Get Published!

Successful Scientific Writing

科技论文成功发表的技巧

Ralph R. Martin 著

清华大学出版社

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内 容 简 介

本书面向希望将英文科技论文发表在更高水平的刊物和会议上的科技工作者。书中以简洁生动的例子讲述了如何写出一篇能成功发表的科技论文，内容包括写作前的准备（制订计划、完成初稿等），一篇成功的科技论文应该包含的要素（背景介绍、文献综述、概述、每一部分的理论和技术细节、实验结果、结论、致谢、参考文献和附录），每一要素应该如何写作，如何让论文更出彩（内容的逻辑性、用词的严谨性等），以及科技论文中表格、插图的排版和组织技巧等；最后给出了进一步阅读的建议。

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Preface

Ralph Martin has extensive experience in scientific writing and editing, with over 250 scientific papers and more than a dozen books to his name. He is also on the editorial boards of several international journals, including Computer Aided Design, Computer Aided Geometric Design, Graphical Models, and Computers and Graphics, amongst others. As an editor and reviewer, he has had to read many papers, from the very well written to the impossible-to-understand.

He has been a full professor at Cardiff University, UK, since 2000, and is also a guest professor at several Chinese universities: Tsinghua University, the National University of Defence Technology, and Shandong University. He has collaborated with many Chinese professors and students and has helped to improve their papers for publication in world-leading journals and conferences.

He has given popular talks at various Chinese universities on "How to Write a Scientific Paper" and, as a result, was urged to write a book on the topic. This book is the outcome. It expands upon the ideas from those lectures, as well as giving other advice on how to present your research results.

If you are looking for a book on English language vocabu-

lary or grammar, there are plenty of other books to choose from. This book assumes you are reasonably competent at English, and you want to put your English to good effect, to improve your chances of your research being published.

Of course, much of the advice will remain the same if you wish to write your papers in Chinese or any other language, too.

Boxes in the book are used to provide examples which illustrate general points made in the text.

Boxes with round corners

like this one

either illustrate points via particular cases, or give examples based on real papers.

Boxes with square corners

like this one

are samples of plans or writing which might be found in fictitious papers, and do not refer to real research. References are made to imagined authors and at times to nonsense topics and methods. If real research has been done on such topics or by authors with such names, and I have inadvertently misrepresented it or them, I apologise—this was not my intention. Two such fictitious examples used in various places concern the estimation of the speed of cars in video and an omnispectral camera.

Boxes with a heading like the one below

Do not write like this!
My paper is the best work ever on computers!!!

are examples of *bad style*, which should not be copied.

I would like to thank Prof. Shimin Hu for encouraging me to write this book, and for pushing me to add further advice and examples to bring it into its present form. I would also like to thank Prof. John Samson, and my wife Xiaoqing Li, for reading drafts of the book and offering helpful suggestions.

Contents

1 Getting Ready

1.1 Preliminaries

This book is intended as a guide to presenting your research in a way which enhances its chance of getting published in a high quality science or engineering journal or conference.

While the author is a computer scientist, and some of the discussion focuses on that field, many of the suggestions generalise to other fields of science and engineering—the principles of clear writing have much in common across these disciplines. Examples from a wide range of areas have been used to illustrate the concepts discussed.

Two things are necessary to reach the goal of publication in a top-rated conference or journal. The first is *content*, and the second is *presentation*, or *style*.

I cannot help you with the content—especially in a discipline other than my own. The content is your great idea, your advance in the subject. Without some novel ideas which further your chosen topic, no amount of careful writing will improve your paper to the point of acceptance.

On the other hand, even if you have made a major breakthrough in your subject, your paper may still get rejected if

it is not well enough written. Many scientists and engineers put in a huge amount of effort to the research itself, but unfortunately, not enough into preparing and describing their work and results for publication. Improving the quality of your writing can make a significant difference to the chances of your paper being accepted.

If the description of your approach is not clear, a reviewer (also called a referee) may declare "no one could reproduce these results", which may lead to a request for major revisions to be made, or worse, outright rejection. If your writing is not clear, the reviewer may not understand the significance of your new idea, and dismiss your work as unimportant. To strengthen your chances of success, careful attention should be paid to your writing as well as your research.

1.2 Content

Before we move on to writing style, let us first consider the paper content a little further. How can you tell if you have material worthy of writing up into a paper? The key point is that there must be *novelty* and *originality* in your work.

The strongest kind of originality is shown by a paper which finds a new problem to solve. Obviously, the problem must be sufficiently difficult that its solution is challenging, and the problem must be one whose results are of use or interest to potential readers. Finding new problems that lie in the narrow gap between too easy and too difficult is tricky, but

should be the aim of all researchers who wish to reach the highest levels.

> Devising an algorithm to find *human faces* in photographs would not be novel—there are already methods for doing this. Devising an algorithm to find *snakes* in pictures might be a novel problem (at least, I have never seen a paper on this topic), but even if it is, it is not really a good problem. On one hand it is probably too difficult to achieve a high success rate, as too many other things look like snakes, and on the other hand, even if you did a achieve a high success rate, it is unlikely that anyone would really want to use the software (even though snakes can be dangerous). This problem is too specific.

If you cannot find a truly novel problem, at the next level down, maybe you have a new approach to solving an existing problem, or at least some step in a problem.

> This might be a better way to find faces in photographs, or a better way of detecting eyes—one approach to finding faces in an image could be to look for pairs of eyes, a nose and a mouth.

Even less original is to take a variety of existing ideas, and put them together in some novel way, to solve some known problem.

A method for detecting *traffic signs* in a photograph could be built out of various standard components which find circles and triangles in images, which find and interpret text in images, and which seek instances of various template shapes representing standard signs.

The originality here would lie in the choice of suitable components, and linking these standard components together to meet an overall goal. However, such work is unlikely to be considered novel enough for a strong journal or conference unless there is some clever insight in the way the system is constructed.

Overall, however, the key point, if you do not have an entirely novel problem, is that your method or theory must typically be an *improvement* over existing work in some way.

This improvement could be a *faster* algorithm, or a *more accurate* algorithm, or a theory which explains *more* observations, or a chemical synthesis which produces *fewer* unwanted by-products.

Merely being *different* is not enough to merit publication. For your work to be an advance over existing knowledge, it must be *better* than previous work in some way. Fortunately, you can choose the way in which you wish to claim your work is better.

There is an exception to this requirement to be better. Negative results can also represent advances in knowledge,

if they are unexpected. For example, you may have devised a method or theoretical approach which most people with a good understanding of the topic would *expect* to be better, and you have shown for some unforeseen reason that it is actually *worse*.

A paper published in 1975 by Trivedi has the title 'On a negative result regarding the use of continued fractions for digital computer arithmetic', and came after several other papers had discussed the possibilities for alternative computer representations of real numbers.

An alternative type of negative paper is one which presents some experimental evidence against, or a counterexample to, a previously accepted theory or approach.

Kempe in 1879 wrote a paper giving a proof of the famous *Four-Colour Theorem*, which states that any map, in which no more than three countries meet at any one point, can be coloured using no more than four colours such that no countries sharing a boundary have the same colour. (Countries are assumed to be just one contiguous land area). However, about 10 years later, Heawood wrote a paper giving an example with 18 countries which showed an error in Kempe's proof. A valid (and computer-assisted) proof of the theorem was not found until 1977 by Appel and Haken.

Previous papers may have claimed a certain drug to be effective in curing a disease, but finding that it does not work if the patient is simultaneously taking some other medication is likely to be a significant negative result.

Finally—put just *one* good idea in each paper! Not only does having more good papers look better on your curriculum vitae, but also putting more than one good idea in a paper can lead to undesirable outcomes to reviewing. For example, even if the main new idea is brilliant, a reviewer who thinks your second good idea is weak may turn the paper down.

If you discover a new wonder drug which cures both asthma and baldness, write two separate papers, one about each cure. Quite different readers will be interested in these results, in any case.

1.3 Community

The way in which you structure your paper will depend to a certain extent on the field, as differing fields have different unstated rules and expectations about how a paper should be written, how to go about describing approaches and analysing results, and so on. An important question to ask before writing is: "To what scientific community do I belong?"

This might be the *computer graphics* community, or the *computer vision* community, or the *image processing* community.

The next question to answer is "What are suitable journals or conferences in that community to target"? We will consider this topic in the next section.

Having established these points, you should consider the structure of papers already published in the relevant journals or proceedings, and use the approach typically found there as a pattern, both for how to carry out the research itself, and for writing it up. A paper (and approach) which follows the expectations and practices of a community is much more likely to be accepted by that community than a paper written in a different style.

Papers using DNA analysis to suggest evolutionary relationships between organisms typically have a section describing how the specimens analysed were collected and identified. If the specimens were incorrectly identified, the paper's results would be invalid, which is why particular emphasis is placed on this issue.

Papers in computational geometry typically have a theoretical analysis of how the performance of each algorithm scales with the quantity of input data, yet do not describe

practical testing of their algorithms. In fact, in this field, algorithms often remain unimplemented.

However, in computer graphics there is a much greater emphasis on practical testing and the quality of the pictures output, and rather less on theoretical analysis.

Knowing your community and its expectations is important.

1.4 Where to Publish

Having identified your community, you should find out which are the leading outlets, i.e. *journals* and *conferences*, for that area.

In computer graphics, leading journals include ACM Transactions on Graphics, and Computer Graphics Forum, while corresponding leading conferences are ACM SIGGRAPH and Eurographics. Of course, there are also many less prestigious outlets in this discipline which may be more appropriate if you have less significant results to announce.

You should also determine the relative importance of journals and conferences. In many disciplines, journal papers are more highly rated than conference papers, although in others, the top conferences can be more prestigious and harder to get into than even the leading journals.