CONDUCTING MEANINGFUL EXPERIMENTS

40 Steps to Becoming a Scientist

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Introduction

The purpose of this book is to help prospective social, behavioral, and health scientists conduct meaningful research. Meaningfulness can, of course, be conceptualized in a number of ways. I happen to define a meaningful research study as one that has the potential of actually helping people and improving the human condition. Other people would define the term more broadly, such as anything that contributes to theory formation or any research capable of explaining the etiology of a scientifically significant phenomenon.

The truth of the matter is, however, that how one goes about ensuring the meaningfulness of his or her research is far more important than the way the term itself is defined. The very acts of (1) formally considering the potential meaningfulness of one’s research prior to conducting it, and (2) taking all of the necessary steps to enhance this potential, will result in better, more meaningful science irrespective of how one defines what is and is not meaningful.

There are a finite number of discrete steps, which, if conscientiously followed, will produce better, more reliable, more meaningful research regardless of whether one believes that the purpose of research in his or her discipline is (1) to improve the human condition, (2) to refine theory, or (3) to achieve any other epistemologically defensible objective. The presentation and explanation of these discrete steps correspondingly comprise the bulk of this book.
Introduction

Just as opinions differ with respect to the ultimate purpose of social, behavioral, and health research, so too do assessments regarding its cumulative accomplishments. Given my orientation toward searching for individually and/or societally useful findings, I tend to be quite disappointed with our successes so far in producing results that actually have the potential to make a difference in people’s lives. I suspect that the same is true for those who value the development of very specific social and behavioral theory, since results that add anything substantive to our understanding of the etiology of important personal, societal, or scientific outcomes also appear to be disconcertingly rare. We have few if any sociological theories, for example, that are capable of predicting the occurrence of new phenomena as Einstein’s general theory of relativity was able to do. We have very few psychological findings that are capable of making people happier or more satisfied with their lives. We have very little educational research that teachers can use to help their students learn more during the course of a school day.

Although there is no shortage of opinions, no one knows for sure why this is so. There are at least four possibilities:

1. Too much research is conducted with too little consideration of its social, professional, or scientific implications. This problem is easily ameliorated by the formal evaluation of these implications prior to conducting a study. Chapters 2 through 4 are therefore specifically dedicated to the process of formulating and evaluating important, relevant (i.e., meaningful) research questions.

2. Too much research of dubious quality is being conducted. Since no one ever purposefully sets out to conduct a poor research study, the reason so many flawed efforts find their way into the literature must be due to either a lack of sound empirical principles or a lack of understanding of those that do exist. I personally happen to know that the former is not true, since I recently spent 2 years of my life assimilating what has become a truly enormous literature devoted exclusively to the methodological aspects of conducting empirical research. After reviewing more than 5,000 articles and books, I abstracted 2,660 of these references for the resulting 800+ page volume (Bausell, 1991). Conducting Meaningful Experiments is a direct result of this effort, because I have become convinced that this vast, disparate literature can be reduced to a finite number of empirical principles that are capable of enhancing the quality of all our research efforts. The bulk of the present book, then, is devoted to the presentation of a series of generally accepted principles (or “rules”) for conducting empirical research capable of producing the types of results that the scientific community as a whole is most likely to accept. I believe that a firm grasp of these principles (especially those presented in Chapters 5 and 6), along with an a priori commitment to quality at the design stage, will greatly enhance the probability that any researcher will ultimately produce valid, methodologically sound work.

3. Too much research is carried out by people who really aren’t scientists in any true sense of the word. Becoming a scientist requires specialized training, considerable hands-on experience, and a painstaking acculturation process. To require someone to do research as a condition, say, for being allowed to teach in a university makes no more sense than to require a nonartist to periodically paint a canvas or compose a sonata. I happen to believe that a scientist is motivated almost exclusively by an internal drive, which can perhaps be facilitated, but certainly not externally instilled. The only way of knowing whether one has this type of motivation, however, is to actually start conducting research and see how one likes it. I firmly believe that anyone who truly wants to be a researcher can be a researcher. The concepts involved just aren’t that technically or conceptually difficult to master. A major purpose of this book, therefore, is to help make the conduct of one’s first research study as meaningful an experience as possible, so that the reader can evaluate whether he or she wants to forge a career in this arena.

4. Too often the ultimate objective of the research process appears to be a discrete “publishable” study rather than a substantive contribution to science or society. Research should not be conducted solely as an item-by-item addition to one’s curriculum vitae. Instead, it should be a concentrated search for something, which does not end until a discrete, recognizable discovery is made. This discovery, in turn, almost always seems to point to the need for a new search for a new piece of an ever-evolving puzzle. It is the search process itself, however, that differentiates science from all other forms of human endeavor. It is also the need to be the first person to solve the puzzle at hand that differentiates a researcher from all our other professions. Any science that is driven by these forces inevitably becomes a river of effort without end, indubitably influenced and made possible by what has gone before. In the final analysis, everything in this book is designed to facilitate this type of discovery and, ultimately, the flow of the river itself.
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It cannot be said with absolute certainty that these factors are solely responsible for our perceived slow rate of progress in the social, behavioral, and health sciences. What I can absolutely guarantee, however, is that the advice in this book can improve the quality of any beginning researcher's work if that person is willing to take it.

I am also not saying that this advice is the only path to conducting meaningful research. All I am attempting to do in this book is lend a hand to anyone who aspires to take his or her science one step further than it has been before and, in so doing, make a meaningful contribution to society.

Since there is no way of knowing who will end up reading this book, I have made as few assumptions regarding my potential audience as possible. I therefore assume no technical knowledge at all on the part of my readers, nor will I attempt to impart any. What I will do is give the reader a list of principles that, in one form or another, are absolutely essential for conducting meaningful empirical research involving human subjects.

Conducting meaningful scientific research can be an intensively rewarding experience like none other. Why this is so, I am not sure, but I think it is related to the fact that, since science really is a river of effort without end, any true contribution to it bestows a certain measure of immortality. Or perhaps it is simply that, whether we know it or not, we all aspire to do something worthwhile with our lives.

Who "I" Am

I make my living both by conducting and by helping other people conduct research (which means that I am also a research methodologist). I wouldn't consider trading my job for any other in existence. I think doing research is the most exciting, fulfilling, important career anyone can possibly have and I hope to somehow, someday be in a position to provide my children with the opportunity to engage in the same line of work (if they have any interest in doing so).

Whether I am a good scientist is for posterity to decide. I did have the good fortune to be in a position to make several very interesting discoveries very early in my career, and to this day I have never, except with the birth of my two children, experienced anything as moving or as exhilarating as having just produced a new and important piece of knowledge that no one, anywhere, except me, knew.

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I should also mention that I have also made some ridiculous blunders. I have probably made every known genre of empirical mistake at least once. I have pursued blind alleys relentlessly—once even conducting a series of 28 studies trying to find something that I later learned just plain didn't exist. None of these things, however, not the mistakes and not the ill-considered studies, ever served to dampen my enthusiasm for the scientific process. If anything I think such misadventures only tend to make the successes sweeter when they come—and they will come to those who are sufficiently determined.

I also had the very rare good fortune of having access to two very excellent mentors. They are Joseph R. Jenkins (now at the University of Washington), who was the best researcher I ever met at formulating important questions and then being able to design studies to answer them; and William B. Moody (now at the University of Delaware), who gave me the opportunity to do as much research as I could handle as long as I could convince him of its meaningfulness.

Unfortunately, mentors such as these are exceedingly rare in the social, behavioral, and health sciences. All too often students are expected to conduct their first research studies with the minimal guidance of a dissertation committee and later, as junior faculty members, to conduct their first program of research with only the memory of this minimal guidance.

I would therefore be remiss if I did not offer the following piece of advice (which I will present in the form of an empirical principle) as an extremely important strategy in the conduct of meaningful research:

Principle 1: Conduct your first research study under the tutelage of an experienced, principled mentor.

The best (and perhaps the only) way to learn how to conduct research is to actually do it. An experienced mentor who has your best interests at heart (hence the "principled" qualifier) can greatly facilitate this learning process and can probably accelerate your learning curve several years. Thus you should search for an experienced researcher with whom to collaborate on your beginning efforts. If this is not possible, then at the very least find such a person with whom you can discuss your prospective study and who is available for consultation during its conduct.
The Scientist as Interventionist

I began this chapter by defining meaningful research as research that has the potential of helping someone. The primary type of research that is capable of accomplishing such an active objective is empirical research in which the scientist personally introduces an intervention of some sort in order to evaluate its effect upon an important societal outcome. My second piece of formal advice is therefore:

\[\text{Principle 2: When conducting empirical research, always attempt to experimentally manipulate your intervention so that you can directly measure its effect(s).} \]

The reason for this is quite simple. If your ultimate goal is to help people or to improve the human condition, then you must contribute in some meaningful way to introducing a change in the way people are treated or in the way in which our institutions operate. Since you are not a mystic, you will not automatically know that these changes are "good." As a scientist, the only way to learn whether your innovation is worthwhile is to introduce it under controlled, experimental conditions and measure its hypothesized effects.

Experiments of this sort are often called intervention studies, experimental studies, or (depending upon their setting and purpose) clinical trials. Their successful conduct is predicated upon the existence of (1) well-developed theory, (2) a considerable amount of developmental work dedicated to the construction of both the interventions and the outcome measures by which these interventions are evaluated, and (3) the utilization of an agreed-upon set of procedures that will allow other scientists to place a certain amount of confidence in any results that accrue therefrom.

As such, intervention studies assume a certain degree of maturity within their parent sciences. They also tend to be more difficult to conduct than other types of research, but there is a growing consensus (especially among the nonscientific community) that it is now time for all of the sciences to begin dedicating more of their truly impressive talents to the task of discovering ways in which the environment can be altered to specifically better the human condition. Concomitantly, there is a growing reluctance on the part of funding agencies to continue supporting "preliminary" studies that never seem to quite realize this promise. There is also a growing realization that our society, beset by ever-multiplying social problems and the escalating costs of caring for a population that is rapidly growing older, no longer has pockets deep enough to finance another generation of preliminary studies.

Because of the difficulty inherent in manipulating many social, behavioral, and health-related variables, compromises must occasionally be made in the design of experiments involving them. Sometimes, for example, we must conduct our experiments under relatively contrived laboratory situations, rather than the real-world crucible of everyday practice. Sometimes we must shorten the experimental interval more than we would like. Sometimes we must settle for accessible subject populations rather than the ones in which we are truly interested. Sometimes we must even alter our outcome variables a bit, making them somewhat less veridical in order to demonstrate change in a reasonable time period.

Suppose, for example, that a preventive researcher believed that he or she could develop a genre of positive health messages that would be more effective in eliciting salutary lifestyle practices than the current negatively oriented ones offered in the media. Because it is highly unlikely that our researcher would have the resources to mount a major community-based study to assess the effectiveness of this new approach, it might first be necessary to randomly assign one group of volunteers to be exposed to a battery of the positive messages and compare their intent to adopt sensible preventive behaviors with a randomly assigned group exposed to comparable negative messages.

Certainly such a study, if successful, would need to be replicated, extended, and made more veridical (e.g., by measuring actual behavior 6 months or so after exposure to the two types of health messages), but studies such as this, which do introduce an experimental intervention and evaluate its effect, have more potential for helping us solve the types of problems we are now facing in society than any other genre of research. Furthermore, with a little creativity (and, alas, perhaps a few initial compromises), most of the important variables in the social, behavioral, and health sciences can be studied under experimental conditions.

Analyzing the results of questionnaires or interviews, observing and recording the way people interact with one another in naturalistic settings, or soliciting opinions and attitudes toward this or that topic are
appealing research options, but they won’t in and of themselves help us educate our children, improve the quality of our lives, or increase our productivity. Research such as this may suggest directions for doing so and, hence, can be useful developmental tools in refining our experimental interventions, but in the final analysis if changes in the way we are doing things (or in the results we are getting) are our objective, it is change that must be implemented and evaluated under the most controlled conditions possible. By convention, this is the type of evidence demanded by the scientific community simply because this is the type of evidence that produces the most reliable causal inferences available to us.

There is another, less pragmatic reason for conducting experiments, however, and that is the fact that there is nothing more exhilarating for a scientist than to manipulate something in a way that it has never been manipulated before and to be the first person in history to observe the resulting effects. There is also nothing more satisfying for someone with such a bent than to take his or her science one step further than it has ever been before and, not coincidentally, to be so registered in its annals. This book is therefore designed to show its readers how to take this step.

Other Types of Scientific Studies

By focusing on the conduct of experiments, I do not mean to denigrate other types of scientific endeavors. Some of society’s most important variables are basically nonmanipulable in nature, and I certainly do not suggest that such concerns not be the subject of empirical scrutiny.

I happen to believe, however, that the most direct means of accomplishing what the social, behavioral, and health sciences should be all about is through the conduct of experiments. I also believe that, with a little creativity, most variables can be experimentally manipulated in one form or another.

I certainly do not believe that experimentation is the only way to generate important new knowledge. Many scientists are almost exclusively interested in studying existing relationships among variables. They are, in other words, more interested in documenting what is than what could be. Studies of this sort (which are variously called descriptive, correlational, naturalistic, or observational research) have a venerable place in the history of science, and there is always a need for good research, regardless of genre.

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I do happen to believe that the conduct of experimental research is an excellent way of embarking upon a meaningful scientific career, however, and that there is certainly no better way to learn the research process thoroughly. I think it is fair to say, in fact, that a thorough knowledge of the concepts involved in conducting an experiment is a prerequisite to conducting any type of empirical research (and therefore prerequisite to embarking upon any type of meaningful scientific career).

Over the years I have also become convinced that there is no other type of research that has quite the same potential for producing meaningful results in the way that I have defined them. Again, however, regardless of the type of research a scientist conducts, the basic principles that govern experimental research apply just as forcibly to nonexperimental designs. Thus if you do decide to limit your research efforts to handing out questionnaires, to reviewing existing records, or to interviewing people, your primary purpose as a scientist will still be to (1) understand the nature of phenomena and (2) predict the exact conditions under which they occur. As with evaluating the meaningfulness of a research study, the procedural steps taken in designing and conducting a study are more important than the actual approach ultimately chosen to achieve this quintessential scientific objective. You can, in other words, produce scientifically meaningful results regardless of the type of research you do as long as you do it well. All I am attempting to do in this little book, therefore, is show prospective scientists who truly want (or perhaps even need) to try to make a meaningful contribution to their science and their society one way of doing so. It is really for this purpose, and this purpose alone, that the 38 rules that follow are tendered.

Suggested Readings

Although I’ve tried to make each chapter as self-contained as possible, there are numerous resources for gaining a more in-depth understanding of the topics contained therein. The suggested readings that follow constitute only a smattering of the many, many excellent books and articles available to beginning researchers. For a more extensive (but far from exhaustive) list, see:

This is an annotated guide to 2,660 research design, measurement, and statistical references deemed by its author to be most usable to practicing researchers.

Two books that I think give the clearest description of the excitement inherent in the scientific enterprise are:

Although this book does not deal with experimental research as we define it in the social, behavioral, and health sciences, it provides a wonderful insight into the excitement of discovery and the excitement of the chase for being the first to discover something. The "something" in this case happened to be the structure of DNA; and I think that, after reading this book, experienced researchers in our fields cannot help but feel a little envious that we haven't even gotten around to defining what our big quests should be, or how we will recognize them if we find them.

Lucy is also about discovery, and while its "chase scenes" may not be quite as exciting as those in The Double Helix, it does provide a powerful lesson in the effort and rewards of making sense out of our research findings. Much of this book is given over to demonstrating how powerful and useful theories can be generated by relatively sparse data. By implication, I think it also shows us that without this time-consuming process, our research data is likely to remain nothing more than numbers stored in a computer (or bones stored in a museum).

What You Need (and Don't Need) to Be a Scientist

Let us begin by briefly discussing some of the characteristics that one needs (and doesn't need) to be an effective scientist.

What You Absolutely Need

If we assume that you aspire to a lifestyle somewhere above the poverty line, the first thing you will need is a doctorate. There are exceptions to this rule, but they grow more and more rare, hence the following piece of career counseling:

Principle 3: Obtain an appropriate doctorate, preferably a Ph.D., as quickly as possible.

The Ph.D. itself is completely irrelevant to the conduct of empirical research. Yours will be a truly exceptional program if it contains as many as three courses that have any direct applicability to the type of research you wind up doing.
Even though a Ph.D. may be nothing more than a union card, however, it is extremely difficult to find employment doing research without one. (You will need this union card to be fairly compensated, and without it, you will not be afforded the same opportunities to conduct meaningful studies.) I would further advise you to be a full-time doctoral student if at all possible, since this will afford you the best opportunity of working directly with a practicing researcher (Principle 1).

Now that I have done my counseling duties, let us concentrate upon those behaviors that are directly relevant to the actual conduct of research itself. I will begin with the most important scientific principle contained in this book:

\[ \text{} \]

\textit{Principle 4: Do not contemplate conducting research if you are not prepared to be absolutely, uncompromisingly, un-fashionably honest.} \[ \text{} \]

This is absolutely the most important attribute for a scientist to have. Scientific progress itself is directly dependent upon the integrity of its practitioners. I am not only talking about the avoidance of complete fabrications here, such as painting skin grafts on mice or reporting the results of identical twin studies for children that were never born. I am also referring to the avoidance of small untruths or sins of omission, such as not mentioning seemingly minor things that may have gone wrong during the course of an experiment, in order to enhance its subsequent chances of getting published.

A researcher needs to have the courage to tell the entire truth even if it means effectively throwing away a considerable amount of personal work, delaying graduation, or not obtaining a much-coveted promotion. For those who do not possess this particular characteristic, I personally beg of them to seek another career. Go into public relations or corporate communications. Make twice the salary and drive a Lexus, but please do not go into research. If you do, you will hinder other people’s progress and you will ultimately be very dissatisfied with your choice of career.

\textit{Principle 5: Do not contemplate conducting research if you are not prepared to work very hard.} \[ \text{} \]

Conducting research is an extremely time-consuming enterprise yet, paradoxically, few social, behavioral, or health scientists can pursue it full-time (which might be another reason for our mediocre track record). The pursuit of scientific knowledge is also a process that has no end point, so if you plan to seriously pursue a scientific career, you must be willing to devote many, many long and late hours to your research.

All the successful scientists I have known have had a very high energy level. Whether this is under an individual’s control depends upon a number of factors, but I have found the capacity to do research to be similar to the capacity to engage in physical exercise: The more you do, the more you can do, and the more you are willing to do.

Because of this need for continual, concentrated effort, research is, in many ways, a young person’s profession. It is far easier to devote the time, energy, and passion it demands prior to taking on, say, familial or administrative responsibilities.

There are exceptions to every generalization of course, but the simple truth is: The more focused and even fanatical a researcher is, the better and more impressive his or her ultimate accomplishments are likely to be. (I personally happen to know that some of the stereotypes in this regard are true, once having helped a MacArthur fellow with a methodological problem who had no residence at all outside of his laboratory.)

The only real way I know around this omnipresent time constraint is to learn to work very fast and to concentrate very hard. I have personally learned to do this over the years, yet every step of the research process always takes more time than I think it will. I still sometimes reanalyze my data again and again—exploring alternative hypotheses, statistically controlling different variables, and looking for implausible interactive or confounding factors that could potentially influence my conclusions. I still sometimes ponder the meaning of my findings for long hours and often go back to the library for one last time-consuming search through a related literature for insights into the problem at hand. Even though I remain able to devote this extra time, however, and even though there is no question that I am technologically far more skilled now, I don’t think my current work is as exciting or as important as that which I produced.
as a young man. Perhaps the reason for this is that I no longer bring a young person's passion to my research. Perhaps my research now has too many other competitors. Perhaps this perception isn't even true, so I will abstain from elevating the importance of youth too highly in the scientific process, although I nevertheless advise all aspiring researchers to stay "forever young" as long as they can.

Unfortunately, even hard work is not enough in and of itself. The conduct of meaningful research involves a great deal of preparation, the first step of which is implied by our sixth principle:

Principle 6: Do not contemplate conducting research until you have mastered your general field or discipline.

Research is not performed in a vacuum. Knowing how to conduct research is not even enough. Researchers must understand their fields as well as the basic paradigms operating therein. Otherwise, I think they have practically no chance at all of making (or even recognizing) a significant contribution thereto.

Thus, if you are not thoroughly familiar with a field, you should not attempt to conduct research in it. If there isn't any discipline with which you are thoroughly familiar in the academic sense, then you shouldn't conduct research at all. (Unfortunately, I can't give any particularly innovative advice about how to learn a discipline, other than the tried-and-true steps of enrolling in a good graduate program, taking good courses, and studying good textbooks.)

It is true that some people do seem to have an "inborn feel" for what is and is not important in a discipline, but it has been my experience that these individuals also tend to be both knowledgeable about the basic content of their fields and extremely familiar with its research literature. It is my opinion, in fact, that the greatest part of this feel is comprised of very careful attention to two distinct behaviors that will be discussed in detail shortly: reviewing the literature and critically addressing the "So what?" question.

Before considering these two behaviors, however, I would like to offer a complementary stricture to the previous principle that is primarily applicable to applied research (which encompasses a large proportion of the empirical work done in the social, behavioral, and health sciences):

Principle 7: Do not contemplate conducting applied research (i.e., research whose primary purpose is to change the way people practice their professions) if you are not a practitioner in the area, or if you do not have a co-investigator who is. (Always remember, however, that when you are conducting research, you are first and foremost a researcher.)

To put it into more concrete terms, one should not conduct research that attempts to improve teaching practices within the schooling paradigm if one is not thoroughly familiar with both teaching and schools. One shouldn't attempt to come up with improved therapeutic practices if one hasn't conducted therapy or isn't thoroughly familiar with the therapeutic process. This isn't to say that you can't conduct learning or therapeutic research if you haven't had direct experience in one of the two areas, but if the goal of your research is to influence practice, it is absolutely imperative that you have enough direct, hands-on experience to be able to judge what is practical and what is not. Otherwise, it is just too easy to overlook something that is capable of completely invalidating the potential applicability of whatever treatment or intervention you happen to be investigating. Specific clinical practices, regardless of the discipline, are affected by so many constraints (e.g., costs, staffing/time considerations, and political/ethical issues) that any study that fails to appreciate, address, or at the very least recognize all of them is usually a complete waste of time. It is therefore essential for investigators who want to conduct applied research to either do so in an area in which they have had sufficient applied experience or collaborate with someone who both has this type of experience and has had some research experience. (This latter condition is necessary since an awareness of the built-in constraints of the research process itself is as important as an awareness of those inherent within the clinical process.)

There is another side to this coin, however, and that is reflected by the fact that sometimes one's clinical role/training may seem to conflict with what one must do as a researcher. Clinicians in the helping professions have a very understandable (and laudatory) tendency to want to help people. Experimental researchers, on the other hand, usually hope that half of their subjects will be helped more than the other half. What does
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The answer is that when you are doing research, you must act primarily as a researcher and see your experimental protocol through to its completion. Obviously, if anyone in your study is in need of medical or psychological help, you should immediately refer that subject somewhere that he or she can get the help needed. (Emergency contingencies such as this should always be built into one’s protocol anyway.) For nonemergency situations, however, you must remember that as a researcher, your primary duty to your subjects (and your profession) is to run the cleanest possible study, not to deliver care.

By the same token, as a researcher you have just as much obligation to protect your subjects as any clinician ever has for his or her patients. You must, in other words:

1. Never physically endanger your research subjects in any way.
2. Never subject them to any sort of emotional or psychological distress.
3. Never embarrass them in any way (if this is part of the experimental intervention, choose another line of research).
4. Always protect their dignity and freedom of choice (including the freedom to leave your study at any time of their own choosing, regardless of reason).
5. Always treat them as you would wish to be treated (or as you would wish your children or your aging parents to be treated).

What You Should Have (but May Not Absolutely Need) to Be a Researcher

There are a number of attributes, which, while important, probably do not deserve the status of principles. Among these are:

1. You should be driven. This goes a bit further than Principle 5. To call someone “driven” is not particularly complementary in this society, but all those I have ever known who have been extremely successful in their areas of endeavor have possessed this characteristic. I used to be a serious weight lifter, for example, and all the very best lifters that I knew (whether Olympic lifters, bodybuilders, or power lifters) pursued their training with a single-minded intensity that subordinated everything else in their lives. I’m sure that this struck their non-weight-lifting acquaintances as patently absurd, but there is really no other way to excel at something, science included. The correlates of this particular characteristic may be too high a price for many people to pay, but this is a personal choice.

Intensity of purpose may be sufficient anyway. By this I mean a mind-set that ensures that the study under consideration is always being thought about, always being mentally played over and over, and always being examined for possible flaws and extensions. If enough intensity can be brought to the task at hand, to ensure that nothing that needs to be done on this study is postponed or delayed, then I think it should be possible to fashion a rewarding study-by-study career without sacrificing any other truly important aspects of life (except possibly either some leisure time or some sleep).

2. You should have a certain amount of innate curiosity. Some of the motivation to be hardworking and driven comes from the external rewards available to researchers: Scientists, especially later in their careers, probably like money and professional advancement as well as does anyone else. In general, however, science is not as good a choice as investment banking, medicine, or plumbing for someone for whom the material things in life are of utmost importance.

What scientists usually treasure more is their status among their colleagues and the opportunity to receive credit for their accomplishments. In the long run, however, it is probably even more important for a scientist to have a basic need to know the answer to the questions at hand and a need to be the first person in the history of the human race to solve a particular puzzle or problem. It is this need, drive, or compulsion that truly motivates scientists, especially young ones, to make the sacrifices necessary to excel at their task. The presence of such drives, in fact, makes doing what has to be done really no sacrifice at all, especially after the first true success is achieved. One of the primary services that I am attempting to offer by writing this book, therefore, is simply to increase the speed with which this initial success comes.

3. You should be skeptical. It is important for a researcher not to take things at face value, because a large part of science entails questioning obvious truths that other people take for granted. Good researchers see
the need to confirm or disconfirm generalizations, everyday common-sense statements, and other bromides that their peers accept without question.

Good scientists also tend to have specially honed antennae for such lead-ins as "experts say" or "research says" or "the literature says" or "everybody knows." One of my most influential experiences as a beginning graduate student came when the instructor, a leading Piaget scholar of the time, asked each student in a doctoral seminar why Piagetian theory was important to his or her particular area of concentration. Mine happened to be mathematics education at the time, so I used a time-honored opening for seasoned students who know the importance of giving instructors what they want to hear by saying something like: "Well, obviously Piaget is relevant to mathematics education because . . . " at which point the researcher interrupted me and said, "It's not obvious to me."

I was so shocked by this that I simply admitted ignorance to the answer and found the experience so refreshing that I have freely admitted ignorance or skepticism in similar situations ever since. In other words, I gave up intellectual bullshit and, equally important, stopped accepting it from other people. This, I think, is a vital attribute for a researcher to develop and perhaps is really submerged under the fourth principle (i.e., being "absolutely, uncompromisingly, unfashionably honest").

4. You should be methodical. Conducting a research study is comprised of a number of often tedious, discrete behaviors, any one of which—if improperly performed—can torpedo the entire effort. What this means, then, is that a researcher needs to have the patience to check and recheck each step performed, as well as to rethink every decision made. This can be especially difficult when one possesses a great deal of curiosity and is driven to work fast, but please be assured of its importance by one who has been reminded of the veracity of this particular lesson the hard way (and probably still hasn't completely mastered it).

5. You should be open-minded. I would like to emphasize the "should" here, because being open-minded isn't a particularly pervasive scientific virtue. Researchers tend to inexorably belong to a given school of thought (or paradigm), which in turn tends to color and mold both the questions they ask and the interpretations they bring to the data they use to answer them. In many ways this is beneficial, because paradigms or theories can be quite useful in generating interesting, meaningful hypotheses. It can be equally disadvantageous, however, because these same constructions can blind researchers to alternative interpretations of their data. (Researchers subscribing to two opposing theories often can and do view the same results as categorically confirming their worldviews.)

I know of no cure for this state of affairs other than to aspire to a certain degree of humility (which definitely doesn't tend to be a common scientific virtue). This in turn will allow you to at least consider the possibility that you can be wrong when you attempt to interpret the meaning of implications of your findings. If the history of science tells us anything, it is that if you become a scientist, you will ultimately be proven wrong in how you view things—if your research is important enough to motivate anyone to bother to disconfirm it. In science it is not imitation that is the sincerest form of flattery, but rather generating enough interest in your work to have it extended, improved upon, and possibly even attacked.

There are other helpful scientific attributes, but the ones I have mentioned here seem to me to be the most important. Before going on to other things, however, I would like to mention a couple that I really did not forget to list:

1. You don't need to have any real mathematical ability. A lot of people avoid conducting research because they never really understood trigonometry and were afraid to even tackle calculus. With the advent of extremely easy-to-use computerized statistical packages, however, such mathematical skills are no longer necessary in any of the sciences involving human subjects. It does help to be able to estimate grossly what one's results should look like, but even this isn't necessary if you are critical (and methodical) enough to question the computer output and to check and recheck each step in the research process.

2. You don't have to be a genius. Geniuses are rare. Being gifted isn't even as big an advantage in conducting an empirical research study as you would think. Being methodical, determined, and willing to work hard more than makes up for any lack of giftedness. This book is, after all, titled "Conducting Meaningful Experiments," not "How to Formulate an Alternative to the Theory of Relativity." To accomplish the latter, or to stimulate a paradigmatic shift, you do need to be a genius (and you will definitely need someone a lot smarter than I to advise you).
Suggested Readings

There are a number of books written on what it takes to be a scientist and what good scientists should and should not do. Here I will list only one example of what I consider to make the most interesting reading from each of the topics covered in Chapter I.


This is a brief (14 pp.) report, prepared as an introduction to the scientific process, aimed at students contemplating careers as scientists. It is clearly written and emphasizes, among other things, the subjective nature of the scientific enterprise as well as its more important mores (e.g., honesty).


This well-written book covers dozens of cases of scientific fraud, both classic and modern, and contains just about everything anyone would ever need to know about the topic. Its 412 references also constitute the most thorough bibliography of which I am aware.


This is a charming little book, written by an eminent British scientist, that covers not only “advice” to young scientists but also some of the things that are good and bad about the research process.


This is an iconoclastic look at scientists (who the author sees as “probably the most passionate of professionals”) from one psychologist’s viewpoint. There is no way of knowing whether the author is right in his conclusions, but the book does provide interesting food for thought for anyone specifically interested in this particular topic.

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The First Step

As Yogi Berra should have said, “You can’t begin until you begin.” Had he said this, and had he been a researcher, it would have been translatable to: “The only way to become a scientist is to begin to conduct research.” This chapter is dedicated to the preliminary groundwork needed for this beginning.

Principle 8: Know the relevant research literature thoroughly.

There is no question that this is the single rule most often violated by beginning researchers. It is very typical for a student or recent doctorate, faced with the need or desire to conduct a research study, to begin the research process in the following manner:

1. A general idea concerning the research question/topic is formulated by brainstorming, with or without some colleagues.
2. A cursory literature review (often consisting of a computerized search) is then undertaken to make sure that the study hasn’t
already been “done.” (Occasionally this second step will precede the first, which is certainly preferable.)

3. The research question is narrowed down into a testable hypothesis, and the study is begun. (Occasionally, this third step will precede the second, which is certainly not preferable.)

Although I have personally been guilty of performing a study in something approaching this sequence, I have never conducted any meaningful work in this way and I seriously doubt whether anyone else has either. It is my firm opinion, in fact, that a thorough knowledge of the relevant research literature is the most important precursor to formulating an important, meaningful hypothesis.

The literature review, which is the process by which this prerequisite knowledge is achieved, is such an integral step in the research process that most serious, experienced researchers probably don’t even consider it a step at all. Instead, they constantly keep abreast of the research taking place in their areas by reading the primary journals and attending research conferences. They are often even aware of important results before they are published via personal communication with similarly minded colleagues.

Without beating this point to death, then, allow me to list a number of reasons why you should acquire a thorough knowledge of the research literature in your area prior to conducting a study.

1. If you have a study in mind, you will find out whether it has already been performed.

2. Regardless of whether you already have a study in mind, you will obtain ideas for hypotheses in need of testing. These ideas can come from:
   a. theoretical and conceptual articles or books that predict what types of interventions are most likely to work in a given field, as well as ideas concerning the determinants of important outcome variables therein,
   b. actual suggestions regarding needed research tendered by other investigators in the discussion sections of their research articles,
   c. an already completed study that you think needs to be redone (e.g., because of a methodological glitch or because it was non-experimental in nature to begin with), or
   d. a good study that needs to be extended (e.g., by improving upon its intervention, substituting a different outcome variable, or even extrapolating it to a different field involving a completely different population).

3. You will see how other, more experienced researchers measured and/or manipulated the variables in which you are interested. In relation to this, you may obtain some very useful procedural ideas for conducting the studies (e.g., types and numbers of subjects to use).

4. Once you do begin to formulate a testable hypothesis, you may be able to ascertain how likely you are to get the results you hope to obtain, which is important, given the difficulty often encountered in publishing null results.

Since most beginning researchers have not been continually monitoring the research going on in their area for any length of time, Principle 8 must be accomplished by a discrete set of behaviors called “the literature review.” Because of the importance of this process, I will offer the following relatively negative piece of advice prior to discussing some of the more positive behaviors involved therein:

\[ \text{\textbf{Principle 9:} When conducting a literature review, do not rely exclusively on computerized literature searches, on abstracting services, on the literature in a single discipline, or on an arbitrarily defined time period.} \]

Although they produce woefully incomplete results, computerized literature search and/or abstracting services do have considerable potential as starting points. Some of the databases most commonly used by social and behavior scientists therefore follow:

- Applied Social Sciences Index and Abstracts
- Arts and Humanities Citation Index
- Biological Abstracts
- Books in Print
- British Books in Print
- British Education Index
- Canadian Education Index
Let us assume that you have a few general textbooks touching the topic at hand, a couple of slightly more focused journal articles, and a preliminary computerized literature review from one or more of the relevant available databases.

Given such a beginning point, I would suggest the following steps:

1. Decide exactly what it is you are searching for and how widely you are willing to stray. If your goal is to find specific studies involving one or more specific variables, then articulate what these are at the onset.

2. Solicit some help from someone who has either conducted research in the area or who teaches therein. Things that might be reasonable to ask of such an individual are:
   a. to be allowed to photocopy any bibliographies available.
   b. the location of any narrative literature reviews or meta-analyses of the area in which you are interested (such documents will allow you to "hit the ground running" by typically supplying more than 100 references, plus at least one person's opinion regarding what they all "mean"),
   c. what journals are the most likely to contain relevant research articles,
   d. the name of someone else that you might approach, and
   e. the names of any seminal studies and/or books on the topic.

In return, you might promise to share a copy of any interesting sources that you uncover. Doctoral dissertations (which are abstracted by topic area in *Dissertations International*) constitute an especially good source of reference lists (actually their literature reviews are often their strongest component).

3. Check the reference lists of the sources with which you started with any obtained via the second step. Look up any sources whose titles sound promising. Skim them for relevance (you can often ascertain this via the study's abstract and/or procedures section) and read the promising ones carefully. Next, go through these new reference lists in the same way. Keep track of any relevant articles obtained by filing them alphabetically in folders and by entering them in a word processing file (see Principle 10) accompanied by your own abstracts.
4. Once you have a feel for the journals that tend to publish the type of articles you are looking for, carefully go through the year-end title indices (which are usually presented in the last issue of each volume), beginning with the most recently bound volume and preceding backward in time. Continue this until it’s obvious that you aren’t going to find anything else of interest. When you have completed this process, repeat Step 3, above, with any new articles, being on the lookout for additional relevant journals. Also be on the lookout for published literature reviews and meta-analyses since they are especially valuable sources of individual research studies. (A few journals are completely given over to these summations, such as Review of Educational Research, Psychological Bulletin, and the Annual Review journals that cover a number of discrete disciplines.) Edited books are especially good sources of chapters devoted to theoretical and conceptual overviews based upon extensive literature reviews surrounding a particular topic. There is also at least one major publisher (Greenwood Press located in Westport, Connecticut) that specializes in bibliographies, both of information sources and of discrete, often quite esoteric research literatures.

5. Solicit the help of one of your university’s reference librarians. Often this won’t be particularly fruitful, but they might be able to steer you toward a reference book or a helpful bibliography with which you are unfamiliar. (Actually, an interesting use of an hour or so of leisure time is to systematically peruse the volumes housed in the reference section of a major research library. The wealth of information contained there is quite amazing.)

6. Consult the Social Science Citation Index (or its physical science counterpart). Although recommended only for the conscientious (who also have good eyesight), these volumes allow the user to find sources that have cited an earlier study which you have ascertained to be especially relevant to the purposes at hand, which of course gives the new study a high probability of being relevant as well. (Most libraries now offer these services on-line.)

7. Check your expanded reference list against your original computerized literature search. For those relevant studies contained in the former but not the latter, find out if they are contained in the databases you searched. If they are, check the keywords under which they are indexed and redo your search, using those words. If the additional references are not listed in the searched databases, find out where they are listed (see the sources above) and search them.

8. Although this step is usually not necessary, leaders in the field in which you are interested could be contacted and asked to suggest additions to your current bibliography. This could consist of simply mailing your bibliography to promising names (journal articles typically provide their authors’ addresses), along with a stamped, self-addressed return envelope, asking for additions thereto. (It will be helpful if you provide a sentence or two describing the exact purpose of your bibliography.) For every five you send out you might receive one back with a couple of new references.

9. Once you have completed what is really a major undertaking, continually update it by monitoring the primary journals (and conference proceedings) devoted to the line of research in which you are interested.

   Obviously, this combination of manual and electronic processes is extremely time-consuming, and the nonautomated steps are judged by many people to be quite old-fashioned. Personally, I think that there are no real options open to the truly serious researcher. Experienced researchers have always accumulated their research literature manually by prospectively monitoring relevant journals over time and keeping in contact with other researchers in their fields, so what I am suggesting is not as reactionary as it may seem. Furthermore, once completed, such a reference list can be updated with a minimum of effort and used for a variety of purposes (including Principle 10, below).

   ▼

Principle 10: Learn to use a major word-processing program and utilize it to produce an annotated bibliography based upon your literature review.

   ▼

Just about every step of the research process can be greatly facilitated via the use of a personal computer, especially if you learn to compose directly on the keyboard rather than on paper. (Writing an annotated bibliography is an excellent way to do this, since polished prose is not a particularly high priority.) Thus, if you don’t know how to use a widely
employed word-processing program such as WordPerfect, take the time to learn how to do so prior to beginning your literature review. The time benefits are truly staggering. As an extreme example, my annotated bibliography mentioned earlier contained more than 4,000 names, which had to be alphabetized for the book’s author index. My personal computer did this for me in about 30 seconds. It is difficult to imagine how long this would have taken if done manually, but I estimate that it would have necessitated a stack of index cards almost 4 feet high.

The best way to learn the use of a word-processing program is to begin with a specific task (such as the compilation of an annotated bibliography) and secure the help of a hands-on tutor to help you get started. Although I own no stock in the company, I would suggest WordPerfect because it is so widely used. (This makes it easier to secure the help of a tutor and it also helps ensure that what you produce is more likely to be transferable to a professional secretary’s work station.) Whatever software package you select, you will probably be better off learning the latest version, even though most companies seem to come out with a complete update every 2 weeks to protect their cash flow.

If the services of a tutor are not available (and I would definitely suggest it would be cost-effective to pay one out-of-pocket if a volunteer cannot be had), then the next best strategy is to obtain an easy-to-follow book on the use of the word-processing program you select. (Most company software manuals are written either by someone for whom English is a third language or by certifiable sadists.) Personally, I have always found books published by the Que Corporation to be quite clearly written.

Regardless of the method by which you produce your annotated bibliography, I strongly suggest you take one further step with this document:

\[\text{\textbf{Principle 11:} Take the time to write a review article and conduct a meta-analysis based upon the references you have amassed.}\]

This may seem like an imposing task, but it is absolutely essential that you thoroughly assimilate the information you have collected. There is no better way to do this than by writing an article based upon it. (It is also important to assimilate this literature while the articles that you have read are still fresh in your mind and while it is still feasible to alter your design, based upon the strategies previous researchers in the area have used.)

You may not wind up publishing the resulting paper, but you should definitely proceed on the assumption that you will. What you should try to do, then, is make sense out of the overall gestalt that reading the articles in question has produced. Do not, in other words, write what amounts to a prose connection for a list of references such as: "Jones (1986), Smith (1990), and Wiley (1980) have conducted research involving manipulation of xxx, while Johnson (1987), Nash (1988), and Wrangly et al. (1992) have studied correlates thereof."

Instead, I suggest that you:

1. Describe the important studies briefly.
2. Don’t be afraid to criticize them if you think they contain flaws.
3. Don’t be afraid to go out on a limb in order to summarize whatever conclusions you think can be safely made based upon them, and most important.
4. Always keep the study you are planning in mind and review all previous research within that framework, for it is at this point that you are most likely to come up with a meaningful hypothesis to test.

Finally, once you are sure that you have located all the relevant studies in your area of interest, I would suggest that you take the time to compute an average effect size for the relationship or difference that you are interested in. (For simple experiments, an effect size is nothing more than the difference between the experimental and control group means divided by the control group’s standard deviation.) Called meta-analysis, this is an especially valuable technique for estimating how many subjects you should employ in your actual study (Principle 28). It is also not a particularly difficult task once the relevant literature has been located, read, and abstracted. If a meta-analysis has already been conducted in the area, then I would suggest that you update it by computing an average effect size for the studies that have been completed since its publication. (See the Suggested Readings section for references dealing with the conduct of a meta-analysis.)
CONDUCTING MEANINGFUL EXPERIMENTS

Conceptualizing Your Intervention and Outcome Variables

In truth, the primary purpose of all of the steps suggested in this chapter is to facilitate the formulation of a meaningful hypothesis. It is hoped you will therefore have read and abstracted each article that you have found within this framework. If you have, you should now be in a position to select what may be the single most important component of such a hypothesis: the experimental outcome that your study is designed to influence. The next logical step in our quest is therefore to:

\[ \text{Principle 12: Select an important, meaningful outcome variable that directly reflects a discrete societal or individual good.} \]

Although the latter part of this dictum may seem a bit restrictive, I think it is quite reasonable, given the purposes of the social, behavioral, and health sciences. (I would personally go even further in this regard and define a meaningful outcome as one that is directly related to someone's quality of life.) This relatively narrow definition certainly reduces the range of reasonable outcome variables available to researchers; but in many ways this is more of an advantage than a disadvantage, since there is almost an infinite number of outcome variables theoretically available for study in any given area. If all of these variables received equal emphasis, scientific progress as we know it would be almost impossible because we measure increases in empirical and theoretical knowledge primarily in terms of what we know about outcome variables and the interventions that influence them. If every researcher were free to choose any variable that struck his or her fancy, then our efforts would be so scattered that we might never understand the etiology of any single variable. (Actually, researchers dealing with human behavior do sometimes appear to behave in this way to a certain extent, which may be yet another reason why these disciplines have not made as much progress as they should have.)

Thus in educational research, a primary outcome variable could be the amount learned to the extent that the purpose of schooling is defined as imparting knowledge deemed necessary to function successfully in society. Any alternative outcome that a researcher wished to employ would need to bear a direct link to learning or to some other primary outcome variable. Hence research that studies eye movements of young children learning to read would qualify only if a link were first established between eye movement and, say, reading achievement. A researcher could, however, reasonably employ decreased disruptive classroom behavior, increased voluntary time on task, or decreased absences from school as outcome variables since these variables have been empirically linked to student achievement. Similarly, changes in teacher behaviors designed to affect such a variable would qualify, but attitudes toward a particular subject matter would not be a reasonable candidate unless someone had first demonstrated a link between attitudes and learning.

In health-related research, reasonable outcome variables include such things as mortality and morbidity rates, increased recovery time, increased quality of life, decreased costs of care (assuming finite resources), and so forth. Any other outcome variables chosen must relate in some way to direct indicators such as these; thus a study designed to increase nurses' hand-washing behaviors after seeing patients would be a reasonable variable if hand washing has been shown to decrease hospital-based infections among patients.

Every social, behavioral, and health science, in fact, has its own catalog of meaningful variables that directly or indirectly affect the quality of our lives: activities of daily living, voting records, creative behavior, compliance with therapeutic regimens, risk-taking behavior, aggressive/disruptive behavior, job performance, drug and alcohol consumption, anxiety, depression, subjective well-being, mental health, marital distress, job satisfaction, absenteeism, employee turnover, social behavior, spatial ability, critical thinking, recidivism—the list, which is shared across disciplines, is almost endless. Again, the only criterion that I suggest is that the variable you choose to study be directly related to some unequivocal individual or societal good.

Some researchers consider even this criterion unduly restrictive. They would consider the demonstration of an empirical link between an intermediary variable (e.g., nurses' hand-washing behavior) and a direct individual or societal outcome (e.g., the development of staph infections) as unnecessary as long as the prevalent theory or paradigm under which the profession operates predicts such a relationship. This is a matter of individual choice, but I suggest that it is always a good idea to test such assumptions whenever possible. The history of science is replete with examples of entire generations of careers being devoted to the assiduous study of such dead-end variables as head size. (Note that
at this point we are talking about global constructs, not the specific manner in which we will measure that construct. There are, for example, literally thousands of measures of student learning and hundreds of specific ways to measure most of the other social, behavioral, and health-related outcomes mentioned above. Criteria for the selection of actual measurement instruments will be discussed later.

The first step in selecting a meaningful outcome, then, is to make sure that you can answer one of the following questions with an unequivocal "yes." If you cannot, I suggest that you go back to the drawing board.

1. Does the outcome variable constitute a generally accepted personal or societal good (i.e., does it relate to health, happiness, or productivity)?
2. Is the outcome variable causally linked to a generally accepted personal or societal good? (For example, consumption of dietary fat would be a reasonable dependent variable to attempt to manipulate since it is related to cardiovascular disease—which in turn results in death, pain, and decreased productivity.)

If we assume a positive answer to one of these two questions, the next step in ensuring a meaningful hypothesis test naturally involves the intervention that is designed to influence this outcome:

Principle 13: Select a theoretically justifiable independent variable that (1) is capable of being experimentally manipulated and (2) is clearly capable of influencing your outcome variable.

Note that we are still talking in very general terms. If you are planning to do an experimental study, there is no way that you will be able to actually design your intervention at this point. Successful interventions do not spring fully developed from the researcher's head like Zeus's children. Full-blown, successful interventions never emerge from a brainstorming session. They are always suggested by theory, previous research, or extensive clinical experience. Also, they always require extensive developmental effort to work out procedural bugs and to fine-tune their various components. What I am referring to here, then, is a globally conceptualized independent variable that will later be refined into an intervention. (An independent variable is defined as a variable capable of "causing" changes in an outcome variable or, at the very least, one that is known to precede it in time.) Examples of independent variables in social, behavioral, and health research might be peer counseling, class size, patient education, biofeedback, and psychotherapy.

Regardless of the general form your global independent variable takes at this point, I suggest that you pay very close attention to the two qualifiers in Principle 13. It does little good to choose an independent variable, for example, that is not directly manipulable. An experimental study by definition requires a manipulation of some sort, and this manipulation by definition becomes the intervention. Thus, even though an excellent theoretical and empirical rationale exists for the relationship between smoking and lung cancer, the former would not be a reasonable choice for an intervention because it is not directly manipulable. (Given that links have been established between smoking and a plethora of important health outcomes, however, smoking cessation can and does constitute a very reasonable outcome variable in its own right.)

It is of equal importance that you should never select an independent variable for which there is no empirical or theoretical rationale that links it to your outcome variable. The chances that any subsequent intervention developed from it will be successful are so remote that the research community probably would not believe your results, even if you were able to demonstrate an effect.

It is important to note that everything said to this point also applies to nonexperimental research. Even if there are no plans to manipulate the independent variable, such as in the conduct of a correlational study that simply involves assessing the relationship between two variables, Principle 13 still constitutes pretty good advice. If there is no theoretical reason why your chosen variables should correlate with one another, for example, the chances are that either they will not or no one will believe your results if they do. Further, if your independent variable can never be either directly or indirectly manipulated under any imaginable conditions, then the societal relevance of any documented relationships involving it is likely to be quite restricted. (Since it is always possible that such relationships may have scientific or theoretical importance, however, I would not award Principle 13 the status of a "rule" for nonexperimental research.)
Returning to the experimental paradigm, however, and assuming that at this point you at least have an embryonic intervention in mind, I would suggest that you evaluate it by addressing the following questions:

1. Can you visualize implementing your potential intervention given the resources (e.g., types of subjects or clinical facilities) available to you? If you cannot, you should go back to the drawing board.

2. Is there some theoretical or empirical reason to believe that your potential intervention will indeed affect your outcome? Although I would absolutely require an affirmative answer to this question as a prerequisite to conducting a meaningful study, I am quite liberal regarding what a “theoretical or empirical reason” entails. Possibilities include:
   a. a formal theory that actually predicts a relationship between the variables involved,
   b. the successful use of the planned intervention involving a different outcome or a different population,
   c. the successful use of a similar intervention involving either the same or different outcomes,
   d. the results of a large-scale correlational study that demonstrated a noncausal relationship between the variable involved, or even
   e. pilot work conducted by the investigator for the very purpose of suggesting the efficacy of the intervention.

An affirmative answer to both of these questions indicates that you are now ready to formally state your research intentions, which happens to constitute the subject of the next chapter.

Suggested Readings

General References

Although general reference books have limited utility with respect to reviewing any discrete literature, there are occasions when certain factual information is required as background for a research study or when hints regarding the location of additional research studies outside

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the researcher’s discipline may be needed. Three sources (contained in the reference sections of practically all research libraries) that may prove to be useful starting points for these purposes are:


This huge text contains general works, bibliographies, texts, and all types of reference books for just about every major academic discipline.


This is a periodically updated directory of directories that consists of an annotated guide to more than 10,000 business and industrial directories, professional and scientific rosters, directory databases, and other lists/guides.


Organized by subject, this 4,000+ page trilogy (the final volume is given over to on-line serials and an index to publications of international organizations) provides addresses, editors, telephone numbers, and circulation figures for tens of thousands of periodicals (including scholarly journals). This volume can prove useful in locating leading journals in related fields that may publish research in the topic of interest.

The Literature Review

Two useful texts specifically given over to the conduct of literature reviews are:


This brief book provides seven chapters dealing with every aspect of performing a literature review, including one titled “Methods for Locating Studies.”


Arguing that most literature reviews are too subjective to be scientifically sound, the authors provide guidelines for conducting a methodologically sound one. They also include strategies for organizing a
comprehensive reviewing strategy and provide a checklist for evaluating same.

Two of many articles that offer useful guidelines for conducting a literature review (both of which start with the explicitly stated assumption that individuals who conduct literature reviews should be just as scientifically rigorous as those who conduct experiments) are:


Meta-Analysis

There are a number of excellent texts on meta-analytic techniques. The following is only a sampling of those available:


This is the classic, original text in the area and should definitely be read by anyone with a serious interest in meta-analysis. It is especially relevant to meta-analyses conducted on experimental research and, although the field has moved on considerably since the publication of this book, is still quite useful for this purpose.


This book should definitely be read by anyone serious in mastering the technique. At present it is undoubtedly the most complete teaching resource available on meta-analysis, although it tends to emphasize the meta-analysis of nonexperimental research.


This may be somewhat easier reading for beginners than Glass et al. and advocates a slightly different strategy.

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For anyone interested in the potential of meta-analysis for resolving some of the problems inherent in the social, behavioral, and health sciences, the following sources might prove interesting:


Formulating a Meaningful Hypothesis

It is now time to talk about the step which everything to this point has been designed to facilitate. Let us assume that the literature review has been completed (although it will always need to be updated) and the other 12 principles have been followed as well. You should now be in a position to make a formal statement of your research intentions based upon (1) an appreciation of what has been done in the area, (2) what needs to be done, and equally important, (3) what can be done.

This rather formidable task boils down to a series of discrete skills, which includes:

1. Being able to ascertain what can and cannot be accomplished within the context of a single empirical research study.
2. Being able to visualize the results, both with respect to the general form they will take and with respect to the different outcomes obtainable. (It is helpful, although not always essential, to be able to estimate which of these different outcomes are most likely to occur.)
3. Being able to judge the implications capable of being derived from the results, including the uses to which they can be put and the types of questions that they will in turn generate.

There are, however, three steps that precede even the application of these skills, and they are:

1. determining what one's "real" or "root" purpose for conducting the study at hand is,
2. determining what one's "scientific" purpose is for conducting the study, and
3. formulating an explicit, operational statement of what the question at hand is.

Although these preliminary steps are probably not necessary for experienced investigators, they deserve the status of hard-and-fast rules for anyone about to conduct his or her first empirical study. The first two are assumed under Principle 14:

Principle 14: Sit down and honestly assess what your true, personal motives are for conducting the study.

Here I am referring to your "real" motivations for conducting the particular study under consideration. I am not referring to anything that the average researcher would ever admit to in person. Instead, I am suggesting an introspective examination of motives. Examples (and it is certainly possible to have multiple agenda) might be:

1. to change a distasteful (i.e., to the researcher) professional practice,
2. to learn everything there is to know about a particular variable,
3. to discredit a theory or line of thought considered to be either wrong or archaic,
4. to test a theory to see if it is valid,
5. to lend credence to the formulation of the investigator's own theory,
6. to further one's career by conducting a seminal research study,
7. to further one's career by obtaining a "research publication,"
8. to obtain one's doctorate in order to go into a professional practice (and possibly never have to conduct research again),
9. to become famous,
10. to further scientific progress in one's discipline, or
11. simply to discover something of intellectual interest.

I am sure there are circumstances in which all of these motives are justifiable, although some may not be particularly realistic. If Motivation #9 involves getting on the cover of People magazine, for example, you should probably either choose another line of work or use celebrities as research subjects. Motive #2 is equally unrealistic and will probably result in a diffuse fishing expedition that produces nothing of value. Motive #8 is quite realistic but can be accomplished without bothering with the 40 rules presented here.

All the remaining motives can be realistic, but I passionately beseech you to make sure that yours are at least compatible with the conduct of meaningful research. This, it will be remembered, translates to research that is capable of making an actual scientific contribution (which is defined as determining the causes, effects, or etiology of a phenomenon), of providing a mechanism to improve the likelihood of obtaining better clinical outcomes, or of actually helping people and furthering the human condition.

Regardless of what one's motives are for conducting research, I think it is essential for investigators to at least be aware of what they are and to use them to evaluate explicitly how likely the studies they are planning are to satisfy them. Perhaps the most integral step in this evaluative process is the composition of a formal operational statement of the purpose of the study within a testable, doable format. These operational articulations of research purposes are conventionally called hypotheses.

Once committed to paper, a formal hypothesis is capable of forcing a researcher to confront his or her aspirations within the cold reality of what the study it represents is capable of producing, which is one of the prime rationales for Principle 15:

Principle 15: Translate your proposed study into one or more formal, written hypotheses.
This is actually a pretty tall order. If you are to accomplish this, a good hypothesis should have the following characteristics:

1. A hypothesis should consist of a statement that can be judged as either true or false. This true or false judgment, in turn, is made on the basis of a statistical analysis performed upon the data that the study itself is designed to collect. The specific analytic procedure need not be specified, but by writing a statement that can be judged as true or false (which is exactly analogous to asking a question whose answer is either “yes” or “no”), you will automatically eschew such phrases as “the best way” or “determine the meaning of” or words such as “good” or “why.”

If, after formulating a hypothesis, you are unsure whether it really will be subject to a “true” or “false” decision following statistical analysis, then you may want to turn it into a research question. If the question can be answered either affirmatively or negatively—there are no maybes in hypothesis testing—then you have a testable hypothesis. If the question cannot be answered with a simple “yes” or “no,” then your hypothesis is probably not testable unless it can be broken into multiple questions, which can be answered in this way. (Hence it is usually a good idea to write separate hypotheses when more than one outcome variable is employed and when more than one contrast between experimental interventions is planned.) If this still does not work, then you should go back to the drawing board.

2. The groups and/or variables should be specified and, where appropriate, independent versus dependent statuses should be delineated. This will be a simple task for the type of research we will be discussing in this book, because the independent variable will always be an experimental intervention that you will manipulate, and the dependent variable will be the outcome that this intervention is designed to influence.

3. When an intervention is involved, the hypothesis should be stated in terms of assessing differences (or changes) with respect to the effects of this intervention. The manner in which it was decided who would receive said intervention should also be stated, along with the type of comparison that will be used to evaluate these differences. When controlling variables (i.e., attributes that are measured before the study starts and used later to statistically “subtract out” any preexisting differences among the study’s groups) or multiple administrations of the outcome variable are involved (e.g., the use of follow-up measurements), some mention should be made of these as well. In empirical research involving people, an intervention is tested either by assigning subjects to groups or by administering it to everyone in the study. When different groups are used, the identity of these groups should be described, along with a statement as to whether subjects were randomly assigned (i.e., where each subject was given an equal chance of receiving or not receiving the intervention in question) or there was nonrandom assignment (where subjects were selected in some other way). (If everyone in the study receives the intervention, then the hypothesis should state this as well.)

4. Some general mention should be made of the types of subjects employed. Since the primary purpose of a hypothesis is to communicate the essence of the study being conducted, the reader obviously needs to know whether, say, hospitalized cardiovascular patients or fifth-grade elementary school students are being employed.

There are almost as many ways to write a hypothesis as there are researchers to test it. If a hypothesis contains each of the above elements, however, then it will serve its primary purpose, which is to communicate the operational objective of the study. An example of such a hypothesis might be:

There is a difference in fifth-grade mathematics achievement scores between students randomly assigned to receive 45 minutes of mathematics instruction per day and those assigned to receive 60 minutes of comparable instruction.

Here the sample is described (fifth-grade students), the groups/interventions delineated (i.e., students receiving longer versus shorter instructional periods), along with the manner in which it was decided who would receive which (i.e., random assignment); and the criterion (i.e., the outcome variable) by which they will be compared (a mathematics achievement test) is described. The hypothesis is also obviously capable of being accepted or rejected since it can be easily reformulated as a question that can be answered either positively or negatively:

Is there a difference in fifth-grade mathematics achievement scores between students receiving 45 minutes of mathematics instruction per day and those receiving 60 minutes of comparable instruction?
If random assignment was not used to compare the two types of classes, the hypothesis should indicate this by omitting the "randomly assigned" portion of the hypothesis and possibly substituting something like "intact classrooms." If the students' prior mathematical abilities were to be taken into account, this fact too should be noted by tacking on "when prior mathematical ability is statistically controlled." If retrospective data were collected on a large number of classrooms, with respect to both their students' test scores and the length of their math periods, then the resulting hypothesis might read as follows:

There is a relationship between mean fifth-grade mathematics classroom achievement scores and length of instructional periods after prior mathematics achievement has been statistically controlled.

This particular hypothesis would tell the reader that (1) a correlational study was being performed (i.e., "relationship between"); (2) the data points would be classrooms rather than individual students (i.e., "mean . . . classroom . . . scores"); (3) the length of the mathematics instructional periods was allowed to vary "naturally" (since no mention was made of any discrete time intervals); and (4) previous achievement was used as a statistical control ("after prior . . . has been statistically controlled").

Everything said here applies equally well to any social-, behavioral-, or health-related investigations, regardless of the method by which they are conducted or their subject matters. The following statement, for example, employs all of the recommended components for a good hypothesis and communicates a great deal about its parent study (even if it does become a bit wordy):

A group counseling procedure involving empowerment techniques will result in reduced high-risk sexual behavior among methadone maintenance African-American women, as compared to a randomly assigned attention-control group, both immediately following therapy and 6 months thereafter, after prior sexual behavior has been statistically controlled.

Writing good, succinct hypotheses takes practice, but they do not have to be "perfect" or aesthetically pleasing to communicate what the study is all about as long as they (1) can be judged as true or false, (2) clearly specify the identity of all groups and/or variables employed, (3) identify the general type of design employed (e.g., whether the study in question employed an intervention to which subjects were randomly assigned, an intervention to which subjects were not randomly assigned, or was purely correlational in nature), and (4) give some indication of the types of subjects employed.

I think it is worth reiterating, however, that none of these recommendations is carved in stone. There is no "rule" that even says you have to write a hypothesis prior to conducting a study. Many experienced researchers do not. There is even no "rule" that says a hypothesis or a research question has to be written in an accept/reject—yes/no format, but this is a tried-and-tested convention that is probably worth retaining.

There is another overriding reason why it behooves a beginning researcher to adopt the above four-step strategy, however, and this is because the main beneficiary of a well-written hypothesis is the researcher himself or herself. This is because the best way to judge the potential meaningfulness of a research study is to judge the meaningfulness of the hypothesis itself prior to conducting the study in question.

Another important benefit of a clearly stated hypothesis is that it allows the researcher to ascertain what can and cannot be accomplished within the context of a single empirical research study. Thus, returning to the sample hypotheses above, serious contemplation of this relatively restrictive declarative sentence should point out to the researcher exactly what he/she stands to learn from the studies they represent. Assigning "true" or "false" labels to these statements will not tell us what the best way to teach mathematics or prevent AIDS is. Such labels will not tell us what the optimum length of a fifth-grade mathematics class is, nor will they tell us anything about the best type of group counseling. They will tell us nothing about other types of instruction or therapeutic modalities and they will probably not tell us much about other types of samples.

The statements' very restrictiveness will, however, enable their authors to visualize the different outcomes obtainable: That is, the 60-minute classes will result in either greater, or less, or equal mathematics achievement than the 45-minute classes; and the group therapy intervention will result in either positive, or negative, or no changes in sexual behavior when compared to the placebo group. Visualizing the restrictive nature of such outcomes can greatly facilitate the a priori evaluation of the meaningfulness of one's research hypotheses, and it is to this crucial process that the next chapter is dedicated.
Suggested Reading

For a readable description of the origins of some of the conventions regarding hypothesis testing, the types of errors one can make therein, and the controversy of whether a null hypothesis is ever accepted, I would recommend:


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**Evaluating the Meaningfulness of the Research Hypothesis**

It is now time to attempt one of the most important, difficult, and often neglected steps in the research process: evaluating the meaningfulness of one’s hypothesis(es) for the express purpose of deciding whether the parent study is worth conducting. Formulating a testable hypothesis comprised of an important outcome variable and an intervention that is theoretically capable of manipulating this outcome is a very important step. There is no guarantee, however, that the actual testing of such a hypothesis will produce results capable of:

1. generating a high quality causal inference,
2. providing a mechanism for improving the quality of someone’s life, and/or
3. satisfying the researcher’s personal objectives for conducting the study in the first place.

To meet all of these criteria, it is necessary to conduct a formal evaluation of the entire study as it has been formulated to this point. Hence the following principle:
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However, the empirical process consists of a number of discrete, relatively small steps that ultimately must be supplemented by further studies and replicated by other researchers.

**Tenet 2:** It is far better to provide a definitive answer to a specific, narrow question than it is to answer a diffuse, broad question equivocally. A corollary to this is that it is better to err on the side of making the study’s focus too narrow than too wide. Beginning researchers have a decided tendency to try to supply all the answers to all of the questions they can think of. The truth of the matter is, however, that whenever a researcher supplies a single definitive answer to even a small question, he or she will have been wildly successful.

**Visualizing Results**

Sometimes one of the hardest things for beginning researchers to accept is the fact that regardless of how great their aspirations for a study are, the ultimate outcome is going to be nothing but a few numbers accompanied by a true/false decision. An experimental study always involves a comparison of some sort of group means. The importance of the difference between these means by necessity becomes synonymous with the importance of the study itself. In the final analysis, then, something is going to be judged as greater than, less than, or the same as something else, and that is basically all that is going to accrue. The researcher’s job at the hypothesis formulation stage is therefore to decide: (a) if these “somethings” are important enough to study, and (b) if it is truly important that one is greater than, less than, or the same as the other(s).

Even if the answer to both of these questions is “yes,” the researcher should still decide if all of the possible outcomes of the proposed study are equally important. (It is usually the case, for example, that a non-significant difference resulting from the evaluation of an innovative new treatment, which no one has ever heard of, will not generate nearly as much interest as a statistically significant difference supporting its efficacy.) If one of the possible outcomes is considerably more important (or more interesting) than the others, the researcher must therefore further decide: (c) if its occurrence is sufficiently likely to justify the study.

Obviously, if the answer to either (a) or (b) is “no,” then the study should not be conducted. It is often a little more difficult to evaluate the third question, but an informed guess, based upon a careful analysis of
the literature, can be quite helpful here. If one of the possible outcomes is considerably less interesting than the others (e.g., if it is unlikely that a leading journal would publish a study reporting such a finding) and if it is considerably more likely to occur than the others, then conducting the study is a risk that may not be worth taking unless the stakes are quite high (i.e., the less likely results would be very important if they should accrue).

When the literature is not helpful in assessing the probability of a particular outcome (e.g., when new treatments and/or variables are being studied), the most likely outcome by far for any given study is no significant difference between groups. Unfortunately it is almost always the case that this happens to also be the least interesting possible outcome as far as the rest of the scientific community is concerned. (Chapter 6 is specifically devoted to ways of decreasing the likelihood of your obtaining nonsignificant differences between your groups.)

Judging Implications of Results

Judging the implications of one’s results is synonymous with answering the infamous “So what?” question. It is perhaps the most difficult and subjective step in the entire evaluative process, but it is such an important skill that its mastery should be pursued tenaciously. Some strategies for facilitating this task follow:

1. Try to assess honestly (Principle 14) what your true personal objectives are for the study. Again, I am not referring here to the words that appear in the research hypothesis; rather, I am suggesting an honest attempt to come to grips with one’s internal motivations. I consider this step important because it may reveal an objective that simply cannot be accomplished. Even if one’s true goal is nothing more ambitious than just to publish a research study, an understanding of this can itself be helpful since some types of hypotheses lend themselves more readily to publication than others.

2. Rethink each element of the hypothesis in view of the answer to 1. In other words, are the choices made regarding outcomes, interventions, and/or the target sample appropriate? (For example, will other researchers be interested in the experimental intervention you have chosen or in the measuring instruments you have selected to compare them upon? Are the types of subjects you have chosen to employ the most interesting available?)

3. Try to visualize how each of the possible study outcomes fits into the previous research literature. Said another way, how likely is it that the results of this study would be published by the types of journals that appear in your reference list? Unpublished research is of little benefit to anyone.

4. In relation to 3, what would the next study be based upon each of these possible outcomes? Later I will suggest that you write follow-up hypotheses to fit these results. Studies that do not generate meaningful questions for future research may not be worth conducting.

5. Actually ask the “So what?” question and attempt to answer it honestly with respect to previous research, relevant theories, and potential applications. If the study is designed to test a theory, then this test should be conducted in terms of identifying the etiology of a specific phenomenon. A good theory should predict that a particular phenomenon will occur under certain specific conditions, so the study in question should be designed to ascertain whether this prediction is correct or incorrect. (Be prepared for the fact, however, that most people will still believe the theory, regardless of your results.) If the theory is not specific enough to derive such hypotheses, as most are not in the social, behavioral, and health sciences, then it is probably not worth testing. Similarly, if the study is designed to change a professional practice, its results should be capable of providing evidence that the practice in question is either harmful or inferior to a proposed alternative or, at the very least, the equivalent of a much cheaper/less invasive procedure. (Again, be prepared for the fact that the practice in question will probably linger on until other scientists replicate your study or until some non-empirical impediment is raised to it.) If the study is designed to improve the human condition somehow (e.g., by helping people live longer, be happier, be healthier, or be more productive), then its results should be capable of providing guidance toward the accomplishment of same. As always, this guidance may very well be ignored, but your job as a scientist is to point the way toward a better world, even if you can’t single-handedly change the present one. (If the study has no implications at all for improving the human condition, then you might want to consider whether it is worth conducting at all.)
6. Pose as many alternative hypotheses for the study's potential outcomes as possible. These alternative hypotheses should be both methodologically (more will be said about these later) and logically generated. Always ascribe to "Occam's razor," which suggests that complex explanations should never be substituted for simpler ones unless the latter can be categorically shown to be inferior. Also, be aware of the major paradigm (see Thomas Kuhn's The Structure of Scientific Revolutions, University of Chicago Press, 1962) in which you are operating and make sure that it is consonant with your hypothesis. It is also often a good idea to at least consider the possibility that this same paradigm may be blinding you from choosing a potentially even more effective intervention.

7. Prepare a formal defense of 5 and 6, assuming a hostile audience. A good way to do this is to assume that you are going to have to present your study at a press conference composed of skeptical journalists who are largely ignorant of your discipline. This will prepare you for the next step, which involves facing the most hostile audience of all.

Soliciting the Help of Colleagues

Only after all of the above steps have been conscientiously followed do I recommend that you get outside help in evaluating your hypothesis. (An exception might be if you have a close friend who is conversant with the area in question.) It can be very helpful to have a sounding board upon which to try out your research ideas, but I suggest that you do your homework first and attempt to line up professionals who have conducted research in the area of interest or who are thoroughly familiar with the research literature(s) and the issues involved.

If possible, actually arrange to present your proposed study to a group of colleagues willing to take the time to critique its importance. This provides an excellent opportunity to obtain independent opinions as well as valuable procedural/methodological suggestions. Many academic settings provide a mechanism for such forums in the form of brown-bag colloquia or journal clubs. (Doctoral students have a built-in mechanism of this sort in the form of their dissertation committee, although I personally suggest a dry run with independent parties prior to this particular experience.)

Regardless of who you line up to hear your proposal, suggest that your audience be critical from the onset. You should probably come prepared to argue for the meaningfulness of your hypotheses (and you should come prepared to be thick-skinned), but most of all view for this process as an opportunity to improve your study (in other words, prepare to be flexible).

If, after this experience, you remain convinced of your study's worth, then you should probably go ahead and conduct it, even if your colleagues turn out to be less than enthusiastic. If you are unconvinced following all of this, however, I would suggest that you go back to the drawing board.

Limitations of Professional Feedback

The above suggestions for soliciting outside help in evaluating the meaningfulness of one's research hypotheses really only apply to beginning researchers who are in the process of learning how to answer the "So what?" question for themselves. There are many drawbacks to this practice, not the least of which is the difficulty of getting honest, unemotional feedback from people who have their own agenda to fulfill. The most common failing in this regard is of people not wanting to appear overly critical and therefore being less than honest in their assessments. Negative opinions too can be wrong (one relatively common academic trait, in fact, involves always being aggressively negative), so the best you can hope for in soliciting this type of help is that someone will bring up a point (or will suggest a procedural improvement) that you have not previously thought of.

Personally, I think I will always remember my late-night completion of the data analysis for a study I had just concluded, in which elementary class size was experimentally manipulated under controlled conditions for the first time. In addition to achieving statistical significance in all the predicted ways, the results formed an almost perfect logarithmic curve, which compelled me to try to find someone with whom to share this exciting piece of information. This someone happened to be the first person I ran into in the corridor, who also happened to be a methodologically sophisticated individual with little appreciation for schooling research. His response to my breathless description of what I considered to be a major breakthrough was a bored "So what? Schools don't have enough money to reduce their class sizes so nobody is going to be able to use your results."

I was shocked. To that point there had been literally hundreds of class size studies, but no one had ever bothered to study the phenomenon in
a laboratory situation, where variables such as teacher and student differences, instructional time, and exactly what was taught during this time could be rigorously controlled. As a result, the overall judgment from this literature was equivocal, with the best guess among the research community being that class size either did not result in improved learning or, if it did, that the effect was trivial in size. (Public school teachers, of course, always knew that class size was an extremely important determinant of how much children learned in school.)

What I had set out to do, then, was demonstrate that smaller class sizes could result in increased learning, when all relevant extraneous variables were controlled, and this I had accomplished admirably. Without my belaboring the point, this particular study would certainly appear to have met most of the above criteria for meaningfulness, so why did my colleague come up with such a diametrically opposing judgment?

I've always found ascribing motivations to people a most tenuous enterprise, but there are several possibilities—all of which are relevant to the issue of soliciting outside, a priori judgments regarding the meaningfulness of a research hypothesis. In the first place, of course, he was judging the study on one and only one criterion, so it is important to at least recognize the different perspectives by which your study can be judged. It is even more important that you should explicitly recognize the criteria you have used to judge the meaningfulness of your own study. (You should always be aware of the possibility that you have made an inappropriate selection in this regard.)

Another possibility that must also always be considered, when professionals render opinions regarding work other than their own, is the presence of professional jealousy, insecurity, or simple pique at the presumptuousness (or threat) of someone else claiming to have just conducted (or is planning to conduct) a seminal study. These factors are things that all researchers must contend with at some point, since, if nothing else, their completed work will be subjected to professional judgment when it is submitted for publication. The fact that unbiased, honest judgments are sometimes difficult to obtain, however, does not mean that such judgments are not worth pursuing. On the contrary, they are so valuable that it behooves you to make the extra effort needed to obtain them. This can often be done via the following simple steps:

a. assume (or at least affect, if it doesn't come naturally) a healthy dose of humility when dealing with one's colleagues.

b. assure them that you truly do want a frank assessment of your work.

c. accept criticism graciously and gratefully (even if you consider it moronic), and

d. take the time to supply unbiased, honest (but diplomatic) feedback yourself when requested.

Subjectivity and Meaningfulness

Perhaps the greatest inherent limitation in all of the strategies in this chapter resides in the ultimate subjectivity of the concept of meaningfulness. In the final analysis, what is meaningful to one person may be completely trivial to another.

The ultimate arbiter of meaningfulness is what stands the test of time and what does not. Some studies contribute to science; the vast majority do not. All I am trying to do here is enhance the probability that your study will make such a contribution. Barring this, my second objective is to enhance the probability that you will conduct a study that you are personally proud of, which in the long run may be of equal importance.

To return to my personal experiences, over the years I have not changed my opinion about the meaningfulness of my class size study (or of a number of other unmentioned schooling studies I conducted around the same time). I think of this work as an artist must think of one of his or her favorite paintings. I will always remember its conduct and the experience of analyzing the resulting data and learning something that, for one brief shining moment, no one else in the world had ever known. This was a decidedly aesthetic experience that I have known only two other times during the course of a career that has spanned many more than 100 studies.

I mention this because I think it is important to realize early in one's career that there are definite limits to the external rewards that a researcher can reap from a single study (or even a program of research). Thus, while my class size study was cited a number of times, that was about all the external reinforcement I received from it. It did not even come close to closing the book on this line of research. It took Gene Glass and Mary Smith's seminal meta-analysis of the class size literature to do that, at least to the extent that things are ever laid to rest in science. There remains a surprising number of educational specialists out there who still think class size does not influence student learning, which is not atypical, as illustrated by the following quote, attributed to James
Clark Maxwell: "There are two theories of the nature of light, the corpuscle theory and the wave theory; we used to believe in the corpuscle theory; now we believe in the wave theory because all who believed in the corpuscle theory have died."

As a beginning researcher, you should at least be aware of the fact that your study (even if it is important and well designed) may lie hidden in the journal in which you publish it. Someone someday may come along and "discover" it, popularize it, or extend it, but even here the rewards are transitory. I have had research presented at news conferences and even summarized in those ubiquitous graphs in *USA Today*, but all of this tends to die down in a day or two and then it is over.

What is not transitory, however, is the aesthetic experience that accrues from completing and publishing a high-quality, meaningful piece of work that at least has the potential of leading to a set of important conclusions that cannot be ignored. What is also not transitory is the excitement of the search for the answer to an important question or the race to find the missing piece to a particularly intriguing puzzle. The fact that a successful conclusion to such a quest will only point the way to the next piece of the puzzle only increases the excitement of the overall enterprise. In the final analysis I think Jonas Salk summed up the scientific reward system better than anyone else ever did when he said that "The reward for good work is the opportunity to do more."

**Suggested Readings**

Undoubtedly the most influential article ever written on this topic remains:


In addition to exploring the question of why some fields progress so much more rapidly than others, this article takes on the task of judging the meaningfulness of research hypotheses. Using fields such as molecular biology and high-energy physics as case studies, the author argues that it is the systematic application of inductive (or strong) inference dating back to Francis Bacon that distinguishes these fields. In his words: "Strong inference consists of applying the following steps to every problem in science, formally and explicitly and regularly: (1) Devising alternative hypotheses; (2) Devising a crucial experiment (or several of them), with alternative possible outcomes, each of which will, as nearly as possible, exclude one or more of the hypotheses; (3) Carrying out the experi-

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... so as to get a clean result; (4) Recycling the procedure making subhypotheses or sequential hypotheses to refine the possibilities that remain." In this vein, the author suggests that the essence of scientific thought resides in two questions asked of anyone (including oneself) who either puts forth a theory ("But sir, what experiment could disprove your hypothesis?") or describes an experimental study ("But sir, what hypothesis does your experiment disprove?").

The next citation, mentioned in the text, should be required reading for all scientists. It is most famous for its emphasis upon the importance of paradigms, but its importance is far more reaching than that: