Discussions about Research: *The Saturday Seminars*[1]

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VOCABULARY FOR DISCUSSIONS ABOUT RESEARCH
Discussion Paper #1

Philosophy. Intellectually, *philosophy* refers to the orderly (or disorderly) ideas and beliefs which are accepted as basic to a person's thinking. As such, one's philosophy is foundational to the way a person thinks and especially to the assumptions one makes while reasoning about parts (or the whole) of the universe of ideas.

The very idea of *research* itself is based in philosophical beliefs about truth, the value of inquiry, the importance of human uses of knowledge, and other convictions that derive from social and religious assumptions about human purpose and responsibility. **Harmonized philosophy** suggests a set of ideas and beliefs that is consistent (non-contradictory), usually as a result of studied and carefully disciplined reasoning.

Theory. In sharp contrast with *hunch* and *guess*, ***theory*** refers to a harmonized body of thought and ideas that is based on responsible evidence or substantial experience. For example, Darwin's theory of the origins of species is not simply Darwin's personal hunch. It is "bigger" than that, and theory is not refuted by some other viewpoint based on guesswork. Most of intellectual theory today is grounded in empirical evidence, in contrast with spiritism or random speculation.

Science. *Science* denotes that realm of thought and ideas in which greater confidence is placed in *empirically* confirmed theory than is placed in theory that is vested in guesswork, speculation, opinions, and imagination. In matters of empirical science, theory guides scientific inquiry. But it can also be said that theory is built upon scientific evidence. Thus science is a sort of circular concept, originating and concluding in bodies of theory. Thus any responsible research study must always look back and look forward. What has already been learned about a matter serves as a platform as the researcher reaches forward to interpret new findings.

Research. *Research* is the technical term for systematic and disciplined **inquiry**. The term is usually qualified and delimited by an accompanying term denoting the domain in which the inquiry is conducted. This qualifying term usually points to the logical or methodological procedures appropriate for the research. For example, *metallurgical research* and *biological research* use substantially different procedures, although both are clearly realms of science and are grounded in essentially similar philosophical

[1] These six discussion papers were written to assist the doctoral study group at Trinity Evangelical Divinity School toward their quest for a more substantial understanding of research design and planning. The intent was to encourage important discussion within and among the international community represented in the Educational Studies and the Intercultural Studies groups during 2009 and 2010.

foundations. Consider the domains of science that are reflected by academic categories: **natural science, physical science, social science,** and **behavioral science.** Sub-divisions, overlapping, and emergent specialties further split these categories and continue to proliferate beyond reason. Computer science, environmental science, management science, genealogical science, medical science, and more recently brain science and sleep science are recognized as categories of scientific inquiry. Certain of these permutations arise out of the invention of new technologies, as in **computer science** and **robotics**; others have come from the inquiry styles developed within various academic fields, as in **historiography** and **linguistics.**

Two major forms of research are most often contrasted. Since pedestrian views of "science" are accompanied by white-coated humans with thick glasses whose hands hold little glass test-tubes, many people assume that science always means *experiment.* As silly as this may seem, it accounts for the fact that many students approach research training with the assumption that the most important sort of research project is some sort of experiment. Anything less is somehow not worthy of being called *research.*

Since experiments are commonly visualized as athletic contests—winners and losers, superior and inferior, success and failure, they also bring along assumptions about *scoring systems*. Thus the first form of research is the **quantitative** study.

In more recent years, often dated from the rise of descriptive techniques in anthropology (Margaret Mead's contributions, for example), the dominance of quantitative research has given way to a recognition of the importance of verbal description. This second form of research is commonly called *descriptive research* or **qualitative research.**

In the foundational concepts of science, these two research forms, quantitative and qualitative, need each other. Without careful quantitative testing of hypotheses, theory is limited. The expansion of knowledge is jeopardized. But where do worthy and workable hypotheses come from? Today's view of science acknowledges the role of qualitative studies in terms of their exploration and verbal description, within the safeguards and conceptual frames of inquiry common to all science. Thus the preparatory role of qualitative research is worthy of its own place in the realm of scientific inquiry. Consider the role of exploration in modern understandings of geography, geology, climatology, astronomy, meteorology, physics, and oceanography. Science is not purely a matter of measuring and counting things! But once theory takes shape, statistical mathematics becomes an important tool.

NOTE: In many scholarly fields and in reference especially to newly emergent conceptual frameworks, there is little or no significant empirical research. Thus experimental studies are virtually impossible when there is only a weak theoretical base and relatively few clearly defined variables. In such fields, and especially in reference to untested practices or unproven assertions, the first line of fresh inquiry is necessarily dedicated to the expansion of qualitative understanding. For example, due to the lack of well-formed theory, the fields of *religious education* and *missiology* have found that moving forward has required significant investment in qualitative research.

Two more modes of research. Many sub-divisions of scientific inquiry are represented by their own sorts of specialized research. It is not essential to deal with each. However, this discourse is not complete without attention to two other modes of research that deserve special attention: **historical research** and **evaluative research**. The first of these, *historical research*, is the oldest known entry in the production, analysis, and preservation of human knowledge. Although it arises from the human need to be remembered, it nonetheless has a valid claim on its commitment to truth as a precursor of empirical

science. The simple command: "Go look it up" describes the child's earliest encounter with the skills of historical research. *Scholarly history has moved far beyond that, but it still suffers from the overgeneralization that research is something you do in a library.*

Evaluative research **is a hybrid**. It has arisen from necessity rather than from the motive of science as inquiry. Its origin is the school. Its purpose is getting data to support the go/no-go decisions of the academic marketplace and the similar quest for grounded judgments about worth of human performance. Evaluative research is guided by rubrics of truth-seeking (testing and reliability of measurement and accounting) very much in keeping with standards and procedures used in *quantitative* research. And because of its roots in formative inquiry, it draws its bias toward hypothesis-testing from the realm of *qualitative* research as well.

ORGANIZING A RESEARCH PROPOSAL
Discussion Paper #2

Every field of academic inquiry develops its own unique formats and procedures. Educational research borrows heavily from other fields, especially from its "near relatives" in the social sciences. Nevertheless, a careful observer will note many peculiarities in any given outline or plan for a research proposal.

The Five Fundamentals of a Research Proposal

SECTION 1: Selecting the Problem

The first task in any research project is to define, explain, and illustrate the decisions that have been made in selecting the research problem. The **research problem** is the domain and situation that motivates the researcher's interest and commitment to inquire. In this spirit, Section 1 of the proposal should be written as a blend of information that is known already and the suggestive clues about what needs to be better understood. This first section of the proposal should effectively engage the reader's mind and imagination as the explanation unfolds revealing that there is to be a limited focus on but one situation, chosen for good reason**.**

Section 1 lays out the foundation for understanding the research problem. *Problem* is used in the same sense as *topic*. The research problem is the general social or psychological area in which the concern lies. It is an area, or arena, within which many problems may emerge. It is the corral full of horses in which a horse fancier might choose to cozy up to one particular horse and dedicate every effort to learning all that can be known about this particular one—all the while bearing in mind that this one horse is related to the larger collection of horses.

The second layer of emphasis is often called the **research concern**. The emphasis in this matter is more particular. This concern is for *the* horse. It exhibits characteristics and a set of problems that is perhaps less a matter for all horses than it is the specific reason for the researcher's interest in horses. The research concern is a particular issue or situation for which the possibility of a clearer understanding promises to open up important possibilities. Assuming that the *research problem* is influenza, the *research concern* is possibly the virus or perhaps the transmission of this virus within a specific population.

The third part of this opening section of the proposal deals with the researcher's decision that it is fitting and timely to make a very specific attempt to answer certain important questions in regard to the research concern that is identified.

The importance of these **research questions** (RQ) must be carefully revealed. Research questions cannot be simple hunches. The RQ must be shown in context of its reasonable and logical foundation in the designated *research problem*. In some cases, the researcher may choose to give some indications of the sorts of investigations and specific inquiries needed to deal effectively with the questions. But the first section of the proposal is not the place to introduce instruments and questionnaire items. These belong in the Section 3. Nevertheless, a well written Section 1 will prepare the reader, at least in general terms, for everything that will follow in the proposal. It may even be able to suggest a forecast of the criteria that might be used for assessing the outcomes of the inquiry.

SECTION 2: How This Problem Has Been Handled in the Literature

The **precedent literature** that underlies and rationalizes this inquiry is reviewed in sufficient detail to justify the concepts, the logic of investigation, and the prospect of utility of the probable findings. The chapter need not deal in detail with every item in a bibliography on this topic; instead it should focus with supporting illustrations on the most pertinent and substantially relevant portions of the literature base. A careful selection of samples from authoritative texts will help to describe the researcher's understanding of the *research problem*. Section 2 of the proposal should provide a sampling of the world of ideas underlying the research.

In this way the researcher is able to introduce the literature that thoroughly explains the inquiry. The selected references must provide illustrations from precedent literature to justify the concepts, explain the logic of the investigation, and make the case for probable outcomes having promise for usefulness and enrichment of understanding of the problem.

The chapter need not deal in detail with more than five to ten of the most significant themes and lines of inquiry that are foundational to this research. Using supporting illustrations, try to focus on the most pertinent and substantially relevant portions of the literature base.

Introduce more detail on any sources that were included to illustrate the decision about the *research problem* and *research concern* in Section 1. Showing the major bones of the skeleton of the literature is more important than showing the breadth of literature in the field. The review of the literature should not attempt to be so broad that it scatters across topics with minimal relevancy to the selected research problem. It is not wise to show equal concern about everything that relates to the general domain.

It is expected that the researcher will have identified the matters toward which the research concerns are focused. In this section of the proposal each of the important aspects of the research concern should be identified and clearly illustrated through appropriate citations. To keep these excerpts from becoming a jumbled mass, be sure that they are presented as a reasoned discussion of the problem, its background, and its importance to the field.

The inquiry should be outlined in the style of a grand preview of the entire research plan. The key assumptions and hypotheses that provide the logic for the planned inquiry should be introduced and briefly explained. Focus strictly on the literature that underlies and rationalizes the chosen inquiry.

Provide sufficient detail to justify the concepts, the logic of the investigation, and the prospect that outcomes will be useful.

SECTION 3: The Inquiry Plan for This Research
This section will name the *research questions* and provide a logical connection for each with the *research concern* that has motivated the inquiry. The logic of the inquiry, its procedural rationale, the researcher's plan for collecting data, and the estimates of prospects for providing answers to important questions should accompany the listing and explaining of the research questions proposed as the major guidance of the planned research.

This section should include a proposed time-table for the following milestones: defining the RQs and shaping them into tasks for the data-gathering procedures; transforming the research questions into items for the actual data-gathering; describing and specifying the population and sample for the study; scheduling the data-gathering (whether interviewing, observing, questioning—or other instrumented information-gathering); processing the data, and summary documentation. This last step will likely need to be subdivided into concerns for transfer of data from field notes, verification, and editorial counsel.

SECTION 4: The Management of Data in This Study—Findings
This section, especially, assumes that a qualitative research methodology will be used. Qualitative research is concerned with exploring and discovering the characteristics of a situation, usually a social situation. Thus the inquiry is more open and alert to emergent factors that would be typically pre-determined in a quantitative research. Section 4 is fundamentally different for the two major types of social inquiry.

Qualitative research is not guided by specified hypotheses, though it ultimately serves as the shaper and source of the hypotheses that guide quantitative research. Without the foundation that qualitative procedures provide, there is inadequate basis for hypothesis-building. Thus the essential component of quantitative research is missing.

Choosing the Research Mode
Which is better research—qualitative or quantitative? Which is easier? *Qualitative.* Which is more valuable: *Qualitative.* Which makes a better book? *Qualitative.* Which should be done first in a "new" research problem? *Qualitative.* These factors explain why qualitative research is so often chosen as the preference among doctoral students.

Which is apt to be more precise—qualitative of quantitative? *Quantitative.* Which can be accomplished more quickly? *Quantitative.* Which is apt to be perceived as contributing findings that are more reliable? *Quantitative.* Which is more likely to be regarded as "real research?"

All these questions are relatively unimportant. They are simply argument-starters. From a scientific and philosophical perspective, there is no legitimate contest between the two types of inquiry. Both are needed. Indeed, there are two major types of research; these are commonly identified as *qualitative* and *quantitative* (sometimes called "descriptive" and "experimental"). Each facilitates understanding of phenomena in the natural, social, or psychological universe. Each answers questions. Each utilizes a rigorous process for developing knowledge. Each is scientific if implemented in keeping with empirical standards.

Two Purposes of Scientific Research

But here the similarities end. The two major types of research serve differing purposes. Qualitative research is concerned with what can be learned of phenomena through descriptive procedures. Quantitative research is concerned with building theory through the testing of hypotheses. The logical sequence of these two processes is evident; it centers on the theory-building paradigm in which description comes first to identify the components and the interactions evident in the phenomenon. Qualitative research, the first operation of inquiry, describes, builds taxonomy, and proposes the relationships and functions of the interactions. These proposals are formed into hypotheses to allow the second process of the inquiry: testing the hypotheses experimentally in carefully controlled circumstances and using more refined observations.

Standards in science insist on models of data analysis that are based on the logic of experimentation. In simplest terms this means that the situation is examined (measured) before and after the treatment (often called the intervention). Statistical tests are then used to determine the probability of the difference (change, if any) occurring by chance. Findings are then interpreted (explained) in terms of the probability that the intervention contributed to or caused the change.

The model for experimental research is strong in terms of the probability that what has been observed is not a chance occurrence. The major weaknesses of the experimental model are that change established experimentally may be relatively unimportant or the extent of change may be inconsequential. Further, the contrivance of experimental conditions may create unrealistic constraints or biasing of the environment sufficient to lead to distortions of the measurements. Another matter that is often overlooked is the necessity of finding reliable and valid measurement procedures which will not introduce interferences or distortions in the data-gathering.

Outcomes of Qualitative and Quantitative Studies

There is no legitimate contest between the two types of inquiry. Both are needed. The following lists suggest the major contributions of each:

Qualitative studies . . .

- Build the necessary vocabulary and enable communicable description.
- Discern and describe the apparent interplay among factors in a system.
- Determine coincidences and possible dependencies and co-dependencies,
- Enable further exploration to identify additional components or factors that may relate.

Quantitative studies . . .

- Allow for hypothesis testing in a reasonably stable environment.
- Accommodate the conditions needed for complex statistical analysis.
- Establish cause-and-effect relationships.
- Determine predictable inter-play between factors in relationships.

Section 4 of the proposal must include a description and explanation of each of the necessary statistical treatments of the quantitative data to be collected within the research.

Each of the research questions should be carefully linked to the supporting data that will provide the field a basis for deeper and more disciplined understanding of this factor or variable. That may sound simple, but it requires careful discipline in the inquiry, discovery, and testing, as well as calling

upon the researcher's creativity and communication skills to compose the text needed for effective dissemination.

The task is to search out, to observe in action, and thus to describe what seems to be occurring. As soon as the behaviors and interactions of the situation are understood in descriptive terms, hypotheses can be formed and tested, adding evidential materials to the theory of the problem field.

SECTION 5: Interpretation of the Findings—Reaching Conclusions

Section 5 of the proposal is likely to be a relatively brief piece that develops one of the following three generalized models:

Model One. The findings will be summarized and presented. They will be presented in such a way as to demonstrate their relevance to the research questions which have guided this study. Conclusions will be based on evidences in the findings.

Model Two. The findings have demonstrated the extreme difficulty of reaching conclusions about these matters. Conclusions are weak since they are not grounded in demonstrable evidence. In a subsequent study the researcher would be well advised to deal with the difficulties encountered and to develop a simpler research plan that will deal more effectively with the difficulties. NOTE: Where warranted, an acceptable dissertation could be a report of inconclusive findings.

Model Three. Findings from the various research questions are brought together in this chapter to illustrate what can be supported on the basis of this inquiry. The intention is to relate each of the themes noted in the findings to the facets of the research problem for which they provide new insights or add more confidence to previous evidences. Thus the conclusions follow from the findings.

Which Model to Use as Section 5?

The first of these models is classical and precise. Rarely, however, do things turn out quite so neatly! To use this model, the research would need to be very thorough, especially in the treatment of precedent literature, and then follow through with a comprehensive theoretical grasp of the design and pursuit of inquiry itself.

The second model is a somewhat polite raising of the white flag of surrender. Somewhere along the line, the research simply does not work out. Rarely do we see the second model in practice—not that it wouldn't be more honest and explicit than a high percentage of doctoral dissertations.

The third model is recommended. It calls for a well-articulated relationship among the five sections of the research plan as proposed.

Again using the research questions as the organizing structure, the proposal should show how the conclusions may enlighten the research problem. Of course, since this is anticipating what is not yet discovered, any discussion of conclusions should be approached as one would talk about a gift that has been received but not yet unwrapped. The researcher should feel free to suggest the major possibilities that may be expected from the research findings. Insofar as possible indicate how these interpretations of the findings would provide explicit answers to the chosen research questions.

Make very clear the steps to be used in drawing findings that are supported by statistical or evidential data and relate them to the narrative of the research.

RESEARCH PLANNING—AVOIDING THE TRAPS
Discussion Paper #3

Discouragement is rarely constructive. Helping people make choices and supporting them in their quest toward worthy goals is a positive business. Even *warnings* can divert and suppress the creative process. But sometimes a timely warning can head off catastrophe.

Possibly this is such a time. This paper is intended to provide a map—an assistance toward making wise choices and increasing the probability of arriving safely at worthy goals. Just as mountain hiking is both exhilarating and at the same time dangerous, research planning is a delightful, challenging, and significant experience, but there are hazards along the way. Perhaps a few warnings can save the newcomer some grief.

To Begin with the Negative
First, several very firm "please don't" items. These are most important; *ignore them at your peril!*

Don't wait. It is never too soon to begin thinking about the research in which you will invest. Yes, think of it as an investment. Wise choices within the research planning process will result from a sequence of tasks. Get started *in earnest*: Focus your reading on matters that will bring you up to speed on the research problem you are moving toward. Keep a logbook on all sources that turn out to be useful. Consider as possibly useful any reading that identifies its own sources. Research never starts from "nowhere." It is best to assume that everything has been written about already, perhaps with different words and from angles that may seem odd to you. Just dig.

Don't think of your research as an assignment. If it were just one more collegiate assignment you would likely bring into it every bad habit you have fallen into since childhood. Instead, focus on what you are *becoming*—***a researcher!***

> *No one is telling you that you MUST do a dissertation.* You chose this academic route, and the decision brings some heavy responsibilities. You may have made the wrong choice, but don't transfer the blame! You wouldn't be where you are today if you weren't able to do what it takes. Go for it!
>
> You shouldn't expect to finish anytime soon; but at the same time, set yourself incremental tasks—do one bite, then another. It will *always* appear that there is more.
>
> You will rarely get much feedback. This "loneliness" comes with the territory. Seek out a colleague for frequent back-and-forth sharing and critique.

Don't make a premature decision. In some ways, dissertation planning is like courtship. It is not wise to fall madly in love with the first person you meet! Dating has its purposes; one of those is the important matter of getting well acquainted before you commit yourself! The way to test out the possibilities is through reading—lots of reading, discussing ideas with academic colleagues, and

through the writing of short drafts that illustrate and sketch out each possibility as it emerges in your imagination. This chain of events should begin *very* early in your doctoral studies.

Don't assume that course work, seminars, and comprehensive exams must come first. *All* your doctoral studies can (and should) contribute to your research planning. There are markers along the way, but all the way along you must invest time and effort in preparing yourself for the dissertation.

Don't hang onto an idea after it becomes apparent that it is out-of-date or lacking in foundational literature. Your research should be grounded in matters (research problems) that are already carefully examined and for which theory has at least begun to emerge. Your research should push forward on research concerns and into research questions that are fresh. Look for gaps in understanding and ask yourself, "Would it be worthwhile to clear that up?"

Don't believe the rumors that your "original" research should "do something that has never been done before." This one is a very common folk-fable. Not true! Science does not work that way. In scientific terms, **research inquires**. Sometimes the inquiry leads to discovery, especially refinement and nuance. More often it "simply" expands the knowledge base. (This task is never as simple as it sounds!) Original has only one valid use in reference to research: *Original* means that you are the source of this particular document—your dissertation. Your fingerprints should be all over it! BUT . . .

Don't copy! International standards call for disqualification and removal of academic qualifications from *anyone* in the academic community who copies and claims (or even suggests) oneself as the source. This is a very serious matter and can create damaging handicaps in your career. Become familiar with the restrictions on plagiarism. To carelessly violate them is SELF-DESTRUCTIVE. Always identify the original author and source and use print styling that calls attention to the fact that you are *quoting*, not copying.

Professional Advice—Hopefully Helpful

Reading dissertations is part of the professor's menu of chores. Sometimes a dissertation is stimulating and exciting. More commonly, it is a task that is approached with anxiety, even fear. What makes the difference?

Be careful to use correct English grammar. This can be a very difficult accomplishment for some doctoral students, especially those for whom English is not the first language. One hint for people with English problems: Write first in your native language. This will assure that you are writing what you intend. Then translate your text into English. Many students follow that with help from a fellow-student who brings native English. For Americans, weaknesses in prior educational experiences are accountable for some of the more grotesque reading experiences that professors are expected to endure. Grammatical carelessness and spelling problems scattered through an otherwise promising paragraph can poison the whole thing. The reality is that these "little" errors of language usage can cloud the text and thus seriously bias the perceiver. Don't take that chance. When you ask a professor to "give the paper a quick scan to see if is 'on track'," it should be clear and clean. Quick scans of sloppy work do not work to your advantage!

Make sure you are making sense. Failure to carefully proof-read your work is a common ailment. It can slow the research process frightfully! Leaving out parts of a sentence, failure to connect one element with those on each side of it, assuming that the reader will fill in the parts of an idea that is incomplete—these are among the most common problems. Any of these may result in the dreaded

criticism: "I have no idea what you mean." That's the comment you don't want to hear more than once! When a professor takes the time to read what you have written and takes it seriously enough to mark it up, *do something about every single marked problem!* When you come back later with another version to be read, no professor wants to find the same errors or gaps that were noted before. That can be hazardous to your health and welfare.

Review the whole document to search out errors or gaps of the same sort as those noted by the professor or reviewer. A professor should not be expected to be as thorough as you will need to be. After all, it is your writing, not hers or his. Cleaning it up and communicating clearly is *your* job.

Avoid painting yourself into a corner. The surest way to get yourself into trouble is to deliberately espouse narrow views, especially in philosophical and theological discussions. There is nothing wrong with taking biblical positions and defending them, but reasoned conversation among scholars is difficult for those who use half-truths, unfounded pious-sounding assertions, and slogan-slinging to win their points. It is far better for a Christian scholar to take a charitable position toward others whose positions seem (at first) to be at odds with Christian principles and godly virtue. Bear in mind that error is not uniquely the property of non-Christians. The "Christian position" and the "Christian worldview" are not immaculate concepts. Avoid adding to the dissonance through your misguided fervency.

Especially when your habits have become loaded down with hostility toward people who see things differently, you are apt to find yourself disregarded and marginalized. Winning people to your viewpoint should not be your highest value and your most obvious purpose. Your primary professional tasks are to listen well and to engage others with your willingness to add your own position to those you wish to reflect upon. Reflection and relationship-building are greater virtues than using truth as a hammer—or worse yet, as an ax.

Scholarship demands collaboration. One popular fallacy about scholarship is that the scholar's life is a lonely life. Indeed, some choose to make it so, but that is not the true norm. The fact that Jesus began his ministry by building a close relationship with a dozen men stands as prime evidence that even the Son of God needed people around him for fellowship, spiritual relationship, and interaction. Jesus established a remarkable example of social life in a world more familiar with religious leaders being isolated, self-important, and ultimately lonely. As you move toward becoming an active researcher, you need colleagues. You need others to reflect with you about your observations, the meaning of your experiences, and the worth of ideas. Talking together is one of the truly significant modes of learning. The capacity to think depends largely on the ability to put ideas into words, reciprocated by the eager willingness to listen and to devote oneself to the collective of learners. These motives are evident in the most ancient of human documents. Consider the "schools of the prophets," the body-imagery in Pauline ecclesiology, and the emergence of monastic fellowships. And those exemplars are reflected in many other of society's forms, most especially in the worldwide phenomenon of schooling—despite its many shortcomings. Indeed, to be a scholar, a researcher, a leader—these aspirations are collective pursuits. *We need each other*

RESEARCH PLANNING—PARTS OF THE PROPOSAL
Discussion Paper #4

Whether writing a proposal for the dissertation research or organizing the final dissertation, the researcher should use the typical five-section format. Each of the sections makes a unique contribution to the presentation, and each section fulfills a designated portion of the presentation. Thus these ten segments of the writing (five plus five) enable the researcher to maintain a more orderly control of the drafts of manuscript.

The five thematic topics, often given **Section Title** names in the proposal and appearing as **Chapter Titles** in the dissertation, are listed following:

1. *Research Problem*
2. *Precedents in Literature*
3. *Plan for Inquiry*
4. *Data Gathering and Analysis*
5. *Conclusion*

While there is no eternal law that requires these exact words, using them as shown will accomplish several important goals: these wordings are *brief*, *explicit*, and *indicative of what should be described* in the respective sections of the proposal. So even if you prefer to innovate, stay close to these characteristics.

The dissertation proposal should be written in a style that puts a premium on brevity. It is not considered good form to be encyclopedic. Tell your story in crisp and reasonably short sentences. Make your points, explain where necessary, and move forward. This will prove to be useful advice up to the point when you realize that what you are proposing is a bit different from what your reader is expecting. Then you will need to add clarifying statements, some quotations from the background literature, and a convincing argument for handling the concepts and the inquiry methodology in the unique manner that you envision. This is the necessity that will consume much time and effort as you compose this otherwise brief document. In any case, try to keep it under a dozen pages.

Writing is an individualistic undertaking. No two people share exactly the same vocabulary and style as they write. Each person has a unique approach to creating a first draft and each person judges that a text is acceptable at differing points on the "refinement scale." These differences explain the very different effects made by variations in substance and quality. In order to move toward a generally higher standard of competency, certain time-honored suggestions can be made.

SUGGESTION ONE: Write as if you are answering questions.
Imagine the most important questions that you could be asked about each section. Develop your proposal on the basis of the answers you are offering for each of these questions. Perhaps you could start with the suggested questions which follow and develop the list into just the right questions that you will need to answer as you describe your own research.

1. *Research Problem*
   What are your interests and concerns that motivate this research?
   What experiences or events are the sources of your concern for this research?

In what ways has your reading affected your sense of the importance of this research?
What workable questions do you see as next steps toward understanding this problem?

2. Precedents in Literature

Who has provided the important conceptual basis for this research concern?
Whose ideas are you most confidently relying on as grounding for this research?
What clusters of authors and topics do you believe provide important background?

3. Plan for Inquiry (your "Methodology")

What questions will your research attempt to answer?
One by one, what will you actually do to get the answers for each?
Where will you get the data needed for each?
In what ways will you safeguard against bias?
How will you conduct your inquiry?
From whom and how will you collect your data?
a. What will you be observing?
b. What sort of instruments will you be using? (include sample items)

4. Data Gathering and Analysis

What will be your schedule and plan for collecting data? (This material should be an outgrowth and complement for matters you have introduced in Section 3.)
How do you propose to analyze the data you collect? Logically? Inductively? Pragmatically?
What procedures, techniques, or statistical routines will you employ in the analysis?

5. Conclusion

What do you expect to be some of the matters that your findings might enlighten?
In what ways will your interpretations be careful not to overstep your study's limitations?
What uses will you make of theological and Biblical materials as you interpret findings?

SUGGESTION TWO: Write with a minimum of prediction.
As you write a proposal for a research project you may discover the temptation to project yourself ahead in time and to write as if you already know how the research will "come out." NO, DON'T. If you let your biases show in the proposal, your reader will wonder if you are really approaching the inquiry with integrity and a sense of exploration. If you are so sure of your findings and so convinced of their meaning, you are betraying the value of scientific inquiry. Your task in carrying out the research is *to find out if or how* actuality and reality play out. It is obviously inappropriate to presume to *know* what you expect to *discover*!

This advice is very hard to follow, not just because of your proximity to the question-asking and question-answering processes, but because of your intimate involvement with the issues at stake. Here is where your expertise is put to the test. Can you honestly be open-minded about matters that you have a stake in shaping and ultimately controlling? Being a researcher is like any other duty that entails authority. Sooner or later you encounter an overwhelming temptation to assert power.

This very common problem among otherwise competent and well-educated academic persons calls for strongly formed values of humility, selflessness, and integrity. For a professing Christian, this is especially important.

SUGGESTION THREE: Join a writing team.
As surely as novelists and poets need situations in which they read and critique one another's writing, the researcher—especially the neophyte researcher—needs time with others who are engaged in similar pursuits. The time that this will require often causes it to be listed last on the task priorities. But the wisest of neophytes are often the ones who most easily recognize that working together with peers is one of the best ways to learn.

The researcher too easily becomes an isolated recluse (the halls of ivy are full of them). Avoid that crowd. Find a little balanced group wherein give-and-take can become part of your lifestyle.

SUGGESTION FOUR: Anticipate the long-term value of your proposal.
The dissertation is destined to be around for a long time. It will be lurking (gathering dust?) on academic library shelves or turning up on digital databases until the proverbial cows come home. Yes, the dissertation, for better or worse, is emblematic of your passage into the over-rated distinction of the PhD. Make the best of it! It will be bound up in glue and cloth. Its title, and your name, will be lettered in gold. It will be elevated to a long shelf of similar monuments to blood, sweat, and tears, on the hardest-to-reach shelves of dimly lit libraries. So much for heritage. But make no mistake about it, when you are nominated for Senator, or called for jury-duty, someone will be assigned to read it. Make sure you can stand behind what it says.

But the much less prestigious "proposal"—what about it? There are three good reasons why writing the dissertation proposal should also be taken very seriously and treated with respect:

1. Someone may read it. Seriously, several "someones" *should* read it! In the strongest graduate schools it is common for the library's files of dissertation proposals to be quite popular. Students for a given degree spend considerable time reading previous and similar proposals, looking for clues about literature and reflecting on procedures of previous inquiries into similar research questions.

2. It will be useful as an example. The chairperson of many a dissertation guidance committee will maintain a list of outstanding dissertations and refer subsequent students to them as guides for their forthcoming proposals.

3. It is your investment in a sound dissertation. A well-developed proposal is an important artifact (a sort of stepping stone) in the process of writing your dissertation. You owe it to yourself to commit yourself to the proposal with the same vigor you would expect to use for the dissertation. It is like money in the bank. Do it carefully and systematically so that you will be able to use substantial portions of it in the writing of "the real thing!"

GETTING STARTED IN QUALITATIVE RESEARCH
Discussion Paper #5

The Difference Between *L* and *N*

The ***L*** means Left; the ***R*** means Right. Right? But what about **L** and **N**? These two little letters make a big difference when they differentiate tw**o** rather similar words (which should ***never*** be confused with one another!)

QUALITATIVE and **QUANTITATIVE** are the two major types and functions of scientific research. It is not appropriate to use either word when you intend the other. QUALITATIVE, based on the word *quality,* is the sort of research that looks for the qualities or **characteristics of something**. QUANTITATIVE*,* based on the word *quantity,* is the sort of research that is concerned with **extent** or **amount.** Remember this:

Qualitative = description
Quantitative = measurement

If you remember this contrast you won't reach for a tape recorder when you should turn on your calculator!

The Scientific Understanding of Research

The purpose of research is inquiry. The researcher's task is inquiry. The possibilities are for discovery of valuable information and understanding. You should not be motivated by the intention or hope to "prove" something. The merit of the research will show in its reasoned foundation and structure, its careful design, and your integrity in demonstrating real events and consequences—even when the results seem to be less than would be needed to make you rich and famous.

Scientific purposes are accomplished through research. Always bear in mind that the researcher's task is to extend knowledge and understanding. False findings and exaggeration will retard science. Don't engage in deception.

Scientific research is orderly and structured. In every aspect of science certain rules, standards, and protocols guide the researcher. As the watchword says, "Get with the program." Even for aviators there are "rules of the sky" requiring many hours of ground school before the solo flight. Be wise. Put in your time and engage in orderly flying! It will shape your career and make your life happier.

In many ways, the two types of research are sequential: *Descriptive studies* are necessary **to develop descriptions and to build taxonomies**. What is it? And how does it seem to be functioning? All the steps of discovery and orderly examination of the visual, aural, philosophical, and logical (perceptible) parameters of a phenomenon are first dealt with through descriptive (qualitative) research. As a result of these efforts, sometimes requiring years of research, hypotheses should become evident. **Hypothesis testing** is the task of *quantitative studies*. The hypotheses that "hold up" under rigorous experimental testing are then assembled into the theoretical literature of the field.

It should be apparent, but is often overlooked by neophytes, that hypotheses do not drop from the sky fully formed. They must come from diligent efforts to clearly define and describe the phenomenon as well as the events and relationships within which it occurs.

A Strong Proposal

Writing a proposal for a research project is a learning experience. The proposal is both the *means* and the *end*. The proposal is the standard means to assure that the researcher has carefully examined the task, defined it well, and clearly set out a schedule of investigative steps that will result in new understanding and new strength for the literature of the field.

The proposal is also an end. It is a self-standing document that may be used by others in the field to guide their subsequent research. A thorough proposal can help "show the way" for those who follow. Thus it is not a throw-away document. A well-formed proposal needs to deal with five thematic topics, often assigned as **Section Titles** of the proposal or **Chapter Titles** of the finished dissertation:

1. *Research Problem*
2. *Precedents in Literature*
3. *Plan for Inquiry*
4. *Data Gathering and Analysis*
5. *Conclusions*

Defining and describing. Every research proposal begins with the task of defining terms. Following is a list of the definitions and descriptions needed in almost every research proposal:

- Describing and defining the *research problem*. What topic or theme is at the center of your research? (Hint: At best, the problem area should be recognizable as a professional priority.)

- Rationalizing the research problem. Why is this problem worth studying? What could then be "opened up" for further study? Are any practical implications already evident? What are the major points already in literature that relate to this problem? What further inquiries have been recommended by others studying these matters?

- What particular research questions are you expecting to explore? The items in this list should be carefully described, one by one, to illustrate the focus and motives of your inquiry. Each should be shown to be linked fundamentally to the research problem you have already identified.

- Specifying and rationalizing the variables to be examined. What are the particular factors that you will be examining or comparing? Make very clear how each of these is important to the research problem you are pursuing. How do these variables relate to the if-then elements of the hypotheses that you will be testing or that may likely be emergent in your research? How will they be measured or described?

- Defining the experimental, standardizing, or control conditions that you will have in place for the inquiry. What particular factors are intended to indicate the evidence that your research questions or hypotheses call for? How will the data be safeguarded against bias and unidentified presuppositions?

- What steps will you take to assure that the comparisons or the descriptions are fair and unbiased (whether intentionally or accidentally)? It is especially important that any experimental or quasi-experimental facets of your research be described in detail in Section/Chapter Three.

Examples of Appropriate Types of Qualitative Research

Comparison of two or more sets of data: For example, different subjects, different times, or different treatments. Such studies provide first levels of useful grounding and may open up deeper inquiries in a relatively unexplored field.

Proposing Relationships. When likely causes and effects begin to become evident and reasonable, the research task moves toward identifying the hypotheses that would, if substantiated, affirm these relationships.

Framing hypotheses. Testable hypotheses should be stated with due caution (necessary in a quantitative research). Many misuses of science can be traced to the premature linking of factors that turn out to be less than cause-effect. The difference between a true causation and a mere co-dependency provides fertile ground for wild-eyed enthusiasm about falsehood. Science demands better.

Gathering data. Sometimes called "collecting" data, the choices of subjects for the research, the choice of conceptual frameworks and linguistics of the instrument or the interview questionnaire, and even the mind-set of the research planner can bias the data. In far too many proposals, the subtleties of the data gathering process are treated far too casually and thus too simplistically. It should not be assumed that the data-gathering will be done carefully and with alert concern for bias and mistaken interpretation. It is very important to provide thorough details on the procedures planned for gathering data.

Designing Items for a Questionnaire. Starting with a list of the raw questions that get directly at the core of the research questions, the design of the questionnaire should take into consideration the range of competencies, levels of understanding, and varieties of experiences of the subjects. Next, several attempts should be made to develop the first list into items that are closer and closer to the verbal communication styles of the subjects. The third task follows: testing the list against a small sample of respondents whose comprehension and capabilities of communication about these matters is matched to the ranges that will be encountered in the actual fieldwork. Final revision then can proceed.

Questions that can be answered with one-word responses should be avoided. Every short-answer question, if indeed it is worthwhile at all, should be followed with a linked follow-up question that will give the respondent an encouragement to "say more about that."

Searching for Further Guidance. Selecting appropriate procedures for *qualitative research* most often relies on ideas borrowed from anthropology. Studies of human behavior in the context of social relationships and institutional forms have brought into scientific utility many useful models of research. Scientific polling is one such illustration, from which there is much to learn about classifying, sampling, questioning subjects, as well as recording, analyzing, and examining data statistically.

Guidelines for Interviewing. Since interviewing is one of the most important means of collecting qualitative research data, the skills and techniques must be learned and continually refined. Interviewing is a learned skill. Granting that there are personality factors that distinguish the best interviewers, much can be gained from the old adage "practice makes perfect." Delays in the research schedule and worse disasters are predictable if the neophyte enters a series of interviews without preparation and practice. It is recommended that the interview-trainee should undertake several hours of practice sessions with a

knowledgeable partner who can critique and advise about ways to improve, expedite, and ably fulfill the interviewing procedure.

Practical advice for the Interviewer:

1. Be sure you have a written interview guide—but don't feel tied to it. The best interviews will come from the careful use of a shorter list of questions with more freedom for the interviewee to "open up."

2. Prepare for each individual interview. Prepare well enough that you *never* need to read your questions to the interviewee. Make the interview flow smoothly and more like a conversation.

3. Small audio (digital) recorders are very useful. Check the batteries and be sure of reliable operation! Ask your interviewee for permission to record.

4. Test your equipment *before every interview*.

5. Show a reassuring and welcoming attitude, but *never* reveal what other interviewees have said. *Do not mention to an interviewee what you have learned from another interviewee.*

6. If you find that some of your questions are unclear until you add explanations, *rewrite such questions* as soon as possible. Work on them until they don't need to be explained.

7. Concentrate on the more productive questions even if it means setting aside some of the less useful questions. ("Useful" can be defined empirically as you become more familiar with your protocol and the responses that it elicits. A useful question is one that results in diverse responses of varying intensity from different people, giving you evidence of the range of positions and judgments held within the sample.)

8. Be a pleasant interviewer; don't be stiff, formal, or mechanical! Avoid sounding like an automatic telephone! If you are bored, conceal it at all costs!

9. Avoid *Yes-No* questions except occasionally as a way to get a conversation started or re-started.

10. Pursue the details that the interviewee reveals. Ask for illustrations or examples whenever you need them. While it is still fresh, find out what's on her or his mind. Don't settle for ambiguous responses.

11. Plan your interview schedule to allow 5 minutes after each interview to record some preliminary and immediate commentary notes. What struck you as three or four highlights of the interview? What did you observe that may not show up in the audio data on the tape-script?

The most important understanding of your dissertation: Your PhD dissertation provides a learning experience in systematic inquiry that moves you into the mainstream of research. It may or may not break new ground, but as your initial contribution as an emergent researcher, it must be your best representation of truth. Remain faithful!

Getting Started in Quantitative Research
Discussion Paper #6

A British colleague once described qualitative research as "mucking about in the data." If that is true then quantitative research is "making very sure that the findings answer the questions." The issue is neither getting messy in the field in the first case or stacking the deck in the second, but the contrast rather well befits the categories. Whereas the first of the research tasks is to define and describe, the second is specifically to test grounded hypotheses.

Why the anxieties about quantitative research? Quantitative research uses a precise vocabulary. Much of this vocabulary is concerned with the mathematics of statistics. Indeed, it is this centrality of statistical handling of data coupled with the typical American avoidance of math that accounts for the avoidance of quantitative research.

The facts say something somewhat different: any given quantitative research study will typically use two to five statistical routines, each of which can be set up on a computer to run like lightning. Math knowledge is not the difficulty; the sort of statistical knowledge required is a matter of knowing what statistical routine (or routines) will be appropriate to test the particular data set against models of probability. And that does indeed require some specialized knowledge. But there are adequate helps available to guide the researcher through these decisions. Fear not!

The material following will introduce the essential terminology and bring the neophyte up-to-speed on the basics:

Mythology. Two items of mythology need to be removed from the logic of quantitative research design. The first of these is commonly accepted as truth by many graduate students, but it has no science in it*: If the research indicates that a hypothesis is not supported, the research has failed.* This sort of "downer" is feared as a harbinger of failure and perhaps death. No Way! Any responsible research that finds that a hypothesis is not supported by the experimental findings, though perhaps disappointing, is nevertheless of value to science. Such a study has every right to be published and will be heeded by the scientific community. Nowhere is it written that an experiment should come out a certain way. Only in fairy tales and "B" movies do the stories end predictably.

Another beginner's misunderstanding is similarly based on a fallacy: If you find that someone else has already done the study (*your* research!), your efforts are disqualified. Your game is lost. The only truth in this situation is that if you find belatedly that a similar study has already been done, you should have made that revelation available in your Chapter Two, and you should explain how and why your study intends to add new evidence to the previous research.

The first of these myths usually occurs in experimental research, but the second may show up within the operational time of a descriptive research.

On a more serious note, a research proposal must use certain terms in certain ways. Most commonly, *experimental research* is intended to test hypotheses. **Hypothesis** is most easily defined as ***an if-then statement that can be tested***. The *if-then* form is illustrated in this simple idea: ***If*** students are sleep-deprived ***then*** they will be less attentive in class. The experiment will be

designed to put this hypothesis to an honest (fair) test. Subjects will be randomly assigned to two treatments. Subjects in the Experimental Group (A) will be awakened by a timed buzzer once each hour for two consecutive nights. After the second night, Subjects A and the Control Group (B) will be observed by teams of observers on one selected day immediately before the experimental treatment and again on one day in the week after the experimental treatment. Records will be kept of persons resting heads on desks, yawning, engaging in apparent note-taking, and initiating comments and questions (on-topic) during the class session.

The experiment outlined above may help to clarify the *if-then* hypothesis and how it might be tested. Also, the illustration puts to rest another of the common fallacies of research planning. Note the use of observation as the procedure intended to lead to the collecting of data from the experiment. In this simple case, it is clear that observation is one of the ways data can be collected—whether for a descriptive study or for an experiment. The assertion that observation is useful only for descriptive (qualitative) research is a fallacy.

Another term used frequently in research planning needs explanation:

S***ubjects.*** Sets or groups or people who have been selected to participate in an experimental research—or merely have been selected to be interviewed, surveyed, or observed in a descriptive research are called subjects.

The basic model of ***treatment and control*** is the basic schematic pattern for most experimentation. It is common to animal experiments in many fields of science, and it is the starting place for many human-subject studies in psychology, education, and occasionally in sociology. The assumption is made that if two very similar groups of subjects are treated differently, the difference will be evidenced in the hypothesized way.

One of the most common procedures in quantitative research is to set up two groups, a *control group* and an *experimental group*. These two groups must be as nearly alike as possible (an almost unrealistic possibility when dealing with human subjects, but the researcher is obliged to try). Then if one group experiences the experimental treatment (e.g., consumes six hamburgers each day), any difference in the **mean weight** of the two groups is likely attributable to the diet heavy in hamburgers.

The subjects are commonly divided into two or more subsets; one will be given no experimental treatment, and the other group will be given the specially designed treatment representing the experimental variable. (Depending on the design of the research, there may be more than one experimental group.)

In this illustration the not-to-be-tampered-with group is designated as the ***control group***. Ironically, only the subjects in the experimental group are under any substantial "control," inasmuch as they are experiencing the experimental conditions. The subjects in the control group are called "control" because they go about life as usual, serving only as an indication of what would be the situation if the experimental subjects were not being "subjected to" the experimental treatment. (Language does get complicated!)

The ***experimental group*** is sometimes called the treatment group, because they are the subjects who are engaged in the *independent variable*. Ordinarily the existence of an experimental group suggests that there are other subjects assigned as the control group.

You likely noted another term: ***mean.*** Mean is one of several applications of the more common word *average*. In scientific writing, "average" is devoutly avoided. Statistical matters require more precision than "average" can provide. Mean, or more precisely, *arithmetic mean,* is calculated by adding the numbers in a set and dividing by the number of items in the set.
Thus the *mean* of *18/23/25/25/28/29/32/55/40* is 30.5555 **MEAN: 30.5555**

Just to make the story complete, the other two words which also can be understood as "average" are ***mode*** and ***median.*** **Mode** is the number most frequently occurring in a data set, and **median** is the number that is in the middle of the numbers in a set after it has been arranged in rank order:

18/23/25/25/28/29/32/55/40 **MODE: 25**
18/23/25/25/28/29/32/55/40 **MEDIAN: 28**

Probability. In simplest terms, ***probability*** is a matter of "odds." What are "the odds" that a flipped coin will "come up" heads? All things being equal, the probability is 50-50 or "1 in 2". This is the same principle that is the basis of a lottery. If the determination of the winner is fair and honest, it is also statistically calculable. You can know what your probability of winning actually is your number, 1, divided by the number of people entering. In a door-prize lottery you get one chance divided by 25, or whatever is the number of people attending the party.

As this principle of probability is used in research, the probability is tested statistically, using elaborately derived formals to determine if a certain outcome could have occurred at random or because of a certain "treatment." In other words, assume that there are 100 people in a group and 50 are asked to read a frightening story. Then a careful observation reveals that 5 of these 50 experimental subjects are discovered to have white hair. The results of this experiment should *not* report that reading the scary story causes hair to turn white. Such a conclusion would depend on one or more major flaws in the design of this study. How many can you identify?

1. Is the proportion of 5 in 50 also similar in the control group?
2. Did the hair color change because of reading the scary story?
3. Are the 50 selected to read the story representative of the 100?
4. Are these findings different from what could have occurred at random?

A well-designed experiment would consider all such possibilities for error and would build into the design safeguards and controls to rule out all such factors that would result in misleading "findings." The four questions above represent threats to **validity** of the findings. What can be done to reduce the threat posed by each is suggested in the corresponding four design features in the list following:

1. In a random sample of any 20 of the (control) 50 are there one to three white-haired persons?
2. A pre- and post-treatment observation will reveal this. So will watching closely during the treatment.
3. Were the 50 in the experimental group assigned at random?
4. In the control group were any changes in hair color evident during the experimental treatment?

Then selected statistical tests would be employed to determine the level of confidence that can be assumed for the findings. Procedurally, It is an easy process right up to the end. And at that

point, many researchers call upon statistical specialists to select and set up the formulas for the data analysis. In most cases these statistics will be asked to compare the probabilities of change with the null hypothesized outcome: no change.

Null hypothesis. A null hypothesis quite literally calls for a prediction of no change (no hair will turn white) rather than to hypothesize that the scary story and the observed matter of hair color are somehow related. The testing of the null hypothesis is generally respected as a more precise way to set up Section Four of the proposal and to report findings. It is often the most careful way to set up a hypothesis because it accepts "rejecting the null hypothesis" as the first-level finding and calls for more data to establish the directionality and substance of the difference. "Null hypothesis reasoning" engages in the sort of mental gymnastics that makes non-scientists climb the wall, but precision is better served! Perhaps it may help to translate "rejecting the null hypothesis" into common parlance: "From the data in this experiment it cannot be concluded that nothing has happened."

Random sampling. Randomness indicates that something happens for which there is no explanation other than simple chance. We think of "drawing a name from a hat" as random. Random sampling uses a process of relying on chance alone to select some portion of the whole set to establish an honest representation of the whole set. Obviously the random sample must not be so small in number that it could not possibly be considered an honest representation of the whole population. Selecting one boy as "random sample" of his family of five boys is out of the question. On the other hand, assume that 50 boys have been drawn by chance from a population of 5,000 boys, and that the 5.000 are reasonably similar in terms of the characteristics deemed of concern to the study. But If, on the other hand, the population of 5,000 is known to include 10 highly trained professional violinists, a random sample of 50 would very likely not to draw any of these. Thus this extremely high quality of musicianship would not be represented in the data collected. The major point here is that random sampling is one way to represent large populations, but it is a highly tricky business. It is quite likely to overlook something important.

Norms. Though this word is akin to the terms "ordinary," "standard," "normal," and "predictable," researchers think in terms of *norm*s. This term often appears in quantitative research. It refers to the data tables that can be referenced for indication of the expected quantifications for a normal population or set of data from a normative sample, as for example, a control group or universal sample.

Statistics is a term used in two ways: In general, it is used rather than *numbers* to indicate any numerical or quantitative descriptor or summation. Thus, "the statistics are in" means that all the numerical data have been collected. Statistics can also be used more precisely to refer to the statistical calculations required to establish and communicate descriptive characteristics of any entity or group. One might say "Statistics show that mean scores for Group A are significantly above those for Group B." A contrast of this statement could be stated, "The difference of mean scores for Group A and Group B indicates no significance."

Level of Confidence is a term representing one of the several ways that quantitative researchers have attempted to develop measures and statistical procedures for what would otherwise seem to be subjective judgments. Efforts have been made to calculate and assign quantitative ratings for such norms. Thus *High*, *Medium*, and *Low* levels of confidence as well as the more pedestrian term, *no confidence,* are sometimes expressed as numerical ratings.

Reliability and Validity. Commonly misused and more often mistakenly reversed, these concerns for the veracity and substance of any evidence need to be clearly differentiated. The following clues will serve to keep these important matters distinct from one another.

RELIABILITY: Think RELIABLE. If it does not change but looks the same no matter how often you look at it, it is likely RELIABLE.

VALIDITY: Think VALID. If it *is* truly what it claims to be it is likely VALID.

Lists, Tables, Charts, and Graphs. These terms are easy to differentiate: A *list* is an orderly series of words, descriptions, or sometimes numbers. A *table* is a list of words or numbers ordered in such a manner as to show relationships of each item to others in the list. A *chart* is a display of numbers, words, symbols, or descriptions presented as a graphic to illustrate a particular feature or set of features indicated by the caption or system of labeling. A *graph* is a diagrammatic visual display that shows the pattern of growth, change, or proportional representation of the elements in the array. Each of these graphic forms is useful to elaborate, illustrate, and enhance the quality of communication in research proposals and reports.

Putting it all Together to Build Theory. Differentiate between hunch and hypothesis! The researcher should never call unsubstantiated personal guesses "hypotheses" in order to make them sound more convincing. Break the habit of assuming that the purpose of research is to "prove" a theory. Keep asking, what constitutes *proof?* Move toward theory-building. Only amateurs assume that evidence is proof. If you confuse evidence and truth your quest for theory will be jeopardized. Regardless of bad habits and other sources of such nonsense, *the purpose of research is to submit hypotheses to rigorous testing and thus to increase confidence in those which are not rejected—thereby building theory and increasing knowledge.*

Proof and Truth. Honesty and integrity are virtues that ultimately determine the worth of research. Agrarian truisms may be falling out of fashion, but the wisdom of the ages lies in such aphorisms as "one bad apple spoils the barrel." One wonders if scientific contributions to human understanding would not have moved society more rapidly toward the fulfillment of human potential if it were not for the "little" quirks added by moments of carelessness and incorrect interpretation. The damage done by shabby research has been long understood, but various sorts of weakness—of intellect, of will, of stress, of overwork, or of incompetency—are still evident. The moral obligation to truth and the social responsibility for accuracy and integrity are still with us. The responsibilities of the researcher are for carefully designing and responsibly executing the inquiries that truthfully extend human understanding. Every newcomer to the field should take this obligation with very thoughtful and sober sincerity.