neuron.

In other words, "It isn't there, because we don't know about it already." This is not exactly a formula for originality, and yet it is one that we unwittingly impose on ourselves and our students. In the case of my friend, it is important to point out that, later, the critical process of eliminating artifacts and alternative explanations dominated everything, as he and a colleague sought to "kill" the discovery by every means possible. This testing of alternative explanations is the essence of the published report (Mackie & Meech, 1985). But the possibility only got to that stage at all because *at the beginning*, it was accepted and nurtured into a full-fledged hypothesis.

In 1620, Francis Bacon (1962) warned against judging one's observations by preconceived notions of what should be seen. He called this the "Anticipation of Nature" and condemned it as rash or premature. More than three centuries later, B. F. Skinner (1948, 1959) described this error more vividly by admitting (through a fictitious character in *Walden Two*):

I remember the rage I used to feel when a prediction went awry. I could have shouted at the subjects of my experiments, "Behave, damn you, behave as you ought!" Eventually I realized that *the subjects were always right. They always behave as they ought.* It was I who was wrong. I had made a bad prediction. (1959, p. 378, italics added)

When a hypothesis, or "what everyone knows," turns out to be not so, the results might be unexpected, inexplicable, and worth double-checking, but they cannot be in any sense wrong. Rather, "the subjects are always right," meaning that people (or animals or any phenomena) behave as they behave; it is our job to figure out how and why.

Logically, one might even speculate that the more important the novel idea is, the more likely that it will appear wrong at first, because it would inevitably upset the orthodox view.

Example: In the early twenties, Cecilia Payne-Gaposchkin presented evidence that the cosmos was made up principally of hydrogen. When her thesis supervisor questioned this, she accepted these doubts and published that her estimate "is improbably high, and is almost certainly not real." Within five years, her advisor and everyone else had confirmed her numbers. (Morrison, 1985, pp. 35-36)

More recently, Barbara McClintock's Nobel Prize for ideas that were rejected for decades (Keller, 1983) reminded us once again that, while not every unexpected finding is a discovery, the big discoveries will always be unexpected findings. As Lewis Thomas (1974) concludes,

A good way to tell how the work is going is to listen in the corridors. If you hear the word, "Impossible!" spoken as an expletive, followed by laughter, you will know that someone's orderly research plan is coming along nicely. (p. 140)

Don't Go Find a Category to Put It In

We have many ways of dismissing new observations, of explaining them away as if our goal were to be rid of them as quickly as possible. "It didn't happen" and "It was a fluke" have just been mentioned. If those fail, then another pair of techniques will succeed for sure: We can go to the literature or to an expert. To try to find my observation in the existing literature, I will have to make a fatal transformation, that is, to put it into words and therefore into categories that already exist in that literature; how else could I look it up? But having done that, it is now no longer itself, just a case of something already in the *Abstracts*, so I might as well close the volume and leave: Nothing new will be found there.

It is noteworthy that social science reference sources are indexed more by concepts than by phenomena. Even the names of many phenomena anticipate their nature by the metaphor used (for example, "reinforcement" or "extinction"). As Bacon noted,

Words are mostly cut to the common fashion and draw the distinctions which are most obvious to the common understanding. Whenever an understanding of greater acuteness or more diligent observation would alter those lines to suit the true distinctions of nature, words complain. (1962, p. 19)

Should the researcher read nothing? Of course not, but as Sir Peter Medawar advises:

Too much book learning may crab and confine the imagination, and endless pouring over the research of others is sometimes psychologically a research substitute, much as reading romantic fiction may be a substitute for real-life romance. . . . The beginner *must* read, but intently and choosily and not too much. Few sights are sadder than that of a young research worker always to be seen

hunched over journals in the library; by far the best way to become proficient in research is to get on with it. . . . (1979, pp. 16-17)

Returning to how to get rid of the new idea, an expert—acting in the role of expert—is an even more brutal means than is the literature. If I take my fledgling idea to an expert, he or she will (with more or less withering patience) explain that this is an instance of X, where X is an already established category (usually the expert's specialty). And it may not even be a very good instance of X. Recall that the "ugly duckling" could not be seen as a swan because no one knew what swans were; therefore, ugly or not, he must be a duckling.

Example: Our research seminar was discussing the way that shoppers sometimes examine things so carefully and often so needlessly in the supermarket, picking up item after item for careful inspection and rejection before a final selection is made, one that is presumably not different in any important way from the others. Everyone had an "explanation" (that is, categorization): "over-learned behavior," "decision-making," "cognitive dissonance," "reactance," "effectance," "irrational selection behavior," and of course there must be "individual differences." None of our ideas was anymore or less silly than most labels experts put on things, but they effectively extinguished the will o' the wisp that was originally intriguing—until the student who initially brought up the example added more examples, in different settings, and finally captured the quality that interested him; it was the "Elmer Fudd" look about it. An odd comment, but the best of them all; this might be an idea on its way.

In the early stages, words can kill. They label, make things familiar, and make the world safe again. Nothing new and unknown running loose out there any more, thank heaven.

The categorizing reflex of the expert is as predictable as any phenomenon I know in science. Yet it has the disturbing implication that new observations are not to be left loose on their own but should be pigeonholed as soon as possible; they should be captured and rendered harmless in an already existing enclosure. We become taxonomists, classifiers, dog catchers whose goal it seems is to leave nothing running around loose. We never say, "I don't know . . . yet." This is even more puzzling when we consider how easily the very basis for classification can change. In active scientific fields, revoltion seems a daily event; yet the introductory student is taught a cut-and-dried view of the world. Perhaps, as Lewis

Thomas suggests (1980, pp. 143-155), we should initially emphasize instead the mysteries, the paradoxes, in short, how little is known.

Don't Belittle It

If an intriguing observation should happen to survive the above treatment, or if its proponent is wise enough to avoid words, experts, and literature at this stage, then it may achieve the right to exist and not to be dismissed. Stripped of any label or a place in the known literature, however, it is easy to belittle it by holding it too tightly, too literally. Because it is at present only "one instance of something," it is easy to think of it as insignificant. Any single instance of anything looks trivial. Finches in the Galápagos were as isolated a bit of trivia as anyone could sail halfway around the world to find. And Senator Proxmire, as we know, can make any research look ridiculous.

If the phenomenon will not be trivialized, I may still belittle it in another way, by devoting myself to looking for *literal repetitions* of the "same" event. In the example above from the research seminar, shoppers selecting mushrooms, cheese, or magazines would be narrow repetitions of the same first example, which needs to be expanded to quite different settings and instances. Many published research papers are even more trivial variations on what has been done already. Lewin would say that these are not "psychological problems," meaning that the focus is on literal, physical similarity rather than on a class of events that has an abstract, psychological similarity.

The worst path of all is to build an experimental imitation immediately and begin to crank out little plastic replicas. Of all the criticisms leveled at Milgram's "obedience" research, there is one I have not seen, the most important to me: We knew that. The world had just seen genocide on a very wide scale, in the Ukraine, in the Holocaust, and the dropping of two atomic bombs on civilian populations. We did not need a replica in the lab; we needed, and still need, to know why—to understand the process, not to repeat it. Milgram's experiment belittled and trivialized the phenomenon rather than leading us closer to its nature.

Don't Be Practical or Critical

If all of the above efforts to rid myself of this new observation have failed, I can always resort to the big guns. Point the full force

of experimental design and logic straight at it and let fire. That is, start thinking of how to set up a control group (For what? Not known yet!). Or better yet, begin thinking of it in entirely statistical terms, in those prefabricated packages called analysis of variance designs. Finally, consider the practical limitations of finding enough of the right kind of subjects, the cost of the experiment, the ethics committee, deadlines, and whether or not the results will be publishable. If the hunch can even be found after this barrage, I cannot have aimed very well.

Before firing, though, it might be a good idea to ask, "If not now, when?" If the undergraduate years are devoted to getting the grades to get into graduate school, and the graduate years are devoted to getting the degree and publishing in order to get an academic position, and the first several years of this assistant professorship are devoted to getting tenure, *then* are you going to do original work you believe in? After more than twelve years of accommodation, the very least one could expect is a period of reeducation, which "the system" does not provide for.

Even the earliest work is often judged by an apparently simple and reasonable criterion, that is, the final thesis or journal article. The standard format of

Introduction (literature and theory)
Method
Results
Discussion

implies a fixed sequence of segregated stages. This is an efficient format for reporting research, and I am certainly not prepared to read diaries or autobiographies on "experiencing this research." But the way in which the research report is organized should not be confused with the way in which the research *process* is organized. In the early stages, trying to dress an infant idea in these clothes is absurd and dysfunctional.

So we return to the idea of stages. I have in my office a worktable, across which my students usually sit. In the beginning of the year, when this table is unnaturally clean, it makes a good measuring stick for what we are about to embark upon. Its entire length of about five feet is the research process from beginning to end. The last six inches are the thesis. The six before would include the experiment itself, and perhaps the six before that are "experimental design." The three and a half feet stretching back from there are

where we begin and what we go through to get there. So it is a good idea to slow down and go the distance.

Don't Panic

All of the above examples of "how *not* to . . ." are probably familiar to the reader. It is possible that the cause is simple, namely, hurrying the process so that the later stages are begun too early. When I asked the students in my research seminar for the single most important negative maxim, they said, quoting *The Hitchhiker's Guide to the Galaxy* (Adams, 1979), "Don't panic!"

This insight has a striking structural resemblance to the first principle of "brief" or "strategic" psychotherapy (see Fisch, Weakland, & Segal, 1982; Watzlawick, Weakland, & Fisch, 1974), which is: *Our problems are caused by our efforts to deal with the problem.* The more we throw ourselves at it, the worse it gets. Many of you will recognize the old Gestalt "detour problem" here, in which the only way to the goal is away from it. In other words, the pressure to get on to the later stages of research leads to applying those standards too early on, with the inevitable consequences of setting us back further. We must calmly resist charging ahead, in order to get anywhere.

There are those permanently in a hurry, unwilling to risk the possible waste of a few weeks or months of apparently idle (though intense) thinking. And there are those who are deeply afraid that a research topic will not be found (as if there were a shortage!). For those who have given up curiosity, a sense of wonder, and respect for that which is unknown, however small, it would be best to choose work in fields already cleared, to be a ploughman and not to seek new lands. There will be an abundance of techniques to be exploited, of fashionable topics to be followed, and short-term success is guaranteed.

How To . . .

By now I have painted a dismal picture of the systematic abortion of fetal ideas when they are only a "gleam in someone's eye." The rest of the time will be spent more positively, on "how to . . .". For those who require them, respectable credentials can be marshaled for this advice, not only Bacon (1620) but Peirce on "abduction" (Fann, 1970), Polanyi (1966) on "tacit knowing," and Neisser

(1976) on schema. And, from theoretical particle physics (arguably as far as one can be from behavioral science), the Nobel Prize winner, Richard Feynman (1983):

People say to me, "Are you looking for the ultimate laws of physics?" No, I'm not. I'm just looking to find out more about the world. And if it turns out there is a simple ultimate law which explains everything, so be it. That would be very nice to discover. If it turns out it's like an onion with millions of layers, then that's the way it is. But whatever way it comes out, *its nature is there and she's going to come out the way she is. And therefore, when we go to investigate it, we shouldn't pre-decide what it is we're trying to do, except to try to find out more about it . . . and the more I find out, the better it is to find out.* (pp. 15-16, italics added)

In simple terms, the researcher must assume that there *is* something there, which is why it was noticed, and that the job is to find out what it is. Strange as it sounds, this means exploring one's own thoughts as much as the phenomenon at this point, trying to find out what the hunch is. So the first (positive) principle, as summarized by my students, is, "Have faith"—in the phenomenon, in yourself. You must believe that you noticed something, at least glimpsed it out of the corner of your eye. So now, how do you get it into full view? (A "case history" will be given at the end of this section, as an example of the process being described.)

Generate More Examples

As already emphasized, you do *not* put it into words; rather, you find more examples. What does this remind you of? Is there another observation that "felt" like this one? Not literal examples, remember, but rather ones that "somehow" have the same quality. This tack has two major advantages: It keeps the thinking analogic rather than verbal, and it will improve the signal-to-noise ratio in the first example. The pitfalls of words have already been mentioned; they lead to familiar categories, familiar ways of thinking, no resolution where ambiguity should be preserved. An example is itself, not a verbal summary. But an example is also particular, and its specifics are distracting. That is, the first example has some qualities of the idea that is there, but it is also full of irrelevant details. Each additional, new example is like an overlay that varies in its particulars but reveals the common pattern; this is how more

examples reduce the "noise" in the first one. Later, it is often surprising how far off the first example was; it is seldom the true exemplar, so it is important to seek the other instances that may be better.

Develop a Schema or Class

The object of this search is not the perfect example but the class to which they all belong. You must assume that the initial observation resonated to some class of interesting phenomena, which you know but cannot yet articulate. We seem to have a very powerful natural tool for finding this class, which Neisser (1976) called a "schema," a cognitive category that actively selects (rather than passively receives) from the environment. When you do not know anything about bird songs, you seldom hear them. But once you begin to learn about them, you suddenly hear birds where there was silence before. Perception is active, not passive, driven by schemata or cognitive classes. Similarly, once you begin to have a critical mass of examples of your vague idea, it will suddenly assault you from all sides; you will be unable *not* to see it. In my area, every social encounter, every novel or short story, every movie becomes full of exactly what it is I am currently most interested in. This hyper-tuning, this exaggerated observation is invaluable, not as evidence, for it is totally, hopelessly biased, but for helping to answer the important question, what class does this belong to?

The only legitimate questions at this point are, What interests me about this? And, What else is like it? If this loose approach seems self-indulgent or solipsistic, you might try promising yourself that you will do all the proper scientific things later, when it is their time. But you need not at present prove to yourself (as you must in future to your peers) that your inkling is a valid idea.

This might mean that you will work alone at first. Someone who practices "how not to" is obviously to be shunned at this stage. On the other hand, a collaborator, teacher, or listener who has faith and keeps you on the right track is invaluable; indeed, I find it difficult to understand how research can be done alone. However, remember that others cannot have your emerging schema; they cannot pursue an idea that is essentially in yourself. They cannot know why a particular observation intrigues you or which other instances are examples of "the same thing." They may, understandably, intrude their own interests instead. Initially, you must generate the examples and develop a schema on your own. The best contribution oth-

ers can make is to listen, encourage, and remind you not to panic, to have faith.

Articulate by Analogues

The next step can begin to involve others, or at least those who are uncritical and imaginative listeners. As you try to find the principle contained in this class of examples, you must begin to articulate it, but not in words, especially not in the technical terms of your field. This articulation, this extraction of essence must first be in analogues: It is *as if* such and such were going on. Metaphor and analogy are as powerful for us at this stage as they are for the poet. They catch the feeling and express it when there are no words. They are rich, saturated, yet uncritical, eloquent without being unnecessarily precise.

Analogies form a bridge to the ultimate necessity, words. The first words must be like these analogies, loose and undemanding. The trap that is always there is the familiar term or phrase, the well-trodden line of thought onto which new ideas may be derailed, smoothly sidetracked into an already known world. Unless the familiar is what you seek, do not trust words too early on. Use them only as they construct analogies and as they capture your idea. Verbal analogies are still “like” your examples; formal, abstract words will cut that connection forever. The latter will permit and enable the necessary logical organization and formalization of the idea, but they will not capture the intuition now. Young ideas may appear vague in words and short on details, but they can still produce vivid images, sharp and clear.

Unearth Your Model

Notice that I have assumed that what caught your attention was a class of phenomena, not a unique event. This is because I assume that interest, experience, and expertise lead to the accumulation of observations that may surface in this way, as particulars that spring from a more general principle as yet unknown. If you have gotten past the particular, generated a schema, and begun to articulate it, you are on the way to unearthing the model that called your attention to the first and to subsequent observations. Whether the model is about social rules, cognitive organization, or a new origin of the universe, it is a set of operating principles explaining the initial observation (and its fellows). Your job is now that of an archae-

ologist, carefully unearthing what is there. Note again that we discover, we do not invent; a major premise here is that this model is there to be uncovered.

What is uncovered may threaten to be very big, much bigger than the simple research topic that is needed. But it should not be chopped off. Unearth all of it, assuming that there is, as Andrew Marvell put it, “world enough, and time.” Pretend that you have all the time, money, and resources ever needed at your disposal; you will not end up needing them, so get them out of your mental way. There is no obligation to explore or test the whole model immediately, but it will serve as a useful map for tactical decisions, for example, for which particular hypothesis should be tested first for maximum information. There are worse problems than having too much to do that is important.

Pilot Work: Arranging Experiences

At this point, if lucky enough in your choice of field, you will be able to begin arranging to see more of your new notion. As I mentioned, my research group’s interests are observable in everyday human behavior, so we can place ourselves in an advantageous position for observation. One of our current interests is what we call “mimetic synchrony,” the virtually simultaneous occurrence of similar nonverbal behaviors in an interacting dyad. So in an interaction (in restaurants, television interviews, or parks), it is possible to observe the conversants’ mirroring each other’s posture, movement, or gesture. We can also ask people to have a conversation in front of our cameras, and we can go on to vary the conditions of that conversation to test our hypothesis that mimetic synchrony is a function of the relationship between the interactants (Davies, 1984).

Thus, if you are in an experimental field where instances can be made to happen, you will be able to begin seeing whether you can get it to happen—*not* whether you can “prove” it happens, that is, eliminate alternative explanations and artifacts—just whether you can see it *apparently* happen.

This is the true nature of “pilot work,” whose metaphoric meaning is lost when we use it later merely to check procedural details before the final experiment. It should be the nautical steerman, the pilot taken on board to guide a ship into waters not yet familiar. Such “arranged experiences” (as I believe Wertheim called them) may in fact turn out to be prototype experiments, but that would be a bonus. Right now, they are primarily an extens-

of that first principle: Find yet another example, this time more deliberately.

The Final Phase

From this point on, it all gets familiar. The idea can now be presented to others in a preliminary but respectable manner. At least it is a “fleshed out” observation, not just a “feeling”; there are the beginnings of a model; empirical work has begun. With a little more nurturing, the idea will be able to stand on its own, even to withstand the necessarily harsh examination imposed by formal methods. This formal machinery is designed to subject ideas to rugged testing conditions, to guard against all the errors and biases that have until now been indulged so freely. Yet its very severity confirms the preceding phase, as we now accept the need for such control against ourselves, implying the power of ourselves and our own intuitions. Our favorite hunches must be strong indeed to require such formidable attempts at eradication. In any case, now it can be unleashed.

I am a firm believer in “crucial experiments,” at least in severe testing that can prove me wrong. The only thing worse than being wrong soon would be wasting 20 years before finding out. I always want to be right (first choice) but, if not, then at least wiser (second choice). Yet we often hold back from such a test, for fear of a contrary result. Volumes could be written on the role of fear in research (and the “play it safe” research that seems to be a consequence). Fear stalks us from the beginning and continues throughout, until the results are in and checked. Certainly overdoing the final design is one direct consequence of this fear; with enough variables and statistics, a result of some kind can be guaranteed, although the cost of this guarantee is often that the result will be relatively uninteresting and unconvincing.

But enough of this well-known phase. My first premise was that research is a differentiated process, occurring over time, and that what is necessary at one stage may be harmful at another. Especially by delaying the later, formal stages, we can keep alive new ideas that might otherwise never grow into creative, new directions.

Extended Example

Jennifer Mullett, a graduate student with a good deal of research experience, meets with me weekly to kick around ideas. The depart-

ment may, at different times, call this activity a “research apprenticeship,” “directed studies,” or “thesis supervision,” and she registers in different course numbers depending on the formal title. Regardless, we meet an hour and a half at the same time, with provisions for extra meetings when necessary or desirable, and we talk about things that may end up as experiments. We had been discussing Erving Goffman; Jennifer had done an interesting paper for another course using Victorian “novels of manners” to illustrate Goffman’s ideas. It is hard to say when or how much her *M.A.* thesis became the goal of the discussion to be described. Early on we were definitely just pursuing other research to do for its own sake, because she already had a thesis topic. Over the weeks, the topic became impractical, and we were looking for a new one; our discussions became the logical source.

Jennifer brought up an example of an awkward interpersonal situation that had happened more than once: She and a friend walking across campus, and a third person (whom she does not know) stops to talk to the friend. Jennifer is left in a mildly embarrassing position, because she can neither join the conversation nor leave; she is “there but not there.” I recognized the situation and agreed about its ambiguous, unresolved quality; usually, in these circumstances, I involve myself intently and conspicuously watching something else.

As we are both over 30, there was an initial tendency to see this as a problem of a student generation bereft of traditional social skills. In the friend’s position, she or I would either keep the encounter extremely brief or make formal introductions so that the three people would be “in” on equal footing. This led to examples of the formal style of introduction (who to present to whom, hand shaking, and so on) and also examples of young people who do not use our system with each other but must have a different context because the problem of introductions has to be solved somehow.

This began to create a vague class that was something like “initial meetings,” which included examples not only of first meetings of strangers but also the beginning of any interaction, even with friends or acquaintances. Jennifer began asking all of her friends how they greeted people. One friend who works at a department store described the informal rules among the staff: You catch someone’s eye and say “Good morning”; later you may have coffee with someone, but the senior staff walk right past and never sit with subordinates at coffee. A friend who is a nurse had some especially interesting information: They say “Good morning” to each

once, at the beginning of the shift, but after that pass each other often in the corridors and either do not look at each other or else exchange exaggerated facial gestures that indicate they are harassed or busy. They seem to cooperate in *not* having to greet one another each of the dozens of times they pass during a shift. The friend also contributed a couple of other incidents: A new doctor came to a nurse at the desk and, *without introducing himself*, asked if there were any messages for him. This implied that the nurse was an object with no name that was supposed to know his name—in other words, an answering machine, not a person. Another example was her own usual introduction to a new patient, “Hi, I’m Pat, I’ll be looking after you.” One patient offered his hand to shake, and this was very awkward, as it seemed inappropriate to the relationship. This brought us back to handshaking, and many examples: Women often do not shake hands; personally, I am inconsistent and temporizing about it, and I gave several examples of how that goes; Jennifer always shakes hands and gave examples of how it is a convenient, clear beginning and end for an interaction. I began to do it more and found she was right. You have the status “noninteractants” at one point and then you are interacting; a formal greeting such as a handshake is a clear way to cross over. It is like passing through a barrier into a new region, like going through the membrane around a cell and then being inside.

So a handshake has many purposes, as a greeting, an “ice-breaker” (the membrane analogy again), and a “statement,” especially for women. We speculated about the cues that a handshake is going to be offered. Ideally both people should be ready, in order to avoid the awkwardness of having to free the right arm or to get around a desk or other obstacle. A young male student said he often shakes hands with other men, but not with women his age; the physical contact seems too intimate. There are many different variations of the standard handshake: the “black brother” handshake of celebration seen at the Olympics; squeezing your left hand with my right, seemingly more spontaneous and intimate; hugging and/or pseudo-kissing are also standard motions of greeting for those well enough acquainted.

Jennifer is originally from Scotland, where people shake hands or hug after some time apart, apparently to indicate that the relationship has not changed. In any case, the initial greeting seems to “frame” the relationship and indicate what may happen within it. Jennifer’s usual doubles match at tennis had a new fourth one day. This man was “all tennis,” with formal introductions, handshak-

ing, structured warm-up, and serious play. The other three changed their usual, more lighthearted approach and played his way until nearly the end, when they began joking and laughing as usual. At this point, we thought of the topic as “greetings” as well as “initial meetings,” although what was being discussed did not by any means fit neatly into one category or the other.

Jennifer decided to get some pilot data. She sat in the hallway of our building at the time classes changed. This gave her many opportunities to observe people passing and meeting. It was difficult to keep track of things, and what she did see seemed very ordinary at first, hardly worth the trouble. But it became clear that many people passed each other with no acknowledgment, some greeted briefly with no further interaction, some stopped to talk, and others went off for coffee together. How they initially greeted each other seemed to presignify these alternatives. What was most striking at the time, though, was the stereotypy of most of the greeting/interactions:

“How are you?”—“I’m fine, thanks.”

“How are you?”—“Just great, thanks.”

“Hi, how’s it goin’?”—“Fine, how are you?”

The rhythm, even the number of syllables, was the same in the inquiry and reply. Goffman had described boats passing and greeting with the same number of horn blasts. As it turned out, when I asked a nautical friend, this was quite inaccurate: Boats signal their intentions (to pass to port, to pass to starboard, or to back) with a fixed code of sounds, none of which is echoed by the other. But there were many other examples of “echoing the beat” or “getting into the rhythm.” Children waiting to jump into the rope turned by two others will mark the beat and enter by its rhythm. It is necessary to get the pace of a new partner in squash or tennis. When joining others talking at a party, it is similarly desirable to find the beat of their interaction before entering.

This mishmash of examples and ideas led to somewhat more general questions, such as What do people do when they plan to interact? What do they do when they do not want to interact at all? What are the parameters of greetings? What differences occur due to settings, functions, or individuals? Yet none of these possible questions was satisfactory; they seemed to dictate only surveys, specific descriptions of events with little general meaning. Throughout, however, there had loomed the idea of being nonin-

teractants (such as strangers passing or Jennifer at her friend's side) who face the issue: Shall we interact? At some point, permission to interact is given or not, and all subsequent signals are extensions of that permission—to make small talk, to discuss a particular topic, or to be intimate. And quite suddenly, in one weekly meeting, it came together:

There is a continuum called "degree of interaction," which ranges from "no interaction," that is, treating each other as objects, to "minimal interaction," which may include a smile, to "greeting," which permits even more interaction. At the other end would be the most intimate topics or kind of interaction. Thus:

Not appropriate to interact—permission refused.

:
:

Only so far as a greeting, no talk.

:
:

Will interact; how much, how intimately?

Any pair of potential interactants must find a place on this continuum, and both parties must signal and agree to this. The extreme, minimal end is very interesting: Standing in a bank line, one can choose to ignore neighbors, or to permit discussion of the frustrations of such lineups, but certainly nothing more personal. This is the problem of the three-party interaction that started this line of thinking: Do I interact with the other person or pretend we don't exist to each other? All of the strangers we pass every day (and even the nurses passing one another in the corridors) signal that we are not interactants, that we can pass by, ostensibly treating one another as objects on the path. To others, even strangers, we may give a vague smile acknowledging their existence but inviting no further response. Or we may begin conversing, perhaps even exchange names, and thus gain the footing of people-who-talk-to-each-other. Handshakes and introductions are simply distinct signals that a certain degree of interaction is permitted and accepted.

This "continuum" has several interesting qualities: The stages seem discontinuous (and "continuum" is probably a poor analogy; "choice of category" is probably more accurate). For example, there is a world of difference between passing a person by and greeting him or her. This step-function is like the membrane, a qualitative rather than quantitative leap. Also, although the signals

about the degree of intimacy permitted seem relatively obvious among persons who converse, they present an interesting problem for strangers. Because both parties have to cooperate in noninteraction, the signal system must be clear, quick, and nonverbal—in other words, a covert communication not to communicate.

With this potentially very general model for how we deal with all of the other beings we encounter on our area of this planet, Jennifer set up a pilot experiment. She arranged for strangers to meet in our lab, in front of the video cameras. Some were (1) to work alone at a task requiring their undistracted concentration. Others were (2) to work alone, but with no time pressure. A third group were (3) to work together. The first two to four seconds of the initial meeting were startlingly different. The subjects signaled their coordinated intentions about interaction with a series of actions that were virtually the same within each condition but did not overlap between conditions:

- (1) Eye contact, smile, break eye contact.
- (2) Eye contact, smile, "Hi," break eye contact.
- (3) Eye contact maintained as the pair move immediately into verbal conversation and introductions.

What logic required seemed to be so. Even strangers can signal quickly what they expect by way of interaction and thus coordinate the degree to which they interact.

If you look back at the earliest instances, all of them are somewhere on the permission-to-interact-to-this-degree continuum. You can look also at this example in two other ways, applying the "how to . . ." principles introduced earlier and, for the most part, avoiding "how not to . . .".

Of course, the pilot work was not proof; there were alternative explanations arising from the tasks given the dyads. As we worked those out for the experiment that formed Jennifer's thesis, neither of us doubted what would be found. Yet both were, as usual, more and more nervous as the harsh tests were made, and even more delighted when they were passed (Mullett, 1986). Furthermore the general model did indeed become much bigger; it includes a general theory of initial encounters, a principle of functional differentiation of the time periods in interaction, and a model of degree-of-relationship encompassing strangers to intimates.

This example is exciting not only because of the neat experiment it led to but because of its potential generality, yet it is only slight

unusual in my experience. Most students survive this apparently aimless phase and burst into an experiment at the other end. The more experienced ones hold off until the most general case is visible. This demands patience, fortitude, intelligence, and imagination, but they need only be told that this is permitted.

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