The Art of Being a Scientist

A Guide for Graduate Students and their Mentors

ROEL SNIEDER AND KEN LARNER

Department of Geophysics and Center for Wave Phenomena
Colorado School of Mines, Golden, CO 80401, USA

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5 Questions drive research

> *In the theoretical physics community there are many more people who can answer well-posed questions than there are people who can pose the truly important questions. The latter type of physicist can invariably also do much of what the former can do, but the reverse is certainly not true.*
>
> Zee, 2003

Many scientists (and non-scientists as well) live under the impression that they don't know much about research topics that lie outside those that occupy them on a daily basis. We often feel like a blank sheet of paper when it concerns such research topics. Perhaps we know more about various areas than we think. The path to ferreting out aspects that we do and don't know is lined with questions. For example, both of us authors are geophysicists, and (an understatement) we don't know much about biomedical research. Yet if we take a topic such as cell therapy, several statements (correct or incorrect, naïve or otherwise) about this area of research can readily bring a number of questions to our lay minds. A line of thinking might go as follows.

> In cell therapy one seeks to modify the genetic material of cells in a body in order to correct the deviant behavior that causes a disease. The genetic material is stored in large molecules called DNA. Viruses modify the genetic material of cells. *Can we use existing viruses for cell therapy? How do we change the genetic material of viruses? Are there ways other than through use of viruses to change the genetic material of cells? We know of such an activity as genetic engineering. Must that technique be used in the laboratory only, or can it be applied in-situ?*

This list of what we already know and questions that they inspire can readily be made longer and progressively more sophisticated. This, of course, in no way implies that we have any degree of e tise in the field of cell therapy (quite the opposite), or that we can formulate research questions that are pertinent for front-line research in cell therapy. Even though many of our notions about the subject are likely erroneous, containing many misconceptions, we nevertheless are not blank sheets of paper regarding cell therapy or many other subjects.

The example above suggests that formulating questions is the precursor to further learning. Posing questions is such an essential part of innovation in research that we focus on the role of questions in this chapter. But where do the questions come from? When embarking on a new line of research, it is useful first to make a list of what you know, or think you know, about the research topic. You need only take a fresh piece of paper, or fresh computer page, and write down or type everything you know about the topic. Don't be critical and don't "filter" your ideas before you write them down. It's as useful to discover your misconceptions as it is to find out which of your notions are correct. This starter list of unfiltered ideas fertilizes the soil from which questions sprout.

5.1 THE NEED TO ASK QUESTIONS

> *He [Aristotle] provided a pattern of learning which is among the most difficult of all steps in science. It consists of being able to ask questions in such a manner that data can be sought for an answer.*
>
> Moore, 1993*

> *That is the essence of science: Ask an impertinent question, and you're on your way to a pertinent answer.*
>
> Bronowski, 1973

Suppose you are lost in a forest and you need to find your way out. One approach is just to go off in some direction and hope that this gets you out. If, however, you don't know where you are going and have a hard time keeping your bearing, this usually leads to endless wandering through the forest, getting nowhere. Instead, what might you do? You could ask yourself questions such as: *Can I see any landmarks, for example, a hilltop that will allow me a view over a larger area? Is there a river or a road that could lead me to population? Is there any direction, such as downhill, in which I can reasonably expect to find a way out? Are there any signs of human activity such as traffic noise or smoke?* From having posed these questions, you can start to devise a strategy for getting out of the forest.

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Consider another problem. You are developing software and are stuck with a bug in your code. How do you find this bug? You could just stare at the code long and hard. This usually leads to myopia rather than to quick results. A far more efficient way to uncover the bug is to ask yourself questions aimed at helping you find it. (This might sound like circular reasoning, yet this is the way to proceed. Before you can find it, you must first focus on where to look and what to look for.) For example, can you narrow down the bug to specific parts of the program, such as a particular subroutine? In order to answer these questions, you can insert test statements in your code or write a test program. In the short run this costs time, but over the long run it can save a great deal of time; it often is the only way to find the bug (unless you are lucky).

The upshot of these examples is that, to solve your problem, the key is not to look for answers but to ask the right questions. If you pose enough questions, some are bound to be right ones. Asking questions is essential in research because they help give focus, and without such focus we are groping in the dark. Questions posed and later pondered give needed direction to our research.

What would our world-view be like, for example, if Einstein had not asked himself the question "what would happen if the speed of light is the same for every observer?" At first sight this seems an absurd question because it implies that two observers moving with respect to each other see light propagate with the same speed. Yet if Einstein had not thought to pose this question, the theory of relativity would not have been formulated, the equivalence of mass and energy would be a puzzle, and many technological advances, such as satellite navigation (Ashby, 2002) would have been impossible. Much of our world-view and technology have been shaped by great minds who were motivated to pose and follow up on questions that at first sight seemed outlandish, but whose inquisitive nature led to discoveries that could not have been imagined before.

Perhaps surprising, when you ask the right questions the answers usually follow rather directly. But, are all the questions you ask the *right* ones? Of course not, so pose lots of them. The process, however, works only when the questions are sufficiently specific. Suppose you are still lost in a forest. Asking yourself the question "how can I find my way out?" is not of much help unless it is used as an aid to thinking of more well-defined following ones. The questions must be sufficiently clear-cut that they lead to actions that will guide you to the problem's solution. If the questions fail to do this, you have to make them more detailed. This process of asking yourself a succession of questions that are progressively more specific could be considered as a path that might lead you out of the forest. Although subsequently finding the answers to these questions could well entail much work, such a path usually is the only practicable way to start toward solving your problem.

Being innovative in the type of questions that we ask is essential. As stated by Nick Woodward, program manager at the US Department of Energy: *"we need to change the nature of the questions we ask, not just seek better answers to the questions we already have."* Wouldn't it be good if all the questions you asked were the right ones in some sense? While that, of course, cannot be guaranteed, a prime characteristic of the right sort is that the question leads to tangible action. Its promise of accomplishing that, moreover, requires that the question not be so large that the suggested course of action requires too many steps in too many directions at once. Breaking up the large question into smaller ones usually helps in formulating those that lead to action that ultimately helps solve the problem.

It is often said that scientists must be good problem-solvers. This is true, but the key to solving problems is not your problem-solving skill; instead it is your skill in posing the right questions. Often when you hit upon the right question – a particularly pertinent one – you almost immediately have solved the problem.

Asking questions is central for a related reason, one that might seem vague, even mystical. Humans have great creative power. The process by which this power is released involves several steps. First we think about a certain issue. After having formed our thoughts, we translate them into words by either saying them aloud or writing them down. Ultimately this sequence leads to actions that allow us to modify our world. We thus have the following chain of events:

$$\text{Thoughts} \Rightarrow \text{Words} \Rightarrow \text{Actions}$$

This sequence parallels the idea succinctly stated by Emerson (1841): *"An action is the perfection and publication of thought"* and formulated by Saint-Exupéry (1931) that *"in life there are no solutions. There are only forces operating: you have to create these forces, and the solutions follow."* Our thoughts, made more tangible by putting them in words, are crucial to giving us direction and moving us to action. Our thoughts are ethereal. This is not to discount the value of pictures in our minds. The ideas of some of the more creative scientists grow in their minds as pictures rather than words.

> *The words or the language, as they are written or spoken, do not seem to play any role in my mechanism of thought. The psychical entities which seem to serve as elements in thought are certain signs and more or less clear images, which can be 'voluntarily' reproduced and combined.*
>
> *Albert Einstein, in Hadamard, 1954*

Words nevertheless are the vehicle to capture these thoughts and make them more tangible. These words can be written, spoken, or simply repeated and quietly pondered in our mind – or any combination of these. Words are more tangible than thoughts and more connected to matter in the case of spoken words (sound waves) or written text. It is through our actions that we translate the words into material events.

There is a simple technique that you can use to generate a large number of questions about a research topic. Take a pencil and blank sheet of paper, and write down any question that you can think of about the research topic. Again, don't filter your questions, but freely associate and write down everything that comes to your mind. Do this for an hour and then stop. As with any effort at concentration, it is best to do this in an environment where you are alone and undisturbed because any distraction can interrupt the flow of questions.

This process is most effective when using free association, writing down any and every question that comes to your mind. Later you will discover that many of the questions you have written come across as being "stupid." That is perfectly fine. You can always throw out these questions at a later stage. If, in contrast, you are too critical of your stream of questions at an early stage, your mind might do so much filtering that you fail to write down potentially "good" questions as well. The quotation marks used here emphasize the judgmental and therefore subjective nature of the words *stupid* and *good*. It is difficult to assess at the outset what is a good question. What at first appears to be a "dumb" one, can often turn out to be a winner, one that is crucial for the direction of the work that follows. Therefore, don't be too critical in the process of writing down the questions that come to mind; write them all down.

Note that, in the above, we have suggested that you write down these questions, starting with a blank piece of paper rather than follow the alternative up-to-date approach of typing them at a computer. Computers can be distracting for this process: Email begs to be read, the internet must be surfed, and a host of projects demand to be addressed. More important, you won't find any questions in a computer (Scales and Snieder, 1999). Starting with a blank sheet of paper forces you to focus on the single task of coming up with and writing down questions. Another advantage of using paper is that later you can cut the paper into little slips that contain one question each, allowing you to easily shuffle and order your questions at a later stage. You will likely find it advantageous at some stage to transfer your questions to a computer, and certainly the word processor can be handy for doing the shuffling as well. You might, however, be surprised to find that hand-sorting of slips of paper offers a particularly effective, free-form means of rearranging your questions while also allowing your thoughts to roam over ideas and further questions inspired by your initial list. Of course, you are free to use the tool – paper or computer – that works best for you. There are no strict rules for doing research; why not try out some options and find out what works best for you?

The importance of generating ideas and questions while giving free rein to association was mentioned by Freud (1899), who quotes the following from a letter written in 1788 by the poet–philosopher Schiller:

> *It is harmful for the creative work of the mind if the intelligence inspects too closely the ideas already pouring in, as it were, at the gates. Regarded by itself, an idea may be very trifling or very adventurous, but it perhaps becomes important on account of the one that follows it; perhaps in a certain connection with others, which may seem equally absurd, it is capable of forming a very useful construction. The intelligence cannot judge all these things if it does not hold them steadily enough to see them in connection with others. In the case of a creative mind, however, the intelligence has withdrawn its watchers from the gates, the ideas rush in pell-mell, and it is only then that the great heap is looked over and critically examined.*

It is worthwhile to give each sentence in this quote time to sink in and to ponder the application to research.[1] Some of Schiller's statements might appear to be outrageous for a scientist whose work is thought to be based on logic. Schiller states, for example, that "the intelligence has withdrawn its watchers from the gates," as if intelligence prevents us from being creative. How preposterous! And yet he might be right. We argued in Section 2.3 that, even though

[1] You might consider the implications of this quote for other parts of your life as well.

logic underpins science, the path taken in research often departs from being logical. At times we can be most effective in finding the path to solution by creative thinking driven by intuition rather than by pure logic:

> *...the temporary relinquishing of conscious controls liberates the mind from certain constraints which are necessary to maintain the disciplined routines of thoughts but may become an impediment to the creative leap...*
>
> *Koestler, 1964*

This is not to say that intelligence is not essential to research. Of course it is. In addition to their value in logical pursuits, intelligence and expertise are there to steer free association at a subconscious level.

The process described in the quote above is nowadays referred to as *brainstorming*. While we often think of brainstorming as a group activity, it can actually be highly productive when done individually. In true brainstorming, the ideas come in by free association. Unfortunately, it too often happens that, when brainstorming with a group, the temptation is large to react on each other's ideas in a way that is destructive for advancing the brainstorming process. Comments such as "I don't think that is right" or "that can never be done because..." interrupt the flow of the free associations needed for productive brainstorming in a group.[2] In our experience, brainstorming sessions usually start on the right track with a group process of free association, but, too often, after a while this process becomes stifled by critical comments from within the group. The later into the session to which such comments can be postponed, the more useful will be the brainstorming. This is not to denigrate the value of brainstorming with others. In the best of group brainstorming, ideas abound, but an effective chairperson is needed to monitor or facilitate the process by redirecting the flow of the discussion when participants become reactive.

[2] Such interruptions to the free flow of thought exist not just in groups; they can break into your individual brainstorming as well.

Questions, of course, can arise in settings other than brainstorming sessions of the group or individual. Questions pop up while reading a paper, listening to a seminar, or at random moments during the day or night. To ensure that these questions are not lost, keep pencil and paper handy so that you can write them down immediately. This can also be done in the margin of whatever (of your own) you are reading or on index cards kept close by. It is useful to carry a small "questions" notebook, suitably labeled with a big question mark on the cover. Alternatively, a handheld computer can be used for this purpose, although the authors do not see themselves operating such a device in the middle of the night after a particularly inspiring dream.

5.2 ORDER AND PRIORITIZE QUESTIONS

> *When there is no problem, there is no incentive to think about it or solve it. Only when a problem becomes irritating enough do we tackle its solution.*
>
> Killefer, 1969

The list of questions made by free association and by collecting questions over time basically frames the problem or problems that you face in your research and that prompt you to take action. Usually, embedded in the questions you have formulated is a logical order, perhaps initially hidden. Because research is most effective when its actions are well ordered, once you have made a list of questions about the topic of your research it is therefore appropriate to organize them into some logical arrangement. As mentioned above, this can be done particularly simply by cutting the sheets of questions in pieces with one question per paper slip. These smaller slips of paper can then be moved around the table until they reflect a logical order. Prioritization of the different questions can be accomplished by defining categories for those that are related to one another and then sorting the slips of paper within each category. The shuffling of questions can, of course, also be carried on a computer since text can be placed at will on the screen.

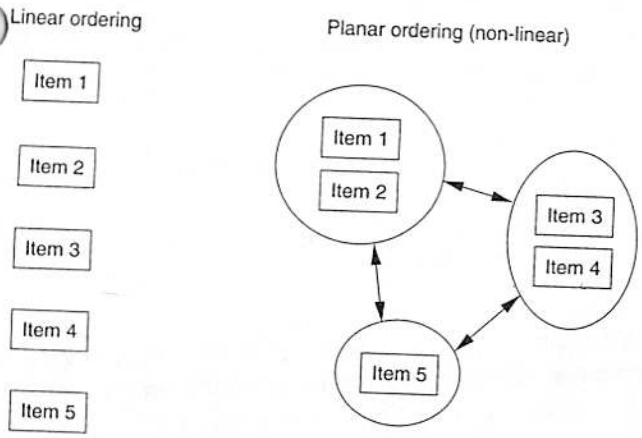

FIG. 5.1. A linear ordering of items (left) versus an ordering of items in a plane (right) that allows for more creativity and flexibility.

Shuffling questions around on a sheet of paper or a computer screen might appear to be a primitive way to organize questions, but it has an important advantage over making a single sequential list. Having items listed simply in sequential order draws the individual into thinking linearly. Such an ordering is shown in the left part of Fig. 5.1. In contrast, shuffling small pieces of paper around over a plane, as shown in the right part of Fig. 5.1, offers freedom to make connections among the different items more readily than with the linear ordering in a conventional list. Once you paste the pieces of paper onto a large sheet of paper, you can use colored markers to draw arrows that show connections among different items, or you can write additional comments on the master sheet of questions that you have created. Alternatively, the drawing tools on a computer can be used to achieve the same results.[3]

You might think that we are describing a merely kindergarten approach to creating order, but consider the method Mendeleev used

[3] The shuffling, either of paper or on the computer, has yet another advantage. Our thought processes do not shut off during the shuffling. Quite the opposite; expect the shuffling process to generate yet more questions.

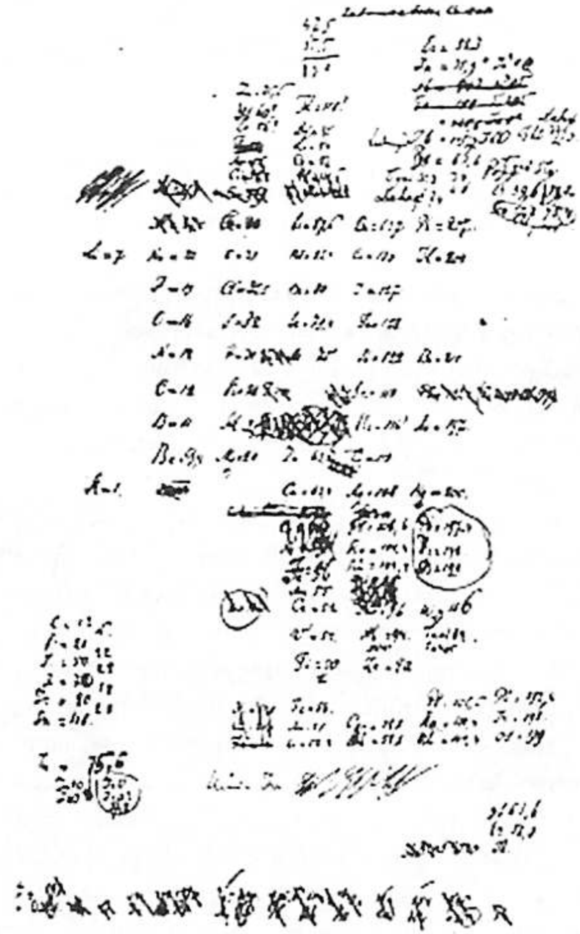

FIG. 5.2. The periodic table of elements as compiled by Mendeleev in 1869. The table is rotated 90 degrees compared to the way it is currently displayed.

in the nineteenth century, which led him to discover the periodic table of elements. An early version of his periodic table is shown in Fig. 5.2. As he describes,[4]

[4] Mendeleev, *Principles of Chemistry*, Vol II, 1905.

I began to look about and write down the elements with their atomic weights and typical properties, analogous elements and like atomic weights on separate cards, and this soon convinced me that the properties of elements are in periodic dependence upon their atomic weights.

By making a card for each element, and by ordering them according to properties of the various elements, he discovered an underlying order to the properties of the elements. Mendeleev accomplished this at a time when about 50 elements that we know today had not yet even been discovered! It is a tribute to his genius (and doggedness) that he perceived the order in the periodic table when so many pieces of the puzzle were not yet known. The early draft of the periodic table in Fig. 5.2, which clearly shows that the intellectual work is still in progress, is a two-dimensional display of his ideas much in a fashion akin to shifting pieces of paper.

The process of generating questions and ordering them is applicable throughout your research career. One of us, together with a graduate student, visited Shell research with the goal of giving shape to a joint research project. We spent the first two days talking about possible research directions, about things we knew and those we did not know. In the process we generated question after question, and wrote down every question and idea that arose in the discussions. On the second evening in our hotel room we borrowed scissors, glue, and a large piece of paper from the surprised hotel staff, and cut our list of questions into pieces with one question on each piece of paper. We ordered these questions into a coherent and logical pattern on a large sheet of paper. The next morning we could show our colleagues at Shell a workplan for the project that needed little modification from their side.[5] Quite likely you might consider it unecessary to revert

[5] This was, in fact, not the end of the story. The student lost the research plan while going through security at the airport in Houston. We could not retrieve the research plan from the security company, but to our surprise we could reconstruct

to such kindergarten steps as cutting, shuffling, pasting, and drawing colored arrows. Give it a try though, to find out if this approach works for you. A variation of it did for Mendeleev.

5.3 TURNING QUESTIONS INTO A WORKPLAN

We've just discussed making a prioritized list of questions that support the primary question of the research project. This list serves a two-fold goal. First, it helps to focus on the issues that you want to investigate. Second, the list can be used to translate those questions into actions. Sometimes it is difficult to decide where to begin when addressing a problem. Your prioritized list, however, can be a valuable aid in deciding the order in which to proceed in carrying out your investigation. This amounts to making a workplan for the research. Such a workplan should not be viewed as a formality for documenting the work to be undertaken. Instead, making a workplan is a process of formulating a strategy for carrying out the research in a systematic way. Developing and following a workplan is critically important when you are working alone, but is even more so when you are working in a group. To use the full potential of the group requires cooperation, dividing of tasks, and coordination of actions. While working in a group poses problems associated with interactions among members, potentially a group can make much more progress than can an individual working alone. Whether you work alone or in a group, the workplan is an important tool for giving focus and direction to research (see also Chapter 6).

A good workplan starts with a clearly defined goal of the project. Write the overall goal at the top of the workplan, and keep it in mind throughout. In a sound workplan, all the actions in the plan ultimately support the goal of the project. Having the plan written down and prominently displayed aids in keeping focus on the project's goal.

the research plan within an hour. Once the research plan had been internalized (i.e., imprinted on our mind – in no small part through the manipulation of the scraps of paper), the physical manifestation of the plan in print diminished in importance.

The task of making a workplan is eased by starting with the ordered list of questions, discussed above, and then translating the questions into actions that could help solve them. When working in a group, selected activities can be assigned to different individuals, or to subgroups. A good workplan contains the following elements:

- An ordered list of activities to be carried out.
- A clear indication of how these activities are interrelated.
- A deliverable and a timeline for each activity.

A deliverable can be, for example, a written or oral report, a set of measurements, or entries for a database of references in literature search. It is essential that the workplan be documented. We have good experience with documenting the workplan in *html* format, the format used for webpages.[6] In this way the workplan can be read on any computer that has a web browser. An additional advantage is that the workplan can also be posted on the internet or intranet – a version of the internet that allows access to only certain users. A *Wiki* is particularly convenient for sharing information with a selected group through the internet. A Wiki is a webpage that can be accessed and edited conveniently through the internet by a designated group of users. The web-based tool to edit a Wiki is user-friendly, and it is easy to set up such webpages.[7]

When working in a group, it is important to hold each other to the workplan and its timeline. Suppose for example that, in a group project, a subgroup is scheduled to report to the full group on a certain issue at the end of the month. It remains a good idea to go ahead with the meeting to discuss the subgroup's part of the project at the scheduled time even if that subgroup otherwise might not feel ready to give its report just then. Useful types of questions that might arise in this circumstance could include the following. What are the factors that cause the subgroup to feel unready to report? Were there unforeseen complications that bear discussion within the full group? When will the subgroup be ready to report? Is it necessary to change the workplan? Even when you are working alone rather than in a group, posing such questions can be a useful exercise because it helps keep your work on target.

While a workplan is a useful tool for keeping focus in research, it is nothing more than a tool to serve the goal of carrying out the research. The workplan itself is not the goal and should never be turned into one. Moreover, the workplan should never be considered as carved in stone. It is in the nature of research that its course can rarely be foreseen in all its details. If it could be, the research probably is not sufficiently innovative. It can therefore be expected that the workplan will need to be modified, perhaps frequently, over the course of the project. Don't be hesitant to adjust the workplan as the project progresses. Again, it is nothing more than a tool to formulate a strategy. Hanging on too rigidly to a single strategy could hamper progress in the research.

Although this chapter opened with an example of questions one could think of about a subject that is outside the individual's field of expertise, for the research that you'll be doing your questions ought not to be arriving totally out of the blue. Rather, they will come from a field of research, even if it is new to you, about which you have become quite familiar. Key to gaining that familiarity is doing a thorough literature search. Such a search is the start of any research project, and it can be worthwhile periodically to repeat the search while carrying out the project. Chapter 9 offers suggestions for aids to making that search effective.

The central message of this chapter is that, whatever background you bring to a problem, *your quality as a researcher depends primarily on your ability to ask the right questions, but that can happen only if you pose lots of questions, many of which will*

6 Numerous editors are available for providing output in html format; hence, there is no need to learn and apply the tiresome rules of this format.

7 For more information see `http://www.wiki.org`.

subsequently be discarded. In case you have doubts about this approach, watch leaders in a research field during a seminar. These usually are the individuals who come up with numbers of questions. Their ability to generate questions, the product of an open mind, is no small factor in what makes them leaders in their field.

6 Giving direction to our work

6.1 SET GOALS

If you don't know where you are going, any road will take you there.

Lewis Carroll, 1832–1898

It's a dream until you write it down; then it's a goal.

Simon, 1998

It's difficult to imagine embarking on a journey, adventure, activity – any endeavor – without having a goal, however vague that goal might be. Even if you don't study a map before going on a road trip, you at least think to put gas in the car. Goals for a holiday might be explicit or implicit, and they can range from short term to intermediate and somewhat long. A career in science, starting from your period in graduate school and continuing into a life of research, is a journey, a long one. Much more so than for a holiday journey, the thoughtful setting of explicit goals is of crucial importance for a successful career in research and for success in the research itself. By *success*, we mean here the achievement of valuable contributions in your field, accomplished with a good deal of pleasure and a minimum of needless pain and time wasted.

Goals give direction to our actions. By clearly choosing and defining goals, we provide a focus for action needed to arrive at a hoped-for destination or outcome. Defining goals not only helps in creating a mental commitment to take certain action, it also enables us to formulate a plan of attack toward reaching the desired ends. The most short term of goals, for example, what you plan to accomplish today, are the easiest to define and pursue. The focus needed for success in graduate school and in research in general, however,

…ires the defining of goals that are of significantly longer term. With well-defined, long-term goals spelled out, you can then proceed to define a sequence of progressively shorter-term ones. To be clear about our meaning, a well-defined goal minimally requires that it be stated in writing.

For good reason, you can expect your goals to change with time. With a sequence of goals kept clearly in mind, however, you can be in control of those changes and avoid being thoughtlessly deviated from your path. Numerous factors can deviate us from our goals or our planned timetable. In research, setbacks are common: experimental equipment might not be cooperative, computers crash, people might try to convince us to choose a different path, and the general level of distraction can be so large that staying focused is difficult. Moreover, the results of our research can fall short of what we might have expected or hoped for. For these and other reasons it is not always easy – and sometimes it is not even desirable – to keep working toward the realization of a defined goal. Nevertheless, without having set well-defined goals, the risk is high that chance events (and, sometimes, other people) will determine where you are headed. It is unlikely that you would be happy with that. That being the case, *you* need to be the one setting, following through on, and, when appropriate, modifying goals for your research and career.

In research it is likewise crucial to define goals. What do you want to discover?[1] What do you want to achieve? Satisfy your scientific curiosity? Publish many papers and advance rapidly in your career? Develop a patent or marketable product? The attainment of such general goals requires that more specific ones be set and addressed beforehand. Your goal could be to find out how a specific biological process works; it might be a desire to make your name by presenting a breakthrough at conference X. Decide on what you *really* want to achieve, and then work hard to reach your goal. This is the only way to maintain control over the direction in which you head.

Appropriate for research is to set a hierarchy of different goals, some of them global and long-term, which require the defining of shorter-term ones that must be satisfied along the way. An example is "I want to develop a career in research with the aim of solving a grand-challenge problem in genetic engineering, and later in my career I want to share the expertise I have acquired with a younger generation." Working toward such a goal requires study in graduate school and the setting of shorter-term goals for graduate studies such as those for course work and research. The satisfying of shorter-term ones amounts to having passed milestones along the road. A milestone can be, for example, the completion of a certain set of experiments, the finishing of a certain amount of course work, or the presentation of a certain piece of research at a scientific conference. In practice, we benefit not only from defining our intermediate goals but also from monitoring our progress over time. Defining the milestones therefore entails not only *what* exactly is achieved, but also *when* each milestone is reached.

Not everybody has clearly defined goals for the longest timescales, and, to be realistic regarding these longer-term ones, we should be alert to "expect the unexpected"; unanticipated events in life often give the realization of long-term goals an unexpected twist.

We find the question "what defines success?" to be particularly useful when defining goals. Answering this question necessitates that we thoughtfully articulate just what is our larger goal and, by inference, what are the milestones along the way toward that goal. The question of what defines success can be addressed for each of these milestones.

In research, one usually proceeds from one phase to the next. A phase in research is usually focused on answering a specific question or range of questions. This could, for example, involve a literature search, the formulation of a research plan, the creation of software, or

[1] In his graduate class "The Art of Science," one of the authors was somewhat astounded to find that so many of his students could not readily complete the sentence, "The goal of my work is…" Likewise, many students had difficulty in completing the sentence, "The questions I'd like to address in my work are…"

the performing of an experiment. Often, it is essential to finish each step, defined by a specific milestone, before moving on. One might need the results of a specific task in order before proceeding to the next. For example, the creation of a workplan depends on a thorough knowledge of the current state of research in the field, so a literature search is necessary in order to learn what is presently known. The design of future experiments likely will depend on the outcome of measurements previously made. In practice, we work toward our goals by realizing a planned sequence of subgoals (milestones). Confusing the order of the milestones can lead to an ineffective line of research. With a well-thought-through sequence, reaching a particular milestone gives confidence to take the next step in research.

One model for defining goals in research is to structure the plans as if preparing a research paper. Such a paper usually begins with an overview of the current state of research and previous work. Next, the research methodology is introduced and then applied to experiment, numerical simulations, or to data that are collected otherwise. This information is then processed and applied toward the research question being asked. Finally, this is all integrated into conclusions and a discussion of the implications of the work done. This generic structure for scientific papers can effectively be used for defining goals and milestones for research. In practice, one does not know ahead of time just which part of the research will be successful, so it is prudent to lay out a number of potential paths that could be followed.

Again, in defining goals, we are choosing where we are headed. Then, by articulating our goals, sharing them with others, and writing them down for ourselves, we build a commitment to work toward realizing them. This commitment then leads to actions that bring us closer to reaching our goals. Note that, in doing so, we follow the same chain of events as discussed in Chapter 5: thoughts (our goals) lead to words (the articulation of goals) and then to actions (aimed at realizing goals). Following this sequence helps us use our creative power to give shape to our future.

6.2 FIVE STEPS TO TAKE IN WORKING TOWARD YOUR GOALS

Intentness is the ability to resist temptation and stay the course, to concentrate on your objective with determination and resolve. Impatience is wanting too much too soon. Intentness does not involve wanting something, intentness involves doing something.

Emery, 2008*

As we have seen in Section 6.1, without having defined your goals, other people and external circumstances – including chance – will decide where you are heading. Defining goals, however, is a different matter from reaching them. A process of translating goals into actions might be the following, adapted from the book of Robbins (1997).

- *Define your goals.* Again, being explicit about your goals, making them sufficiently specific, and writing them down helps you create a strong commitment to them so that they can be translated into actions. Remember the question "what defines success for my project?" Answering this question often leads to a more clearly specified, and sometimes more measurable, endpoint of the chosen goals, which can help you work toward their realization. Note also that having a timeline will be particularly helpful once you've defined your goals.
- *Decide if you are willing to pay the price.* Achieving something large in life rarely comes for free. Just as becoming a great athlete requires training, a shining academic career requires dedicated study to learn both your chosen field and the professional skills needed to be successful. This will entail hard work and, at times, some sacrifice. You should make a conscious choice as to whether or not you are willing to follow through on your larger goals. To excel in research you must be willing to – that is, make the conscious choice to – put in the energy necessary to learn new things, follow the scientific literature, establish contacts in the scientific community, take steps

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foster your creativity, and have the courage to be truly innovative. A mismatch between your goals and your willingness to invest in them is a recipe for disappointment. Your choice can work out either way, *but choose!*

- *Define a strategy.* Once you have decided that you truly want to realize your goals, define with care your strategy for achieving them, for planning steps to take, and for defining milestones along the way toward the goal – and don't forget to include a timeline. Again, put your strategy on paper in order to make it more tangible. Then start executing the plan.
- *Evaluate your strategy.* While executing your plan, you will probably discover that some portions work out well, and others don't. This is normal; nobody is perfect, and no plan is fool-proof. This is aggravated by the fact that "in the beginning we don't know what we don't know." Our initial ignorance often leads to a strategy that needs adjusting. Therefore, periodically evaluate your strategy by identifying its weak and strong points, assessing what works and what does not.
- *Modify your strategy.* Adjust your strategy based on evaluation of which of your actions have been effective and which have not. Likely you will go through cycles of re-evaluating and modifying your strategy, making changes to the execution of your plan.

Neither setting nor following through on goals is a trivial matter, but realizing that you need to modify your goals and strategy, and having the discipline to define and act on the changes, can be the hardest part. These steps are applicable to any type of large goal in life, including, of course, your research goals. You might find it instructive to re-read the above sequence of steps with your research project in mind.

6.3 WHAT IS YOUR GREATEST RESOURCE?

In following the above steps of defining your strategy, evaluating your plan, and adapting the strategy, you are not alone. You are surrounded by people who can help in a variety of ways. A good way to use the experience of others is simply to observe what they are doing. Suppose you are struggling with an academic course you are taking, and this course appears to be much easier for a friend. Much as your inclination might be to trudge through the material by yourself, it can be worthwhile to see how your friend learns the course material and to compare her approach with your own strategy. It could simply be that your friend learns certain types of material more easily, but often it could be that her study habits are more effective than yours. But, you can do much more than just observe. Why not ask colleagues how they approach subjects or problems, and even see if you can study alongside them? Most people are willing to help and share their expertise; certainly that is true of friends.

Consider two basic ways of learning. The first is to learn by encountering your own mistakes and then figuring out how to correct them. The second is to imitate the methods of others. Although both approaches to learning, or a combination of both, work well, the second one, emulating others, can be a much more efficient investment of your time. The essential point here is that you are likely to be surrounded by a wealth of expertise and possibilities for getting help. *The people around you can be your most valuable resource.* Most people are flattered for you to have sought their advice, including for help in defining, evaluating, and adapting your strategy on the basis of the evaluation.

A corollary of the principle that your colleagues can be your greatest resource is that you can be a great resource for them as well. Not only do you learn from others, but others learn from you, often without actually being aware of your being the teacher. We learn much from simply observing others; likewise we often influence others through just our words or actions.[2] Whether casually through our actions or explicitly through study together, a network of colleagues

[2] This is one reason that "leading by example" is such a powerful tool in management. Whether we are aware of it or not, we are being observed by those around us. A discrepancy between the words and actions of a leader rapidly undermines his or her credibility.