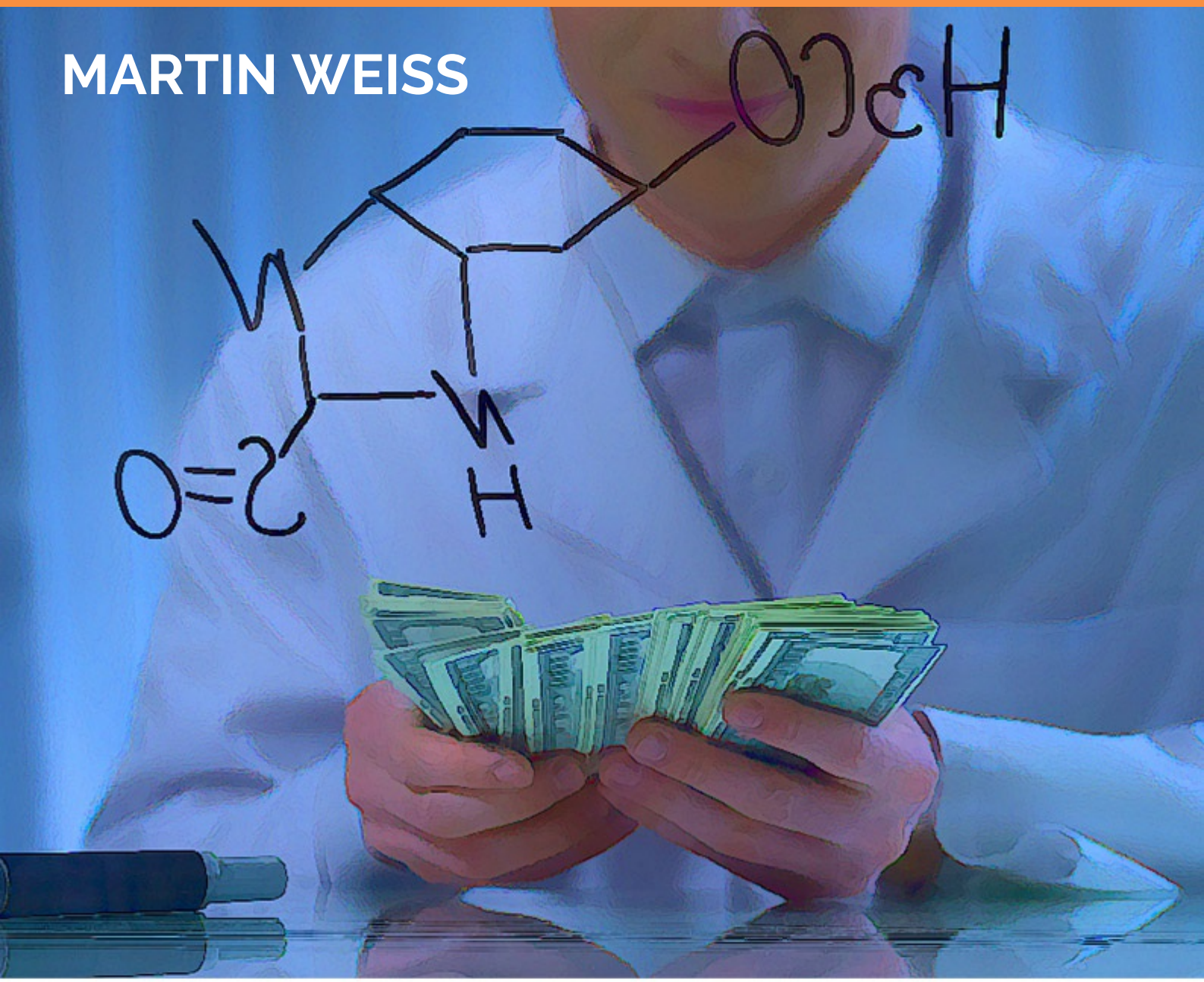
MARTIN WEISS



Writing Scientific Research Proposals

A Practical Guide

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MARTIN WEISS

WRITING SCIENTIFIC RESEARCH PROPOSALS

A PRACTICAL GUIDE

Writing Scientific Research Proposals: A Practical Guide
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CONTENTS

ABOUT THE AUTHOR

Martin Weiss graduated in Electrical Engineering (specialized in Automatic Control) at the Bucharest Institute of Technology in Bucharest, Romania in 1990 and received a Ph.D. in Applied Mathematics at the University of Groningen, The Netherlands in 1994. After spending six years in academia in the Netherlands (Eindhoven and Groningen), he moved to the applied research world as a research scientist at the TNO Organization (The Hague, Netherlands). Since 2013, he works as a senior research scientist and lecturer at the Aerospace Engineering Faculty of the Israel Institute of Technology, Israel, Haifa.

He has authored or co-authored a research monography, about fifty journal contributions in international reviewed journals, many conference contributions and two patent applications in different areas of systems and control, guidance, navigation and process control. His current work is supported by various scientific projects financed by national and international institutions specialized in scientific research, for which he wrote or co-wrote the proposals. This book is based on his experience gained as a research scientist both outside and inside the academia, and also on his experience as a reviewer for a European scientific research program. The dual perspective, from both sides of the fence, will hopefully make this book most useful for the young scientist looking for a future successful career.

Contact information:

For questions, suggestions, comments about the book, please email weimarconsult@gmail.com

Additional material and resources related to this book will be posted at https://weimarconsult.wixsite.com/scipropguide. You can also subscribe and comment on the site.

FOREWORD

First of all, thank you for your interest in this book. It is written in the hope that the time spent to read it will prove worthwhile, as you will spend less time on writing research proposals and greatly increase your success rate.

Research proposals are the most important way for securing the financial means for scientific research, but writing research proposals is one of the least favorite activities of scientists. Nobody chooses a scientific career in order to write proposals, but to discover hidden truths and report them in scientific publications. However, failing to write good proposals means failing to attract the resources needed to realize potential scientific talent. So, why not trying to improve this important skill?

The purpose of this booklet is to help young scientists approach this activity, often regarded as rote, as a necessary evil, with a different, more positive, attitude. I have more than twenty five years of experience in scientific research, so I was necessarily confronted with the need to write proposals during most of my career. Of course, I tried very hard to avoid it as often as I could. However, in the end, I noticed that it is better to get good at it rather than dodging the unavoidable. Of course, I tried to learn from different sources about writing better proposals: books, presentations, courses. Often I learnt from successful proposals written by others. However, I learnt most about writing scientific proposals when I took part in the proposal evaluation process of a national institution for financing applied scientific research. It is in this position, on the other side of the fence, that I started to realize how a proposal is received by evaluators, what a proposal should have more of, and what it should have less of, or even miss altogether. Most importantly, I learned that a proposal has one mission: to convince evaluators that a piece of future work could be useful, is opportune and has a good chance of successful completion. Anything that does not contribute to make these arguments should not be part of the proposal. As an evaluator, I could understand what elements help me evaluate the utility, opportunity and feasibility of a proposal. I could see what is convincing and what raises doubts about the proposal. It is my experience as an evaluator that increased tremendously the success rate of the proposals that I wrote ever since. Unfortunately, a young scientist cannot achieve this experience before getting some seniority, and this will not happen before writing many research proposals. This booklet has one aim: to help young scientists overcome this temporary handicap by formulating practical tips that are easy to follow. Here is the first one: *try to participate in the evaluation of proposals as early as possible in order to become better at writing proposals!* Learning from the errors of others is always better than learning from your own errors.

Throughout this text I tried to follow as well as I could, the classical principle abbreviated KISS (Keep It Short and Simple!). Scientists need to develop good science and should concentrate on developing their field of research. Complicated techniques for writing research proposals are to be avoided, so reading involved texts on the subject should be avoided too. The main intellectual effort needs to be concentrated on the substance of the research. A text on writing proposals should offer a few principles easy to understand and follow with the purpose of helping organize and direct the thinking process.

Finally, let me wish you **good luck** with the proposals that you will submit and let me hope that this book will help you not only to avoid mistakes, but especially to encounter success.

1 INTRODUCTION, OR TO WHOM IT MIGHT CONCERN

Your learning goals for this chapter:

- *Types of readers for which this book is intended.*
- *The structure of the book.*
- *How to read the book.*

Ever since starting his college studies, John had the time of his life. He worked hard, but he enjoyed his studies, and his efforts and motivation were reflected in his results. He was about to graduate among the top ten of his colleagues in just a few months, so it was time to think about the next step in his life. Graduate studies, especially at a prestigious center, are the path to a better position on the job market and definitely a good option to consider in his situation. As always, there is an obstacle to be passed if he is to make use of such an opportunity. Besides the unavoidable resume (or C.V., as some call it) which would hardly be a very thick piece at this young age, most admission procedures ask for a personal statement, or even a research plan before being allowed to start as a graduate student. John has never thought about anything like this before. He was good at answering the questions at exams, solving the technical problems posed during the projects he participated in, but writing his own questions, and formulating his own projects is a different matter altogether.

Caroline is about to finish her PhD thesis. It was a great time, she learned a lot and even discovered things that were interesting enough to get published and communicated to peers at conferences. Scientific research has always attracted her, and after her PhD experience, a future scientific career looks even more attractive than before. The typical path for the future scientist she wants to become consists of one or two postdoc positions before getting a tenure-track position that would allow her to continue in an academic career. In case of an applied science branch, as the one she is pursuing, a few years in an industrial laboratory or institute could be a valuable part in her professional development, but chances are scarce at the moment, so a postdoc position looks as the more promising possibility. There are a few places where she wants to apply, but there is one requirement that most of them have listed in their admission procedure: a research proposal for one or two years. Caroline had written some years ago a proposal for her PhD research, as this was part of her admission procedure as a PhD student. However, that was a very loose proposal, for four years of her own work. The work as a postdoc should ideally involve the supervision of graduate students, as well as collaboration with peers. The fact that the work is mostly limited to one or two years means that the work plan has to be more concrete. How can she convince the admission committee that she is the best suitable for the postdoc position that she wants?

Terry was just accepted as an assistant professor, a tenure track position. He loves his work and is very happy that he can start his academic career at a prestigious academic institution. Next to his teaching tasks, that take a lot of his time, he has to develop a research program based on contract research. He has to submit many proposals for research projects to national and international institutions that finance the kind of research he is interested in. He needs his money to cover some of his expenses, but in the first place to finance the work of graduate students and postdocs that would work under his supervision to help him advance his ideas. Some of these projects have to be performed in collaborations with colleagues from other institutions. The capacity of performing collaborative research will be well appreciated by the committee that has to approve his tenure over a few years.

This book is written for people like John, Caroline and Terry. People that started or are about to start scientific careers. They know that they possess the skills and talent for pursuing scientific research. The challenge is to convince other people to allocate financial and non-financial resources to help them achieve their goal. There are famous examples in the history of science when important contributions were not recognized by contemporaries and had to wait for many years to gain deserved recognition. The scientist that writes a proposal faces the problem of obtaining recognition even before the work has started. Especially for a scientist, educated in the spirit of believing only what is proved to be true, the task of making somebody believe that something can be accomplished before even starting the work looks very daunting indeed. And still, the success in a scientific career is determined to a large extent by the ability to attract research money from research financing institutions. Managing the skills necessary to write successful research proposals is an important asset for the starting scientist and this book is intended to help and improve these skills.

In keeping with one of the recommendations that this book contains, you will find this book structured around three (types of) questions:

- The "**why**" question: *why write a proposal* in the first place is addressed in the first chapter.
- The "**what**" question: *what should a proposal contain*, is addressed in the second chapter.
- The "**how**" question: *how to write a proposal*, actually various aspects of this question, is tackled in the chapters 4-6.

In addition, the book contains a final chapter that gives some clues for finding additional information, mainly on the Internet, beyond the scope of the book. Few concrete links or references are included since, on one hand, it is very easy nowadays to find available information if one knows what to look for, and, on the other hand, current links and references may become quickly obsolete.

How to read this book

It depends on what you want from it, but I will make my recommendations based on the three heroes that I introduced at the beginning of this chapter.

John, the graduate student, will probably be satisfied reading thoroughly Chapters 1, 2 and 3. A research proposal at this level is more of an outline of a proposal in the sense explained in Chapter 3. From the rest of the book, I recommend Sections 5.2, 5.3 and 5.4 as well as Section 7.1.

Caroline, the Ph.D. student, will have to read most of the book, but skim the parts related to effort planning, budgeting, etc. Terry will obviously have to go over the entire text and may even need to deepen some aspects. I definitely hope it will be worth the effort for all of them.

2 WHY WRITE A SCIENTIFIC RESEARCH PROPOSAL

Your learning goals for this chapter:

- *What do you try to achieve when writing a research proposal*

The most typical answer that someone may give to this question is probably "***Because I have to***" with the closely related, and almost equivalent "***Because I need to***". This is obviously a shallow answer and it is patently incorrect. Scientific research does not appear on any list of essential items for a human being that I am aware of. However, for somebody who has chosen scientific research or academia as a career, there is a certain feeling of inevitability, of "necessary evil", when it comes to writing proposals. As in each way of life, a career in scientific research has its high moments, those that motivated the choice for the career, and it has its down times, when more or less important, but boring or uninspiring tasks have to be completed in order to make possible the good stuff to happen. The answer "***Because I have/need to***" is just another way of saying that writing research proposals falls in the same category, albeit higher in importance and complexity, with putting the desk in order, completing travel expense declarations, and similar tasks. To be sure, doing scientific research should be the strongest motivation for writing a research proposal. After all:

> *"Scientific research is one of the most exciting and rewarding of occupations."*
>
> – Frederic Sanger (1918-2013), twice Nobel Prize winner in Chemistry

There is another serious reason why the "***have/need to***" answer deserves no attention: it is not helpful. Understanding why a proposal is written should help identify what a proposal should contain in order to be effective. Therefore, another answer is needed.

A better answer in this respect is "**Because I need resources to do some important piece of work**". This answer touches the essence of what a scientific research proposal is about: asking resources for a piece of work that will be done in the future. However, it misses another important aspect that a proposal needs to cover. A more complete answer is "**Because I have to convince other people that my work is important enough to allocate the necessary resources**".

Indeed, the scientific proposal is in the first place an argument to convince somebody to allocate resources for a future effort. This is true for every type of proposal. From a graduate student submitting a plan for his research to an admission committee, up to and including the international consortium of academic centers and industry research laboratories that submit a cooperation project to a multinational scientific financing organization, the aim is to convince one or several individuals that it is worthwhile to allocate resources (time and/or money) towards a future goal. The quantity of requested resources may vary widely from one case to another, the "burden of proof" is widely different, of course, from one case to the other but the essence remains the same. Consequently, a scientific research proposal has two primary functions:

- to **inform** about the planned work, its expected results and the required effort;
- to **convince** decision makers that the work deserves to be pursued.

These two complementary functions of the research proposals determine the two types of items that the proposal should contain: **descriptions** to inform, and **arguments** to convince. In the next chapter we describe what constitutive elements are needed in order to put together a research proposal.

3 WHAT IS A SCIENTIFIC RESEARCH PROPOSAL (AND WHAT IT SHOULD CONTAIN)

Your learning goals for this chapter:

- *The four essential elements of a scientific research proposal, their goals and their specific contents*
- *The challenges of communicating through a scientific research proposal*
- *Balancing planning and creativity in scientific research*

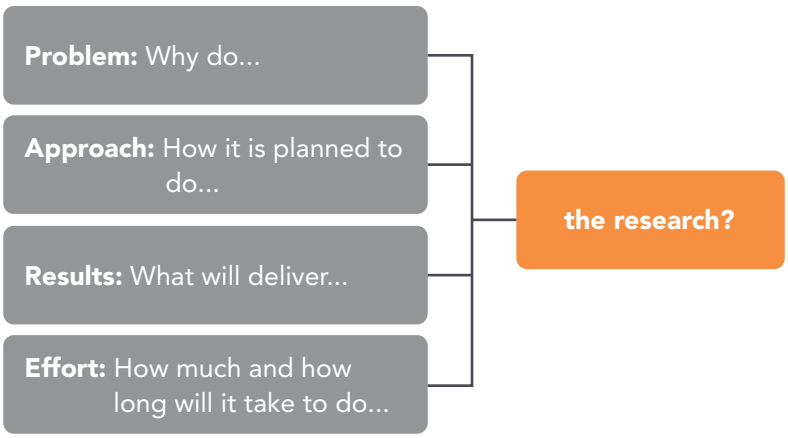
There are two possible points of view to consider when answering this question. The first concentrates on the inner structure of the proposal, its content. The second point of view concentrates on the outer effect of the proposal. It should be obvious that a good proposal has to balance high quality content with clear communication. In this chapter, we answer the "what" question about scientific research proposals from each of these two points of view.

3.1 WHAT SHOULD A SCIENTIFIC RESEARCH PROPOSAL CONTAIN

In essence a scientific research proposal is a plan for a piece of scientific work that is written with the aim of obtaining the means to conduct the proposed work. This section defines the ingredients that each research proposal needs to contain in order to achieve its goal.

There are four elements that constitute the content of each research proposal: **the problem** (that the research is addressing), **the approach** (how the problem is supposed to be tackled), the **expected results** (what will the proposal deliver if successful), and the required **effort** (what will it take to complete the research). Templates provided by various research-financing institutions may use slightly different terminologies for these constitutive elements, but the essence remains the same.

An overview of the four constitutive elements and the specific question that each of them answers is presented in the next figure.



These four items should be present in each proposal and should be made as explicit and as clear as possible, but before making them clear to others they should be clear to the ones that submit the proposal. To help in this endeavor, each of them will be carefully defined. Moreover, throughout the book, you will find methods and tips that can be readily used to organize the thought around these items in each of the stages that the proposal will undergo. There is a fifth item that is sometimes explicitly requested by the funding institution and that is often misunderstood, therefore misreported in many proposals: the **risk assessment**. An entire section is devoted to this item in Chapter 5, so for the moment, the discussion will be limited to the four items listed above.

As explained in the previous chapter, a scientific research proposal has two functions: to **inform** and to **convince**. In other words, the main questions that the proposal has to answer at each level are the "what" question, to inform, and the "why" question, to convince.

Consequently, the scientific proposal has to answer the following generic questions:

- What problem/challenge/issue is tackled by the proposed research?
- Why is the problem important?
- What approach is chosen to tackle the problem?
- Why the proposed approach has significant chances to solve the problem?
- What results will the research deliver if successful?
- Why will the results of this research contribute to the solution of the problem?
- What resources (time, money, people, installations) are necessary to complete the work described before?
- Why are the requested resources sufficient in order to successfully carry out the proposed work?

Let us define now each of the four constitutive elements (problem, approach, effort) of a proposal and indicate their respective role and importance within the proposal.

3.1.1 THE PROBLEM

This is the main question that the proposed work is aimed at answering. It is a good practice to formulate *a single* research problem in the proposal that is given the main attention. It is possible to detail the problem into a number of partial problems, which may be interesting in their own right. It is appropriate to indicate that a successful completion of the proposed work may contribute substantially to progress in solving other outstanding problems, but it is a great advantage in formulating one single central research question. It should be clearly formulated and its importance should be carefully argued even if it may appear to be obvious. Ultimately, if this part of the proposal is not convincing, the proposal is doomed. There is nothing else that can save a proposal if it does not articulate clearly an important problem that is aimed for. If the aim is not important enough, there is no point to allocate resources in order to reach it.

3.1.2 THE APPROACH

This part of the proposal is less important than the research problem. Unfortunately, many spend an inordinate amount of effort on this part of the proposal. The reason has probably to do with the fact that this part is the closest to the motivation of the scientist. It is about developing and applying new theories or techniques, designing experiments, gathering data and analyzing it. Most proposals are written by people that are passionate for and experienced in one or another approach, a theory, or a technique. Therefore many proposals are very informative about the chosen approach. As long as the description of

the approach demonstrates that it is reasonable that it can achieve a solution to the stated problem and that the proposing team is comfortable with the expertise that is required to pursue the chosen approach, there is a lot of value in this part of the proposal. Beyond convincing the evaluators of the feasibility of the proposed work, no amount of detail will improve the chances of the proposal.

There is another aspect that is often forgotten by the passionate scientist. The approach described in a proposal is only a possibility for attacking the problem. If another approach is deemed more promising at the time that the work has started, the approach could and should be changed. It is important to have a good approach at the beginning of the work, but it should not be regarded as a definitive choice. Understanding that the proposed approach is only a possibility to be considered at execution time has two advantages for setting up the proposal. First of all, it eliminates the necessity of describing the approach in too minute details. Second, it avoids the unnecessary limitation of scientific creativity.

3.1.3 THE EXPECTED RESULTS

This is an important element of the proposal that receives often relatively less attention than the approach. Ultimately, the research is conducted, effort and resources are spent, in the hope of obtaining results. The results should obviously represent a solution, or at least a significant advance towards a solution, for the research problem that was stated in the proposal. A common confusion that creeps in many proposals is between the *results of a research* and the dissemination means. To be sure, a report, a journal publication, or a patent, even a book, or a Ph.D thesis are channels for communicating the results of scientific research, not the results of the research. A winery may specify its result in number of bottles, but only because everybody understands what the delivered bottles will contain. In the case of scientific research, the content of what will be delivered needs to be specified in more details. A new algorithm, a new production method, or a new insight into a particular phenomenon are typical results of scientific research that may be communicated through a variety of means such as those mentioned before.

Still, the proposal could, and sometimes has to, mention the plans for dissemination of the results. In this case, it is appropriate to mention how many articles, reports, patents are planned to be published. In any case, describing the expected results should take priority, and it should be clear how these results relate to the research problem.

3.1.4 THE EFFORT

This part of the proposal is second in importance to the problem formulation, but it is very important nevertheless. It consists of three elements: financial, material and human. The most important of these elements is the human part. This part specifies the people that are supposed to participate in the effort, and the proposal should make clear that the expertise necessary to pursue the work is present in the team. This is true even if the team consists of a single member, of course. The other elements should be filled in appropriately, but most attention should be given to the people that will participate in the work and to the reasons why there is a good chance that they will complete the work successfully.

To summarize this section, Table 1 gives a synthetic overview of the four constitutive items of the proposal and the role that information and conviction should play in each case.

The item	Informs	Convinces
Problem	Formulation	Importance
Approach	Description	Feasibility
Results	Enumeration	Usefulness
Effort	Planning and evaluation	Effectiveness and Efficiency

Table 1. A synopsis of the content of a scientific proposal.

3.2 THE SCIENTIFIC RESEARCH PROPOSAL AS A COMMUNICATION CHANNEL

In my experience, it is this aspect of writing a research proposal that is most challenging for young and even for experienced scientists. The primary reason is not lack of communication skills, although this could be the case sometimes. Unfortunately, the difficulty lies deeper than a mere individual lack of skill.

If we look at a research proposal as a communication channel, it is important to identify two essential elements: the transmitter and the receiver(s). In case of a research proposal, the transmitter is the proposal team, or at least its leader or coordinator. The receiver is a little trickier to define. Primarily, the receiver is the decision maker that can be individual, or collective, as in an evaluation commission. However, typically a commission relies on anonymous reviewers that do the hard work, usually poorly paid, to read in detail the proposal and review it. An effective proposal should be addressed to the reviewers. Unfortunately, hidden as they are in the cloak of anonymity, it is very hard to anticipate how they will receive the message. This is, in my opinion, the greatest challenge for setting up a proposal and it is most challenging for the young scientist that has never seen the process from the other side, as a reviewer.

To understand the challenge of communicating through a research proposal, let us have a look at other forms of communication. The simplest form of communication is talking to a friend at a table. You know the recipient, and you can gauge the reaction in "real time" by sight and sound. Disagreements are possible, but misunderstandings are rare and they are easy to correct. Talking to a friend over the phone is a little harder. You lose the sight, but you still have the sound. You know the receiver well, and the friend will tell you if something did not come over. Still misunderstandings may occur sometimes and may take longer to detect and correct. When talking to a person that you do not know, misunderstandings are even more probable. You do not know the recipient, but by gauging the reactions in real

time, it may still be possible to guess how your message arrived. It takes more attention, and more experience, but it is generally not hard to detect and correct misunderstandings. Written communication is one step further on the difficulty scale as you lose the "real time" element of the interaction. Sometimes misunderstandings arise even between old friends. The written word is easier to misinterpret because it is much harder to include intentions and emotions into a written message than into the spoken word. The modern use of emoticons and emojis has been widely adopted in order to partially circumvent this difficulty, but for the time being their application is strictly limited to short informal digital communication. Extending their use to scientific research proposals is not socially accepted at the time of this writing so I strongly discourage it.

In this scheme of things, a scientific research proposal is one of the hardest forms of communication. You do not know to whom you are communicating, you do not know if your message arrived and how it was received. Reviewers are generally experts in their fields of expertise, but these may not coincide exactly with the field of your proposal. Due to the increasing specialization of science and technology, there is a significant chance that your proposal will be judged by one or several reviewers that are not intimately familiar with your field of research. What to do about it? Here are some tips:

1. Make sure to specify as clearly as possible to which domain the proposal belongs. If possible, try to specify the related domains to which the proposal DOES NOT belong. This will increase the chance that the right reviewers will see your proposals, and also warn reviewers about the background.
2. Avoid terminology that is too specialized. It is better to err on the safe side.
3. Avoid abbreviations. The limitations of our alphabet, and the multitude of specializations, led to the current situation that many abbreviations are used by different communities with entirely different meanings. Avoiding them is an easy way to avoid unnecessary confusions. Remember also that abbreviations make a text harder to read.

To be sure, there is no guarantee that your message will be received correctly, but keeping in mind not only what you want to communicate, but also to whom, should improve the effectiveness of your message

3.3 THE FINE BALANCE BETWEEN PLANNING AND CREATIVITY

One of the aspects often difficult to grasp for the beginning scientist is the contradiction between the creative character of scientific research and the planning required when writing a proposal. Great discoveries are often presented as the result of more or less random observations made by brilliant minds. An apple that falls from the tree is noticed by Newton and this is how the theory of gravity is discovered, to cite one of the most famous examples, as it is often described in popular materials. Planning scientific research seems slightly paradoxical. After all, there are famous people that seem to contradict the very possibility of doing this in any meaningful manner.

> *"If we knew what it was we were doing, it would not be called research, would it?"*
>
> – Albert Einstein

Notwithstanding this or other similar quotes, the reality is that, just as setting records and winning cups or medals is only a small part of pursuing a sporting career, brilliant observations that lead to revolutionary theories are only a very small part of pursuing a scientific career. Actually, a spontaneous observation will lead to a greater or smaller discovery only if supported by a good preparation of the mind and of the "eye". The scientist has to look in the right direction in order to notice a significant fact. Scientific proposals are plans to tackle certain problems, to look in certain ways at these problems and to try possible approaches to solve them. Brilliant insights or ideas cannot be planned but they are only possible on a good basis of scientific enquiry. Furthermore these insights or ideas can only

be developed into useful methods, processes or useful products, as a result of serious research and development work that tests and refines the initial ideas in order to bring them closer to the point where they can be useful beyond the scientific laboratory. This kind of "down to Earth" work can be planned and it is this kind of work that is represented in the great majority of proposals of applied scientific research.

Another point related to the planning of scientific research that is often overlooked is what the plan should be about. To be sure, a scientific research plan cannot be a list of steps that lead to a scientific breakthrough. It is rather a list of objectives that, if followed, is *likely* to deliver a certain desirable result. As explained before, the proposal has to convince the evaluators that the objectives are important and reachable. Finding the exact way to reach the objectives should be left for the execution time as this is the right moment for unleashing scientific creativity. This is the ultimate solution for the apparent contradiction between creativity and planning in scientific research and it applies equally to applied research as to fundamental research. It is also the reason why the problem and the expected results of the research are relatively more important elements of the proposal, than the approach.

4 THE DIFFICULT START

Your learning goals for this chapter:

- *Preparing the writing process*
- *The three steps of writing a proposal*
- *The first step: the outline*

A good strategy in front of a difficult and complicated task is to divide it into smaller steps or subtasks. No matter how this divide-and-conquer strategy is applied, there is always a difficult step to take: the first. This chapter will try to help you cross quickly the threshold of starting to write your proposal.

4.1 THE PREPARATION

It should be obvious that before writing a proposal, the requirements of the funding institutions should be carefully studied. Modern research funding organizations provide templates for the proposals and these should obviously be downloaded and used. But is there anything else that can be done even before starting to write the proposal?

We are familiar with the traditional wisdom about learning from errors. The higher form of wisdom recommends learning from the errors of others. There is an even higher form of wisdom that recommends learning from successes of others. Indeed, there are many ways of failing and avoiding one of them is no guarantee of avoiding them all. Therefore, the most effective way to prepare for writing a proposal is to study **another proposal** that was **ACCEPTED** in the past by the same institution. In case such a proposal is not available, the second best is another proposal that was rejected by the same institution. This is only useful if there is usable feedback from the reviewers to try avoiding the errors that failed the "model".

Clearly, there are different ways to learn from each type of old proposal. A successful proposal may provide information, or suggests solutions for the following aspects:

- the organization of the proposal;
- the level of detail in the technical presentation of the proposed work;
- the proportion between theoretical and applied work that is expected by the financing institution.

As said, a failed proposal is only useful in so far as the reasons for the failure are clear. This is true in case the reports of the reviewers are available, in which case it is important to find out:

- What did the reviewers find acceptable among the aspects listed above?
- Where did the reviewers have objections about the same aspects?

4.2 THE THREE GENERIC STAGES OF A PROPOSAL

As mentioned already, it is a good idea to divide the process of writing the proposal into several phases. This makes it easier to plan the setup of the proposal, and a good plan is actually the very start of the process. This section offers a generic planning for writing a proposal, one that everybody may and should adapt to the concrete situation.

Essentially, the generic stages that the proposal should go through are:

1. **Outlining**: A good proposal needs a good outline that is a good general plan. A few major choices have to be done at this stage and the general structure of the proposal has to be fixed. This step may be easier to perform if the financing institution for which the proposal is written has a fixed structure for the proposals, e.g. a template. It is important of this stage to decide what goes in and, as important, what stays out of the proposal in terms of aims and high level research questions. The result of this step is an outline of the proposal that should normally be discussed only within the circle of those involved in writing the proposal. A detailed description of the decisions that need to be taken at this stage is given in the next section.
2. **Drafting**: The meat of the proposal is produced at this step, following as closely as possible the outline. If things do not go smoothly at this stage, it could be that the planning was deficient. The result of this phase is the first version of the proposal, which can and should be open to evaluation and criticism from people not directly involved in writing the proposal.
3. **Fine tuning**: With the first version available, it is possible to compare the aims of the proposal with what the proposal really contains. This is obviously the phase in which external feedback received on the draft proposal should be processed and incorporated in the final version. Last but not least, attention needs to be given to the "soft" aspects of the proposal such as language, layout, etc. before submission.

The rest of this chapter will deal with outlining the proposal. The other two stages will be dealt with in Chapter 5, and respectively in Chapter 6.

4.3 THE PROPOSAL OUTLINE

Actually, writing an outline is only important if the proposal is a collective effort. If the proposal is individual, it is enough to make a rough scheme of the proposal outline. In any case, there are a number of specific choices that have to be made during outlining regarding each of the four constitutive elements of the proposal: research problem, approach, expected results and required effort. In the rest of this section, we discuss these decisions for each element separately.

4.3.1 THE RESEARCH PROBLEM

The choice of the central research problem is the most important decision to make in the outlining phase. This decision establishes the **Scope** of the proposal. Therefore, this element will receive the most attention here, whereas the other elements will be treated in more detail in the next chapter. As explained before, each proposal should preferably be devoted to **a single** central research problem.

Although it is not compulsory, it is a good idea to link the central research question to a **high level problem** that (almost) everybody can relate to, even outside the specialist field of the proposal, and that represents the larger frame which the proposal is a part of. If

the proposal is submitted for a large research program, the high level problem has to be chosen from the objectives, or themes of the program that can be found within the call for proposal documents. Most funding institutions have such themes and they should be consulted in order to choose the high level problem that the proposal will tackle. The high level problem is typically not a readily reachable goal. In fact, it could actually be a continuous aspiration. Curing cancer, improving pollution levels in cities, decreasing energy consumption of transportation systems are a few typical examples of high level problems.

The central **research problem** has to be an achievable goal that, if realized, would advance the solution to the high level problem. If the high level problem is curing cancer, then a possible research problem may be about finding means of early diagnosis of cancer using a particular class of tests. Choosing a high level problem helps to motivate the importance of the central research problem and thus the importance of the proposed work. Since the importance of the high level problem is evident and unquestionable, the proposal needs only to motivate how solving the research problem would advance the solution of the high level problem. To continue the example, the proposal needs to show how early diagnosis contributes to the improvement of the chances of cancer survival using, for example, statistical data or previous studies.

Although the formulation of the central research problem may address various issues that we will discuss in the sequel, there is a single criterion that should be kept in mind when the main research problem is chosen and formulated:

- **Relevance**: The most important quality of a good research problem is relevance. It goes without saying that irrelevant problems make little chance for support. Nobody will put resources financial, or otherwise, to find answers for irrelevant questions. Neither should you. If you have difficulties in arguing the relevance of a question, just skip it. In any case, every research problem needs to be formulated in a way that demonstrates maximum of relevance.

Merely as a help, here are a few arguments for relevance that can be tailored to apply for every specific proposal:

- **Cost savings**: When arguing relevance it is important to try to be as concrete as possible. An estimate of the possible money savings if the problem tackled in the research would be solved is a good way to argue relevance of research in a very concrete fashion.
- **Time savings**: Time is money, so the savings can be expressed in time in some cases, when time savings are easier to evaluate than cost savings.

- **Number** of people, enterprises, countries **that face the problem** that could be solved: it may be hard to evaluate concrete benefits in certain cases, but the relevance of a problem can be argued by the number of people or groups of people that are affected by the stated problem.
- **Amount of prior work** to tackle the problem: The fact that many people are tackling a problem is definitely an argument for relevance, and it should be made in the proposal. A good presentation of the "prior art" should be done anyway in order to demonstrate a good mastery of the domain of the proposal. However, as an argument for relevance, the amount of prior work may cut both ways. If many people have considered the same problem, it is necessary to argue why a new effort would be necessary and beneficial. A tempting solution, often used by many scientists, is to justify the new effort based on the chosen approach, but it is a weak argument unless the chosen approach is very innovative and it was not possible to be applied before to the same problem. A better solution is to formulate a slightly different research question than those considered before. Take another point of view, rather than another approach. It is an easier choice to justify.

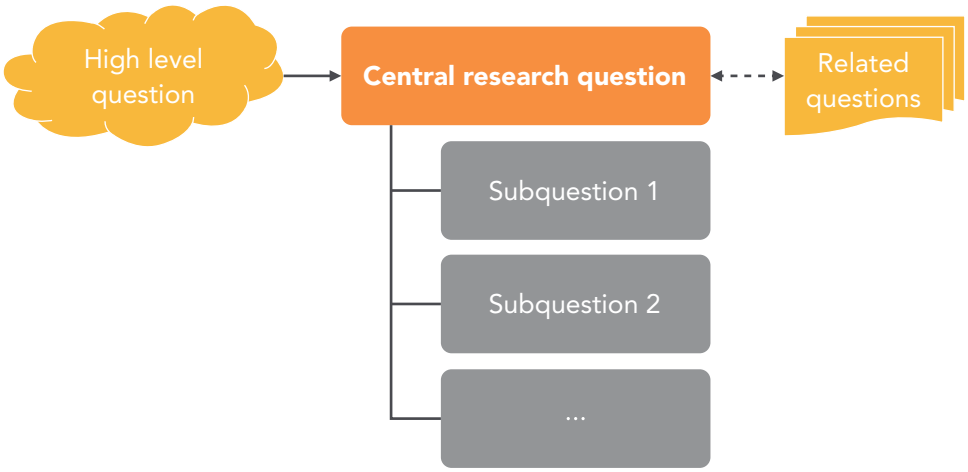
Besides the relevance argument, the formulation of the central research problem should take into account the following aspects:

- **Balance abstract/concrete**: It is a common mistake to formulate the research problem as general as possible, probably due to the impression that solving general questions would be more praiseworthy or would appeal to a larger public. Unfortunately, abstract questions are harder to motivate convincingly. Choosing the right level of generality can help to argue relevance in a way that more reviewers can relate to.
- **Clarity of formulation**: If there is any doubt what question the proposal addresses, there is little chance that people would be ready to honor it. A clear formulation of the central research problem is essential to make the proposal convincing. Conversely, a vague formulation is extremely damaging for the effectiveness of the proposal. It does raise doubts not only about the opportunity of the proposed work, but also about the expertise of those that are supposed to perform it.
- **Conciseness**: Being able to express the central research problem concisely is not only important to save the time of the proposal evaluators, a nice gesture in itself that may put them in a positive disposition towards the proposal, but it also demonstrates a solid expertise in the specific field of research. Being able to extract the essentials is one of the key skills of an expert.

Although, each research proposal should have a single central research problem, it is useful to structure it further into a *list of questions* that are either derived from the central research problem, or reflect partial aspects of it (sub-questions). The process of drawing sub-questions from the central research problem is sometimes determined by the approach proposed for attacking the research question as it indicates the steps that should lead towards the solution.

To add context and potentially increase the relevance of the central research problem, it is sometimes advisable to formulate related research problems in case that it can be argued that the solution of the central research problem may advance the solutions for these related problems. This can be used as secondary argument for the relevance of the proposal, but the distinction between the central research problem and the related problems should be maintained. The proposal promises to pursue the central research problem. The related problems are only a "nice to have" from the point of view of the proposal.

The following figure illustrates the relation between the central research problem, the high level problem, the sub-questions, and the related questions.



4.3.2 THE APPROACH

In the outline of the proposal, the approach description can be left very sketchy. A brief description of the steps towards the solution should be sufficient at this stage. In general, few scientists will encounter difficulties to state the approach that they plan to take in their research. Often, it is the approach that motivates the scientist to do the work. It is the preferred method, the interesting theory to apply, the fascinating laboratory installation that has to be explored and put to work that motivates the scientist to do the work. The difficulty is to make convincing arguments that the chosen approach is likely to tackle the stated problem successfully. However, these arguments can (and need to) be filled in later. In the proposal outline it is sufficient to describe the main steps of the approach merely to support the global planning of the proposed research work.

4.3.3 THE EXPECTED RESULTS

In principle, the result of the scientific work is the solution of the central research problem. However, the concrete form of these results is important and should be stated clearly. The result of a piece of scientific work could be a mathematical result, an algorithm for processing data, a device, or the concept of a device, a method for doing something differently than it is currently being done. The outline may specify the expected results only summarily and indicate their relation to the central research problem. Some thought should be given to the dissemination form of the expected results: reports, conference papers, journal papers, patents, etc. as they will partly determine the effort required to complete the work.

4.3.4 THE REQUIRED EFFORT

In the outlining phase, this part of the proposal needs relatively little attention. However, some decisions need to be taken already at this stage. In case a single research team is involved, you need to decide on:

- **execution time** for the research, typically in number of years. It clearly takes a different amount of effort to plan for one or for several years;
- **number of people involved** and main stakeholders, in case this is not clear from the beginning;
- **rough estimate of effort level from each participant** expressed as percentage of work time during the execution of the research.

For more complex proposals, that involve several teams, possibly part of different institutions, it is important at this stage to establish as early as possible, the level of involvement of each team and their area(s) of primary responsibility. This should include the responsibility for writing the specific part of the proposal.

5 THE FIRST VERSION OF THE PROPOSAL

Your learning goals for this chapter:

- *The second step in writing a research proposal: writing the first version*
- *How to complete each element of the research proposal for the first version*
- *Risk assessment in scientific research proposals*
- *Streamlining the proposal*

Moving on from the outline to the first version consists in the first place in putting more meat on the skeleton that is the outline: completing the background information, completing the arguments for the research questions and the (sub)questions, detailing the approach and arguing its effectiveness, detailing the expected results and completing the estimate of the required effort. There is one more element that typically needs to be added to the first version of the proposal: the risk assessment. It is a part that is often required by financing institutions and it is misunderstood and misrepresented in many submitted proposals.

Adding information to the outline is not the only task during the setup of the first version of the proposal. The most important operation during this phase is to streamline the various pieces of information into a coherent single piece of material such that they reinforce each other in arguing the proposed piece of scientific work.

All these issues are dealt with in detail in this chapter. The first four sections deal with completing the information on each of the four elements of the proposal: problem, approach, results and estimated effort. The last two sections are devoted to the risk assessment and to the streamlining of the proposal.

5.1 DETAILING AND SUPPORTING THE RESEARCH PROBLEM

Assuming that the formulation of the research problem as well as the relevance arguments have been done in the outline of the research proposal, the largest effort in this phase should go to a good literature review, or in other words, an analysis of the "prior art" to borrow a term from patent literature. This part of the proposal has two functions. The first, that I consider secondary, is to demonstrate that the proposing team is mastering the field of the proposal and has enough expertise to understand what has been done in the past so it can identify and fill existing gaps in the future. I consider this a secondary function of the literature review because it functions most often in the negative: a poor analysis of previous contributions is signaling a lack of expertise.

The main purpose of the literature review is to support the research question. In order to fulfill this function, the literature review has to be structured around the central research problem, the subquestions, and the related questions. There are two types of previous contributions that are used to support the research questions, and both should be given an appropriate place in the proposal: motivational items and competitive items. The motivational items are those that stated the same or similar research problems, but did not aim at solving them. The motivational items are obviously used to argue the interest that the proposed work is likely to have. The competitive items are those that tackle the same or a similar research question. For each competitive contribution, there are two questions to answer:

- how does it contribute towards the research question;
- what aspect is not (fully) covered and needs further attention, presumably in the current proposal.

It should go without saying that it is important to make a good case that the proposed work is a significant improvement on the work previously done.

5.2 ARGUING THE PROPOSED APPROACH

The approach to the research problem receives too much attention in the typical research proposal. Trying, or developing a new, brilliant idea, theoretically or experimentally, is the drive of every scientist and attracting the resources to pursue this activity is the motivation for writing proposals. However, this is usually not the motivation for honoring proposals. It is the research problem, and the hope to solve it, that is the primary motivation for accepting a research proposal.

This does not mean that the proposal does not need a good approach description. However, the main purpose of the approach description is to show that the research problem is *feasible*, that is that there is a reasonable way to tackle it and that it is likely it will produce interesting results.

One consequence of this idea is that the proposal should insist on arguments that the proposed approach is likely to solve, or significantly advance, the research problem. This can be difficult because before performing the research, the proof can be circumstantial at best.

Here is a list of typical arguments that can be used in most proposals:

- *A similar approach worked for a similar question*. This is by far the best argument and should always be considered if possible.
- *The team has extensive expertise in the proposed approach*. Even if this is a weaker argument that the former, it is always important to make sure that the expertise necessary to pursue the proposed approach is present. Still it is important to bring evidence that the proposed approach is likely to deliver the results.
- *Alternative approaches*. The proposed approach should always be considered as a possibility. The proposal is not a promise to use an approach but to try and solve a problem. If alternative approaches can be considered during the execution, the chances of success will increase.
- *Collateral advantage*. This is the weakest argument at all from the point of view of the proposal and consists in arguing that, if the proposed approach does not yield a solution for the research problem, the mere trial will likely produce different, but equally interesting results. One particular version of this argument that should be strictly avoided is that the work will improve the experience of the research team. As this can be said about any work, it is no argument for the *proposed* work.

5.3 SELLING FUTURES: THE EXPECTED RESULTS OF THE RESEARCH

Arguments to support the expected results need to convince the reader that they will help to solve or significantly advance the research question. This is obviously not an easy task as it refers to hoped-for results. There are three types of possible advantages that could be stressed when supporting the results that are promised in a proposal:

- If successful, it will be possible to do something that is now not possible.
- If successful, it will be possible to do better something that is now possible.
- If successful, it will bring a better understanding of a truth that will be helpful further.

The last type of argument seems, and is, weaker that the first two types. However, it can be used in the alternative form:

- If not successful, we will understand something that we do not understand now and that will, potentially, bring new solutions in reach.

Scientific research is not perfectly predictable by its very nature. It is important to take advantages from failures as well as from successes. There are many examples in science where failures lead to successes. A famous one is the Michelson–Morley experiment that was meant to prove the existence of the ether. Although it failed in its goal, it did contribute to the discovery of Einstein's theory of relativity, one of the great scientific achievements of the previous century.

5.4 STATEMENT OF WORK, OR PLANNING THE EFFORT

Many institutions are explicitly asking for this item in the submitted proposals. For complex proposals, involving many participants, organized in several teams, a statement of work should definitely be part of the proposal. It should show to the evaluators that there is a reasonable plan to achieve the promised results within a certain time and with given resources.

Sometimes the statement of work may be called "project plan", or "work breakdown structure", but no matter the terminology, it should contain the following elements:

- definitely, a **list of** sequential and, possibly, parallel **activities** with projected start and completion dates;
- sometimes, **milestones**, i.e. important events that are planned during the execution of the proposal, typically defined by the achievement of an important partial result;
- sometimes, **decision moments**, which are a particular type of milestones. They are defined as a moment in time when an important decision has to be taken about the rest of the work;
- definitely, **progress reporting** times.

In principle, if the desired results and the approach are well formulated, the statement of work should be a logical consequence of these two items. The time planning element is the only additional ingredient that a statement of work brings to the proposal.

It is important to understand that the statement of work is strictly related to the chosen approach. If the approach were to change later, the planning needs to be adapted. The proposal is not a promise to execute a list of activities, but to tackle an important problem and to contribute to its solution. This does not mean that the chosen approach and the statement of work should not be given appropriate attention in the proposal. A proposal will only be successful if it convinces the evaluators that it can be realistically executed within time and budget. The proposing team has only two arguments that can be raised in a proposal. The first argument is that the team is competent enough to bring the task to a successful end, which is supported by past achievements. The second argument, and the

most important one if past achievements are rare or non-existent, is that the chosen approach and the statement of work are likely to produce the promised results. Therefore, these items have to demonstrate internal consistency and a good relation to the stated problem. In other words, they have to be well thought out and communicated in the proposal.

Notwithstanding these considerations, it should be well understood that the nature of scientific research may dictate changes in the plan when new facts surface. In the positive case, when a new, better approach becomes available, that would reach the same results, or better ones, it is reasonable to modify the original plan. On the other hand (more on this in the next section), things can happen during the execution of the work that require changes to the original approach. In both cases, it is important to assign the responsibility for deciding and implementing these changes. This is very important when more than one team is involved in the execution. *Decision points*, as mentioned before, are planned moments in time for taking this sort of decisions, but even if they are not planned, it is important to keep some flexibility during execution. The fact that life does not always go as planned is not a reason to discard planning altogether.

5.5 RISK ASSESSMENT

If the format of the proposal is free, very few scientists will add voluntarily a section on risk assessment to a scientific proposal. Unfortunately, applied research financing institutions often ask for a risk assessment section as part of the proposal. Since this is, in my experience, one of the weakest parts in many of the proposals that I had the chance to evaluate, it is worthwhile to give it some particular attention.

First of all, there is a widespread confusion about the nature of risk in a scientific proposal. The general impression is that risk is negative because it is related to some undesired outcome, and therefore a high risk proposal has fewer chances to be accepted than a low risk proposal. Consequently, if forced to add a risk assessment to a proposal, most people try to minimize the risk associated to the proposed work, and in extreme cases to even deny its existence. This is actually a self-defeating strategy since a truly risk-free proposal is worthless: the results have been already discovered, or the problem is so trivial that nobody bothered about its solution. In both cases, it would be a waste to allocate resources for such a proposal. In fact, there are financing institutions that routinely reject proposals on the ground that the risk is too low. One such example is DARPA, the U.S. institution that was created with the express purpose of supporting *high risk* research. "No risk, no gain" is valid also in the scientific research business. Risk should not be underestimated and/or underreported. Rather, it should be properly **assessed** and **managed**. **Assessing the risk** means evaluating the likelihood that some unwanted event will occur and the effect that this will have on the rest of the work to be done. **Managing the risk** refers to considering and preparing alternatives to avoid negative consequences for the overall result of the work, that is *mitigation solutions.*

Here are a few possible sources of risk that may typically apply for scientific research proposals and the typical mitigation solutions:

- essential expertise necessary to perform the work will not be available. Typical mitigation solution: expertise will be sought through new hiring, or consultation with recognized expertise centers.
- required equipment will not be available, at all, or within the planned time of executing the work. Typical mitigation solution: alternative equipment will be used, or planning will be adapted to accommodate availability.
- data that is necessary to establish or validate results will not become available in time. Typical mitigation solution: alternative data source will be used.
- assumptions that are made, but that need to be verified during the research, and are needed for the progress of the research, will turn out not to be true. Typical mitigation solution: an alternative approach will be proposed to generate useful outcome.

To conclude, a mature and realistic risk assessment demonstrates a good command of the research field and is a great bonus for the proposal. A high-risk proposal is not the same as a proposal that has a high risk of being rejected. In fact, claiming low or no risk in a proposal means either that the proposed work is worthless, or that it was not thought through well enough to identify the associated risks.

5.6 DRAWING THE LINE

> *"Science is built of facts the way a house is built of bricks: but an accumulation of facts is no more science than a pile of bricks is a house"*
>
> – Henry Poincaré (1854-1912), French mathematician

Up to this point, the proposal may appear as a repository of a few items, more or less related to each other: the research problem, the approach, the expected results, the estimation of the effort and the risk assessment. However, just as an accumulation of facts is not science, putting together the five elements of a research proposal that were described in the previous sections will not make a great proposal. Before submitting the proposal, it is important to make sure that it is a consistent and unitary piece of text that puts facts in a logical order and makes arguments that reinforce each other.

This is particularly important in case of multi-team proposals, when the initial material of the proposal consists of pieces of information from several sources. It is customary and practical that every team contributes their specific part to the proposal. This is not likely to enhance the uniformity and the consistency of the proposal. Even if the proposal was put together by a single person, it is very important to make a special effort in order to harmonize the different pieces of information, to put them in connection with each other so they can support each other and the ultimate goal of the proposal: to convince a group of people of the importance and opportunity of a piece of scientific research.

One way to think about this operation, that I particularly favor, is to make the proposal into a "story", or to give the proposal a *story line*. Before explaining why this is a good idea, let me explain that this does not mean that the proposal has to become a literary piece in the artistic sense. It merely means that it should have a natural flow.

It should start with the introduction of the framework (background description), move towards the climax (the main research problem), and then, through action (the approach) it should approach resolution (the expected results). From this point of view, the estimate of the required effort and the risk assessment are merely conclusions and commentary around a well-structured story. They should be easy to compose if the story is standing.

To follow on with the literary analogy, a good proposal, just as a good piece of fiction, is about something that has not happened (yet!), but the reader has to be convinced that it can happen, given the right conditions. Concerning the structure of the proposal, rather than its content, the "story line" should be chosen in such a way that it can be followed, with a minimum of sidetracks, from the beginning to the end. There are two advantages

that this will give to the proposal. First of all, it will make it easier for the evaluators to read the proposals. The reason has to do with human nature: we are better trained to read and follow stories than any other form of information carriers. Try a scientific text, or a legal text, in case the last sentence sounds doubtful. The second advantage is that an appealing form is more likely to convince the evaluators that the proposal is well thought out, which is an important argument for acceptance.

6 THE FINE POINTS

Your learning goals for this chapter:

- *Budgeting the proposal*
- *Paying attention to form: language, grammar, readability*
- *Writing the (executive) summary*

The aspects discussed in this chapter relate to the preparation of the final version of the proposal. The recommendations that follow should be considered in combination with the recommendations in Section 7.1. on incorporating "friendly" feedback before submission.

6.1 BUDGETING

As a request for resources, budgeting may be a required part of a proposal. Realism should lead the quest for the correct budget figures. Underestimating the budget may have some positive effect on the chances of acceptance. However, this positive effect is marginal at best. It has also a negative effect, albeit on a longer time scale, on the chances of success

of the research and this negative effect is more important. Unfortunately, shortsightedness may encourage neglecting this latter effect. It is important to make sure that the budget figures are correct, not only to ensure an execution with no hiccups, but also in case of some contingencies. A serious risk analysis, either included or not in the proposal (see Section 5.5) should be considered when drawing the budget estimates.

Obtaining the correct budget figures depends on the research institution(s) where the work will be performed. Usually, the financial service of the institution will be able to help.

6.2 LANGUAGE, GRAMMAR AND ORTHOGRAPHY

For most scientists in the "exact" sciences, these can be serious obstacles in delivering their message. It is true for the scientific paper that they publish, and it is also true for scientific proposals. In both cases, scientific success is negatively affected if these "soft" aspects are neglected. There are three reasons why poor language in a scientific proposal will reduce the chances of success:

- general impression: the reviewer will see sloppiness and will assume, rightfully or wrongfully, that it is not limited to the form of the proposal;
- readability: a poorly written text is harder to read. Even a minor mistake can draw attention away from the message, which is where a reviewer should concentrate;
- reliability: by far the most important negative effect of a poorly written proposal. If somebody was not able to write a proposal properly, it is very likely that the same will be true for reporting the results of the research, including journal or conference papers. This will be a serious impediment in spreading and accepting the results of the research, assuming that the work will be successful.

There are many good books on technical writing. It is also a good idea to ask help on this item, if possible. A native speaker should always be consulted if available, and recommendations for improvement should always be taken seriously.

6.3 READABILITY

We have made an argument for readability before. The reviewer should be treated respectfully, even friendly, for obvious reasons. Reviewing proposals is not a thankful job, typically not or poorly paid in proportion to the time it takes, and readability should help the work of the reviewer.

Proper language is not the only contributor to the readability of a proposal. A few more points to improve readability:

- sufficiently large fonts;
- clear figures, including captions, legends, labels;
- clear and well explained structure of the proposal;
- proper sectioning with clear titles;
- adding summaries to stress the main points of the proposal besides the almost always compulsory Executive Summary;
- eliminating verbiage.

The latter item deserves particular explanation, since many proposals that I have seen had serious deficiencies in this respect. Many financing institutions impose a maximum number of pages for the submitted proposals. Make sure to respect the limit, and do not try to approach it unless relevant information justifies the length of the text. To be sure, long chunks of text, not clearly integrated in the context, do not contribute in any way to improve the chances of a proposal and should be rigorously eliminated. In fact, every long paragraph should be examined for relevance. Remember the two functions that a proposal has to fulfill: to inform and to convince. It is highly recommendable to scan the final version of the proposal and test every paragraph against two criteria:

- Is it informative and what information does it transmit? This question should be completed with the subsidiary questions:
  - Was the same information transmitted in other parts of the proposal? If yes, there is an opportunity to eliminate duplication.
  - Is there another, better way to transmit the same information?
- Is it convincing? The subsidiary questions for this one are:
  - What arguments have been formulated for the relevance, opportunity and feasibility of the proposed work?
  - Are the arguments specific and concrete enough?
  - What background information is necessary in order to understand the arguments as formulated?
  - Can the arguments be re-formulated such that the advantages are becoming clear with less background information?

Here are two tips for improving the strength of your proposal:

- Be as concrete as possible. Replace vague formulations such as “many advantages”, “important savings” with concrete examples and quantifiable information.
- Connect as often as possible to the central research problem, or to a subquestion that was formulated before.

6.4 A GOOD SUMMARY: LAST, BUT DEFINITELY NOT LEAST

There is a good reason why every funding agency asks for a summary of the submitted proposal. On the basis of the summary, the decision to assign the proposal to one reviewer or the other will typically be taken. The summary is usually the first contact that an evaluator will have with the content of the proposal. A well-known pun states that there is no second chance for a good first impression. After the hard work of communicating complex information in the body of the proposal, completing the planning and all the other requirements for submission, neglecting the quality of the summary is giving away the chance for a good first impression. Another unwanted consequence of a bad summary is that it may lead to the assignment of the proposal to people that are not optimally suited to evaluate the proposal, something that will diminish the chances of success considerably.

A good summary should inform primarily on the problem that will be tackled and its importance. Second, the summary should inform on the expected results in direct relation to the stated problem. Only a very short indication should be given about the approach: what kind of methods, from which field, will be used. The information on the solution approach is primarily meant to help the choice of the experts to evaluate the proposal. A very cursory mention should be given about the required effort and only in the case of complex proposals: number of teams involved, number of people.

7 PROCESSING FEEDBACK (AND REJECTION)

Your learning goals for this chapter:

- *Looking for and processing feedback before submission*
- *Processing reviews and writing rebuttals*
- *Learning lessons from rejected proposals*

The bad news is that there is no *perfect* research proposal. However, there are many good and (unfortunately!) many bad proposals. The strange thing is that I have never heard anybody admitting to having written a bad proposal. Since bad proposals exist, self-evaluation turns out to be an imperfect tool for identifying bad proposals, or even weak points in reasonable proposals. Feedback from independent readers is a much better way to separate bad and good proposals. It is, in fact, the main mechanism used by financing institutions in their acceptance process. It is also a very good means for improving a proposal so it should be actively sought by those that intend to submit a proposal even before submission.

This chapter deals with processing feedback on the proposal, both before the submission, and after the proposal has been reviewed.

7.1 LOOKING FOR FEEDBACK BEFORE SUBMISSION

After submission, a proposal will face review and evaluation, a process that may take a few months and is performed by anonymous reviewers. It is a good idea to look for feedback before the submission. It is the only way to test if and how the intended message of the proposal arrives to people outside the group directly involved with the proposal. Shakespeare wrote somewhere that a joke's success is determined by the ears it entered, rather than by the mouth it came from. By analogy, the fate of a proposal depends on how it is read, rather than on how it was written.

It is strongly recommended to look for a knowledgeable and friendly person to review the proposal before submission. If possible, find reviewers that will take care of different aspects of the proposal: the scientific aspect, the planning and financial aspect, and, last but definitely not least, the linguistic aspect. In fact, if the language of the proposal (typically English) is not the native language of the proponent, then a review of the language is highly recommended.

Hereby a few suggestions on dealing with this kind of feedback:

- Adopt a listening attitude and not an argumentative one, even if some of the criticism appears not warranted. Remember that if your friend or colleague misunderstood you, there is an even greater chance that a reviewer will misunderstand you. Try to find the source of misunderstanding and eliminate it, rather than argue why your friend is wrong.
- Find out by directed questions if the criticism is a matter of form or content and change accordingly.
- Contradictory criticism is not to be dismissed, or attributed to the inconsistency of reviewers, but should be carefully investigated. It is an indication that the message of the proposal was not received correctly.

7.2 DEALING WITH COMMENTS FROM REVIEWERS

Submitted proposals are generally evaluated by a number of anonymous reviewers. Their reviews are used (generally, by a commission) to decide upon the fate of the proposal. In most cases, the reviews are sent to the proposing team together with the decision on the proposal. In some cases, there is a **rebuttal** phase that gives the opportunity to the proposing team to answer the issues raised by the reviewers with the possibility to eliminate misunderstandings and change the initial evaluations of the reviewers. Obviously, this opportunity should be used to the maximum. In case there is no rebuttal phase, the comments from the reviewers should be put to good use too. Both situations are discussed in this section.

7.2.1 WRITING REBUTTALS

It should be kept in mind that few reviewers are ready to change significantly their first evaluation. They spent time reading the proposal and only very serious and adequate objections may convince somebody to have another look. Therefore, it is wise to have modest expectations about the effect of a rebuttal. Still, the chance of improving the score of your proposal should be wisely used.

Here is a stepwise approach to writing convincing rebuttals:

- *Make a prioritized list of objections and criticisms that the reviewers had.* Criticism that is common to more than one reviewer should definitely be given higher priority.
- *Classify the objections according to the following criteria:*
  - are they related to the form (presentation) or the content of the proposal,
  - those related to the content, to which element (importance, motivation, approach, etc.) of the proposal does each objection refer.

- *Make an assessment about the motivation of the objection*: is it due to an ambiguous formulation, is it due to the unfamiliarity of the reviewer with the field, or is it due to a genuine mistake in the proposal that the reviewer identified. Remember, in each case, the responsibility is for the one, or for those that submitted the proposal. The proposal needs to be written clearly and to be understandable even for people that are not directly familiar with the narrow field of the proposal. Attacking the professionality or the intentions of the reviewers may sometimes look tempting, but it is a temptation to be resisted at all times. It is better to identify the reasons why the proposal was sent to the wrong expert. If there are reasons related to the proposal itself, such as e.g. non-informative summary, corrections need to be made.
- *Deal with each objection fairly*: recognize the failings in the original proposal, offer corrections and, if it is the case, new relevant information. Do not take criticism personally, and definitely, do not show it, if you do!

7.2.2 LEARNING LESSONS FROM FAILED PROPOSALS

If a failed proposal is to be useful for the future, it is important to gather as much information as possible about the cause of the rejection. This is often a hard task even if reviewers' reports are available. The first step in finding out what can be learned from the failure should be to find out as much as possible about the reasons for the rejection. Any possibility in this respect should be considered. If possible, ask the head of the evaluation commission for additional information. Personal contact should be preferred to written communication, but if the latter was the only thing on offer, it should be graciously accepted.

Based on the recovered information about the evaluation process, it is important to set an honest diagnostic. Only after this step, should recovery measures be considered. Honesty is of essence here, and given the personal feelings naturally related to a completed proposal, this is a very difficult requirement to fulfill in practice.

Here is a list of possible causes of failure and possible measures that should cover most situations:

- not the right institution to submit the proposal: look for a different institution that is more appropriate to the type of proposal that was submitted;
- the subject is not of interest: look for problems of greater interest that can be tackled with the available expertise;
- the approach is not appropriate: consider different approaches to tackle the problem, or look for a different problem for which your preferred approach is adequate;
- the available expertise is not sufficient: consider cooperation with experts and/or teams that complement the lacking expertise;
- the presentation was not good enough: consider hiring expertise for the particular aspect of the presentation that was deficient (e.g. language expert);
- others were simply better: not much to do but try to improve and do better next time.

8 USEFUL RESOURCES

Your learning goals for this chapter:

- *Where and how can you find more information if needed*

8.1 THE INTERNET

Of course, even a short visit to google.com or to your favorite search site will deliver in a very short time plenty of resources related to scientific research proposal writing. The danger is rather of too much than of too little information on this relatively popular subject among internet users. All I can do is to help a little with finding the right information.

Therefore, instead of listing any particular site with useful information, I will list only a few tips that may help to avoid information overload and the associated time loss.

The first stop on the internet, even before starting the outline of the proposal, should be on the site of the funding institution. Most large funding institutions provide templates on their sites and even samples of proposals. They also provide sometimes instructions and advice about setting up the proposal. Obviously, make maximum use of these resources and pay special attention to all instructions, even those that may seem unimportant.

If your own funding institution does not provide a template or samples, do NOT use templates or samples from other institutions. They are useless for all practical purposes as they are specialized for particular fields and the chance that they are of any use for your own proposal is negligible.

There are many one-page lists of tips for writing proposals on the internet. Your favorite search engine will provide them to you if you are interested. In my opinions, most useful information on the internet are lists of reasons for proposal rejection. They could be useful as checklists at the fine tuning phase, before submitting the proposal. Besides the typical "Top X reasons for rejecting a proposal", an internet search may reveal some exotic results such as this article from the journal Nature: https://www.nature.com/news/grant-application-rejected-over-choice-of-font-1.18686 that exposes the case of a proposal rejected because the wrong font was used in the document.

8.2 OTHER RESOURCES

There is a great wealth of books and articles, some available in digital form, that tackle the subject of this text and/or related subjects. Depending on the available time and interest, many of them may contribute useful information to anybody that wants to write better scientific research proposals. In my opinion, one should be very selective with such reading. The scientist is and should remain an expert in his/her own field of research. Keeping ahead with the developments in your own field should always take priority. Writing proposals is a useful skill that needs to be acquired. Doing it often and over a long period of time will improve this skill as any other. Reading about it is useful, but only if the new information, tips and techniques are exercised in practice.

For anybody that feels the need to know more than is covered in this book, I will recommend additional reading in two fields that were necessarily not covered in detail here.

The first field is *technical writing*. The importance of good language skills should be obvious for everybody and was stressed in this book in a few places. There are many books devoted to the subject and consulting one of them, or taking a specialized course, is very important especially for non-native English speakers. I would say that native English speakers may want to pay some attention to this aspect too as they may face less tolerance for language errors than non-natives may enjoy.

The second field that was only superficially covered here is *project planning*. In case of complex proposals, some acquaintance with this subject is very useful. Again, literature on project planning is vast, and specialized courses are available too. It is important to choose a source of information that covers the appropriate scale of the project, compatible with the proposal size and complexity under consideration. Obviously, techniques that apply to projects involving more than three teams and budgets in the order of millions of dollars are inappropriate to individual research projects with budgets in order of tens of thousands. Some techniques from project planning can be useful, however, for phasing the approach and for evaluating the required effort involved in scientific research work.

AFTERWORD

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Thank you!