Ethics in Competitive Research

Do not get scooped; do not get plagiarized

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June 2018.

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ISBN 9789387480865

Takeaways from this book:

- *We should choose relevant research problems consistent with our available capabilities to ensure speedy and impact-making research. We must plan how and where to publish. We must also plan how to protect ownership about our original contributions irrespective of possible economic benefits. Like patents, research papers must be proudly owned!*
- *Be generous and give credit wherever it is due, rather than paraphrase and avoid giving credit. You are open to scrutiny for posterity, and social media will highlight the plagiarism allegations pertaining to your old work when you achieve prominence later in life*!
- *The message on self-plagiarism is that refer to your earlier work not just to avoid charges of self-plagiarism, but more importantly to highlight your continuing and sustained contribution!*
- *It is not so straightforward to start reporting our results if our results can be termed as 'unexpected'. While one must not be paranoid, one should also not provide unpublished results to competitors who have the facilities to quickly reproduce these, or produce slight variants thereof, and release them as their own. Some effort has to be put in having a strategy to bring your research to the world's notice*! *Any attempt at dissemination must highlight your new contribution*
- *Dissemination without delay but with a high level of visibility ensures both (i) ownership of the researchers and (ii) a proper post-dissemination validation and evaluation of the research*

output. No one should steal our idea. Upload the manuscript on an E-print archive simultaneously with submission to a Conference.

- *Validation of major path-breaking research output has always been linked to the post-publication acceptance by the community of researchers in the field, and not just to its being published in any journal, however 'reputed' it may be. Bias in the review process of a reputed journal can make us lower the level of our research output by forcing us to dilute our claims at the publication stage.*

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Contents

**The first two chapters will appear as a rehash of what active researchers already know, and can be skipped. They are included for completeness and context.*

Foreword

This book shall discuss ethics during dissemination and publication of research results. It will discuss the need to give credit where it is due, and will discuss how to prevent unintentional (or accidental) plagiarism of others' text while writing one's own manuscript. It will discuss how to prevent allegations of self-plagiarism. And, most importantly, it will also discuss steps that can help to prevent others from usurping credit for your research contributions.

We do sometime find an idea that occurred to us, or even research that we carried out, being reported independently by established research groups. We seek credit through patents for that research which has potential financial implications. Can we seek credit and priority for all our original contributions irrespective of financial implications, even as we disseminate these research contributions through conferences and peer-reviewed journals? We shall discuss some steps that can help. This book aims at guiding students who will lead through new ideas, and whose research may later be used as templates by other researchers. My dictum to combating plagiarism is "one should be neither a perpetrator, nor a victim, of plagiarism".

I shall discuss some examples from literature that come from my discipline, and I am familiar with. I will try making them intelligible to researchers across disciplines.

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Prologue

Why am I writing this? I have had many unusual experiences in my research, and am arguably the most plagiarized Indian scientist! In one reputed journal (viz. Physical Review B) published by the American Physical Society, there are four Errata that have been published because paper(s) on which I was an author had not been given due credit by the authors in their original work. The four Errata are listed in the Appendix. (I am providing links.) These appeared in 1977, 2002, 2005 and 2011, spanning almost the entire length of my active career starting from my first ever journal paper and my first ever conference paper (with the new ideas presented there being euphemistically stated as 'observations')! All our papers that had to be cited in each Erratum were works done from India, with no foreign collaborators. All were cases of perpetrator-authors being from established and respected institutes, who had done original work that was otherwise publishable, where our idea was underlying their work but was not being acknowledged. Our idea was being plagiarized in that credit for the idea was not being given where it was due. There were many other cases where corrections were not forthcoming, an experience that I consider common for Indian authors working without foreign collaborators. Is my possible record in obtaining ethical corrections and in getting the misdemeanours accepted just because of my good luck? I believe that at least in the last case (2011) I had taken many precautions to prevent our idea from being usurped, and these precautions enabled the swift though limited correction in this particular case.

These precautions follow a path not commonly trodden, and there may be lessons that can be learnt.

This book is not my story; it is about what I learnt. In my research I have liked to see what others have seen but to think what others have not thought, and to pursue those new thoughts to predict and see what others have not seen. My research output has thus been characterized by this dictum, and I have now followed this dictum in understanding plagiarism. I am disturbed by checks for plagiarism moving away from experts, and being now taken over by software that only checks for text-plagiarism and cannot detect plagiarism of ideas that has been camouflaged with clever word changes. I consider that text-plagiarism could be inadvertent or accidental for those who are limited in various aspects like command over language, and may have no connection with their original contributions to knowledge. I have argued that plagiarism of an idea would only be undertaken by someone who can assess the possible validity of an off-the-beaten-track idea, and who must accordingly be an expert. Idea-plagiarism, I believe, is practiced by established scientists (whose papers *are* easily accepted for publication) with the victims being scientists from emerging by-lines (whose papers *are not* easily accepted for publication). I consider the youth from less-established by-lines as potential victims of idea-plagiarism, and this book is actually an attempt to educate them and empower them with suggested precautions to ensure ownership of their original ideas.

1. Introduction*

1.1 Research is creating new knowledge ─ credit to the first past the post

In school I was taught a poem with the line 'he who knows not, and knows that he knows not, is a child, teach him'. But this saying assumes that someone in the human race knows what the individual does not know. What if the human race does not know? Research is what is required to teach the human race, to teach posterity. Research intends to add to human knowledge, to *create new knowledge*. We paraphrase the proverb to '(s)he who knows that the human race does not know, has taken the first step in research'! The planning for research now starts, and new knowledge is created when our research finds an answer.

The claim that new knowledge has been created remains only a claim until it is independently verified and accepted. Such verification by other experts requires that the research output should be disseminated, along with the procedures followed, in a forum accessible to experts or it should be released in a public domain. This is called publication of the research output which may, however, be available to the public only after payment of a fee to the journal where it is published. We shall discuss this accessibility issue, and how the researcher can control it, in a subsequent chapter. There is often a review by a few experts

**This chapter can be skipped by active researchers. It is included for completeness and context as we emphasize planning for competitive research, where one must be the first and not just an also-ran who is somehow publishing a me-too paper*

(typically 1 to 4) before the research output is accepted for publication. This pre-publication review process does not ensure validation of the research output because the limited number of experts could have missed errors. In a subsequent chapter we shall reinforce this point with some famous examples that involved even fraudulent or fabricated data. Validation of "new knowledge" requires continuing post-publication reviews, as *any counter-example to an existing accepted theory will question and raise a doubt about that theory*.

Once a research output is published, other researchers will do experiments that provide confirmatory tests. This test could be done by introducing minor variations in the research methodology, and are accepted and published as a research output irrespective of whether they support or contradict the previous research work. Or the test could be an exact duplication, in which case the results would be accepted and published as a research output only if they contradict the previous research work. In cases where the results of the previous research are considered drastic or path-breaking, even an exact duplication would sometimes be accepted and published as a research output when they confirm the previous research work; this happened in the case of high-T_C superconductivity, where papers supporting previously published experimental work were published by the Physical Review B [1]. However, only very few such reports were accepted for publication. We have, inter alia, introduced the concept that research output could be confirmatory (or negating), or could be path-breaking. We shall return to a proper characterization of 'types of research' shortly.

1.2 Publication validates, and claims priority for, new knowledge

At this point we stress that dissemination, or publication, is an essential part of research because it is essential for validation of new knowledge that the research created. A research project starts with choosing the problem, and cannot be considered completed without dissemination. Validation can follow subsequently.

As we have noted, research begins by formulating questions whose answers do not exist. The question could be of minor or major significance, and this dictates how much 'impact' our research can make. If the question is of major significance, then others would have also asked ─ or would be asking ─ the same question; and we shall stress that competition in finding answers is an essential component of research. We must caution that the reverse does not hold. Just because others have not asked a particular question does not imply that it is of minor significance. Many Nobel prizes have been won by people who asked questions that others did not dare ask!

Before proceeding, we wish to stress that since research creates *new* knowledge; by definition, this can be done only once. The same idea, or product, cannot be patented twice by different inventors. Novelty is an essential requirement for claiming a patent.

In the context of claiming a patent that is protected by the legal system, priority is essential. Priority can be set as being 'first-to-invent' or 'first-to-disclose' or 'first-to-file' [2]. If person 'x' can

establish priority in inventing, without ever claiming financial benefits through a patent, then in the 'first-to-invent' system it can prevent others from filing a patent. Similarly, the person who is the 'first-to-disclose' was being granted a grace period within which the patent could be filed. These two utopian concepts of giving credit to the real inventor of the novelty have not really survived in today's legal-system driven world. Even the U.S. switched the right to a patent from the previous "first-to-invent" system to a "first-inventor-to-file" system for patent applications filed on or after March 16, 2013 [2]. Thus priority is now only being given to the 'first-to-file' claimant.

Similarly, the same research result cannot be claimed as a new result by different sets of researchers, and will not be knowingly published on a second instance as a research output. This 'first-to-publish' system in research is the counterpart of the 'first-to-file' system in patents. Date of publication is a major issue even in research, and we again note that competition is an essential component of research. However, there is some movement from "first-to-publish" system to a "first-to-disclose" system for credit in research. We shall discuss this in detail starting with Chapter 6.

We have made two points. First, we have to choose our research problem as a question whose answer is not known, and is likely to be of interest to many. Second, we have to find the answer before anyone else finds it. As noted in the previous paragraph, we also have to publish our result before someone else publishes the same result. (This is the serious issue of claiming ownership *without being scooped*, and we will address it in subsequent chapters.)

After choosing our research problem, we have to constantly follow current research literature to ensure that we are aware of any progress made by anyone in this research problem. If someone else finds and/or publishes the answer before we do, then we cannot publish our research output and we have to start afresh with a new research problem. If we are ignorant of someone else already having published the answer we have just found, then the reviewers will enlighten us when we submit our paper for publication, and will reject it. We would have lost, at very least, the time spent in preparing the manuscript. The date of public dissemination dictates priority, and we will discuss this starting Chapter 6.

1.3 Planning our research

This starts with choosing our research problem, as will be discussed in Chapter 2. The question we are asking could be one that many people are asking. This is colloquially referred to as doing research on a hot topic. Issues of ethics and priority become very relevant here. Here the questions are reasonably well-posed and there is a race to find the answer because all credit to those who found the answer first! The researchers subsequently coming up with the same answer may find it very difficult to publish and, if they can publish then these authors must necessarily refer to the work that provided the answer first as in the example cited earlier [1]. One will then always be an also-ran.

Researchers may address slightly modified versions of the same question. Addressing slightly modified questions has an advantage

if one is trying out-of-the-box ideas, viz. that one may be ignored by the large number of researchers who are focused on the hot topic. Aggressive attacks from this large group of peers who are working with conventional ideas are avoided. This allows new ideas to make inroads in literature, to be tested by unbiased researchers, and to get established without facing an onslaught. This strategy is somewhat like Chanakya's suggestion of nibbling at the edges of a kingdom rather than trying to attack the capital! (Similar idea was enunciated much later by Arthur Hugh Clough in the poem 'Say not the struggle not availeth' where he talks of sea water entering through a creek.) We shall discuss an example of this way of attacking hot problems in Chapter 2.

We now make some general observations on the choice of a research problem. First, we must be aware of our capabilities, both experimental and theoretical. In case we need additional capabilities for working on this problem, we should cautiously explore whether there is a group with those capabilities, and whether they would be open to a collaboration *that is driven by us*. Otherwise, we should explore whether it is feasible for us to develop those needed capabilities in an acceptable time-frame, or whether we can reach some meaningful conclusions with the limited capabilities that are presently available to us.

To summarize what we have discussed, *we should choose relevant research problems consistent with our available capabilities to ensure speedy and impact-making research*.

Research often shifts from attempting to understand what is, to attempting to visualize what is not; i.e. shifting the question from 'why' to 'why not'. In the latter case we sometimes create new knowledge which mankind can use**,** and may pay us for using it if we own the patent. In view of the possible financial overtones, patentable research is usually kept under wraps; patentable research and basic research are both conducted and disseminated in very different ways. This book only deals with research that is not considered patentable, that is not considered to have financial benefits.

1.4 Ethics, plagiarism, and ownership of research

Ownership of research output is also important in basic research, and there are many instances where researchers have worried seriously about how their research is disseminated to ensure that ownership is established. We shall present some such instances involving scientists who subsequently won Nobel Prize for that work. We first discuss below that ensuring ownership and priority is important irrespective of its apparent or perceived importance.

Evaluation of individuals, and institutes, involved in basic research has become very commonplace, and is now discussed openly since our society seeks transparency. The evaluation of universities has become an annual feature, with many organizations, each using their own varying criteria, being involved in such exercises. Without discussing the reasons for such evaluations, what concerns us is that the need for transparent evaluation of research output has put emphasis on quantitative metrics (even though experts question

the relevance of some of these metrics). Further, the development and easy availability of commercial software that checks for text-similarity amongst documents in public domain has made "plagiarism" a very common term in the vocabulary of monitoring agencies. I give below two examples of how plagiarism has become a very common and compulsive worry.

1. In 2014 the MHRD announced an essay competition, with one clause on 'Quality' stating therein "Provisionally selected essays will be open to public to comment on plagiarism" [3].
2. In 2017 UGC has proposed the setting up of plagiarism detection authorities in all institutes of higher education, with all allegations of plagiarism being examined and the report submitted to this authority [4].

Some worrying trends have also emerged because of this obsession with plagiarism, and I have noted earlier [5] that "Plagiarism-detection software has opened up scrutiny of scientific publications to non-experts…… Hobbyists and political opponents have made a cottage industry out of searching the back catalogues of high-profile individuals for evidence of such misdeeds." This point in my introduction has been elaborated into a long article by Bailey [6] titled 'The weaponization of plagiarism'. The crucial point is that *you are open to scrutiny for posterity, and social media will highlight the allegations pertaining to your old work if you achieve prominence later in life*!

The emphasis on plagiarism has, quietly and unnoticed, put an emphasis on speedy dissemination, in addition to speedy research. We understand this by creating the following scenario.

If you are the second to report your research output, answering a question that others are also addressing, you may, of course, not be able to publish in a reputed journal. To protect your effort of months (or even years) you will still try to publish in a 'lesser journal' by emphasizing variations from the earlier report, or by not giving due recognition to the earlier report, and earn the publication required for your Ph D degree. But two research reports providing knowledge on the same problem will probably have at least one 10-word string identical. You definitely run the risk (even much later in life) of being accused of plagiarizing the paper that had disseminated earlier.

There is thus a pressing need for research scholars (and also other researchers) to speed up dissemination, and establish priority. The protocol for ensuring this shall be the main theme of this book.

1.5 Categorizing types of research

Before proceeding, we identify research into three categories viz. supportive or confirmatory research, incremental research, and path-breaking research. We note that the criterion for classification into such categories is still evolving, and the categories have been given different tags by different authors. Stephan et al [7] have classified published papers into the categories 'non-novel', 'moderately novel' and 'highly novel'. Their categorization is

based on the time-profile of the citations the paper receives and is possible only some years after the research output is disseminated. Another classification [8] is based on the contextual manner in which the paper is cited by other authors; this classification is again possible only some years after the research output is disseminated. We now discuss how to classify the possible research output at the initiation of the research project. This is important because, as we shall discuss in the next chapter, this will help us outline our strategy for carrying out the research. If the research proceeds along anticipated lines, this classification will also help us decide how to disseminate our research output.

Research proposals are routinely submitted to funding agencies (like DST-SERB in India) for funding research proposed to be carried out over a 3-year period. Such proposals also usually seek funding for a research student and one concludes that this is what a young research scholar will do as (s)he works for a Ph D degree. Such a proposal can be considered for approval if experts in the area conclude that publishable research output is likely. While the proposals considered for approval are not presently formally classified into these categories, I believe we do informally do some such classification.

We classify a research proposal as supportive or confirmatory if it is pursuing very closely already published research. Confirming (or negating) already published research is what post-publication validation is, and this constitutes legitimate research activity. The confirmation or support can come under identical or nearly identical conditions and the first such confirmatory reports would

be published if the original research outcome was drastic, as in the case of high-temperature superconductivity [1]. Here the transition temperature reported in the original research was beyond a theoretical limit (of about 29K) that had been accepted for many years. Also, it was seen in oxide materials with low carrier density and with a normal-state conductivity that was very low.

Support can also come by studies in conditions that are only slightly different (like a slight change in the composition of an alloy), and one can foresee problems getting such papers published in reputed journals. This kind of a proposal is also loosely termed as 'me-too research' [9], as it follows the earlier published paper very closely. (Publication is, of course, easier if it negates the earlier published paper!) Such 'me-too research' does not really add to human knowledge, and is not likely to receive many citations or create an impact. We shall discuss later the care needed while writing our own paper because, as we will discuss later, there is a danger of inadvertent reproduction of some word-strings. And we can later be accused of text-plagiarism! We shall discuss this in Chapter 4.

The next category is of 'incremental research' [9]. Research that adds to human knowledge has to be incremental, but the incremental steps can have varying levels of 'originality'. If the steps are drastic, as in the case of high-T_C superconductivity in the oxides, we would term the research as 'breakthrough' or as 'out-of-the-box' research. This will be the third category that we will discuss next. In the second category of incremental research, we restrict to one-step developments that would follow quite logically

to most researchers, provided they decide to ponder on the new knowledge claimed in any published paper (whether it is of the breakthrough or of the incremental category). Incremental research makes a one-step extension of the new idea or result in a published paper, and then checks its validity.

I take an example from materials research. If a research paper reporting measurements on some material has proposed new ideas to explain some new or unexpected observations, these ideas can then be tested on a variety of other suitable materials to check whether the same explanations apply. Or the ideas can be used to predict what should be observed in some other properties, and this would provide a test of those ideas. Here again we closely follow the work described in an earlier publication, and there is again a danger of inadvertent reproduction of some word-strings. The precautions to be followed while reporting our research will again be discussed in Chapter 4.

The third category is of research based on out-of-the-box ideas and, if successful, results in some kind of a breakthrough. This kind of research is deviating from standard or published ideas in that the researcher may be seeing what other researchers have already seen, but thinking what those other researchers have not thought. This could then result in a theory paper that presents a new model or theory. The researchers could be more ambitious and make predictions based on this new model. They could be even more ambitious and do experiments to test some of these predictions! Following such an ambitious path would require tenacity and time.

If one publishes at the first stage, i.e. with the new thoughts on existing data, some others will try to confirm or refute the new ideas. In an ideal ethical environment, there will be some copying of text with appropriate apportioning of credit. This should result in extensive citation (as against a typical running citation for the earlier two categories), which is a measure of the higher level of originality. Research publications in this category serve as templates for future research, and thus the researchers run a small risk of becoming victims of plagiarism. These risks have to be factored in and minimized, and successful researchers ensure both visibility and priority while disseminating their research output, aspects that are not usually discussed. (Discussions are usually restricted to our not being perpetrators of plagiarism, and avoid the scenario of our being victims of plagiarism!) We will discuss these aspects starting Chapter 6.

We now summarize what we have discussed in this chapter. While economically relevant ideas and technical innovations are patented, the creators of original thought do not want to be deprived of credit even when no economic potential is envisaged. Credit is a driving force for humans and quality of original thought is one of the most fundamental ways to judge scientists' work and determine who progress in their career. *Giving credit to those who did the original work is essential.*

References

[1] See, e.g., J V Yakhmi et al, **Phys. Rev. B 35**, 7122 (1987). They reported in the abstract that "our results are in very good agreement with those of Cava et al".

[2]https://en.wikipedia.org/wiki/First_to_file_and_first_to_invent

[3] MHRD announcement of Gurutsav 2014, August 2014.

[4] UGC Public Notice on draft regulations, September 2017.

[5] P Chaddah **Nature 511** (2014) 127.

[6] J Bailey https://www.plagiarismtoday.com/2017/12/12/the-weaponization-of-plagiarism/

[7] P Stephan, R Veugelers, and J Wang, **Nature 544** (2017) 411-412.

[8] P Chaddah, **Current Science 113** (2017) 1814-1815.

[9] P Chaddah, **Current Science 104** (2013) 405.

2. Choosing the research problem

2.1 Identifying research problems through literature survey

We have discussed in the introductory chapter that research starts with identifying a question without a known answer, where we believe we have the capability to help find an answer. The second step, constituting the whole research effort is then to find that answer, and to find new knowledge. The final stage is to disseminate our research output for its validation by experts.

Young researchers are often advised to dream big, and not settle for problems that appear easily surmountable. In the words of Kurt Wuthrich, a 2002 Nobel Prize winner, young researchers must "aim for the big fish" [1]. Realism requires that we must confine this choice to problems we can tackle in the period stipulated for completing our Ph D. While no rules can be stated for identifying the most challenging research problem that we can tackle in the stipulated time, this chapter shall try to present some guidelines for choosing the problem and for the research strategy. The guidelines shall be mainly for scientific research, coloured by the author's physics background. We consider the final part of public dissemination of our research output as an essential part of the research project, a part that is often ignored when we educate young researchers. Failure to ensure proper dissemination can result, as noted by Kochhar [2], in a discovery announcement ending up as a me-too report. This is the major thrust of this book and we will discuss some rules that have general applicability for research in all subjects.

Needless to say, the question we choose to address must emanate in an area where we have a background. It must be commensurate with our experimental/theoretical capabilities, and also with the facilities that are accessible to us. It thus appears to be constrained to be an extension of the previous research done by the group we work in.

2.2 Augmenting our capabilities

The constraints and restrictions placed by the various capabilities/facilities would naively imply that each successive research problem would not be drastically different. This is not usually true because every research group keeps augmenting its facilities, and every researcher is continuously reading and learning. Researchers consciously take breaks (or sabbaticals) where they work in different groups on new areas with new techniques, or they become aware of new developments from published literature. They can get excited about a new problem, or about a new technique. If the former, they worry whether they can use their capabilities to attack that problem in a new way. If the latter, they worry whether they can set up the new technique and what kind of problems would they then be able to attack. This kind of reinventing one's capabilities is a continuing process for every researcher. To sum up, the constraint of limiting to extensions of earlier research from the group is not really applicable! As we try to develop new capabilities, we have to ensure that we can develop these and complete our research in a reasonably short time frame.

If we concentrate on research efforts of Ph D students (they form the focus of this book), a student spends a period of 3 to 4 years in actual full-time research after completing the course work. The tradition is that the first half of this period is devoted to enhancing the research capabilities of the group. If the group does experimental research then this involves setting up a new instrument, or enhancing the experimental capabilities of an existing instrument. The second half of the research scholar's tenure is spent on utilizing the now-enhanced capabilities to do research.

The exact question which is to be answered, or the identification of the research problem, keeps undergoing suitable modification until the capabilities that are available for the research are now frozen.

The modifications are of course necessary because the capabilities acquired and now available may be at some variance from those that we were planning to achieve. More importantly, researchers elsewhere are also working on this and related problems. We have to stay aware of developments in the field by constant searches of literature. These searches are now-a-days made so easy by various search engines on the internet. Before the onset of the internet we would search journals for keywords of the topic being investigated, and for citations of authors who had landmark papers in that topic. (Searching for these citations is a good way to stay abreast of developments.) We now need to use a search engine on the internet because the titles, abstracts, and even reference lists, of papers that are kept behind a pay-wall by journal publishers may be accessed by popular search engines like google. And one can regularly

check the internet because only the same search string has to be clicked; one can even do it every morning! This enables us to know the latest developments and to reformulate our research goals if, since our last search, some answers have been provided by others working on the same problem.

The problem we identify must have captured our imagination and interest. We must be able to focus on it and work with dedication. It is obvious that we cannot build a perfect facility, or acquire absolute competence and all capabilities, in a 'reasonably short time frame'.

We thus assume that we have identified the research problem, and have to plan our research strategy. How do we proceed? There are no general rules that can be laid down, and we will only discuss some guidelines in this chapter.

2.3 The role of collaborations

The easiest and fastest way to augment our capabilities (this term includes both facilities and competence or knowledge) is to find collaborators who have what we need but lack. Collaborations built are usually continued for a long period, but this does not allow our augmented capabilities to expand every time. The collaborator and his group become a part of our team, and are involved in the research from the initial concept and planning stage. A collaboration that culminates in just one research publication, is not that common in this scenario.

Augmenting capabilities for just one research paper is, however, the norm when we submit a research proposal to a centralized user facility; examples of such a facility are synchrotron radiation sources, neutron sources, particle accelerators, an inter-university centre, or even an instrumentation centre. Such facilities are one-paper interactions, with the instrument scientist becoming the collaborator. The collaborator is not involved in the initial concept and planning stage. We submit the proposal to a user-time allocation committee, identifying the instrument required for completing our research. The instrument scientist enters the scientific research only if our proposal is accepted by the committee. (There is some genuine worry amongst those submitting research proposals for time on a user facility that such proposals may not be treated with the same confidentiality as is a manuscript submitted to a journal. They worry that if they disclose too many details of the research they propose to carry out, a better-equipped referee or committee member can try to carry out this proposed research faster and they will get scooped. We shall discuss how to protect against such possibilities in Chapter 9.2) There is an absence of continuous human contact, and collaborations in such situations are purely need-based; they present the most efficient way of enhancing our capabilities without future encumbrances.

2.4 Follow Chanakya ─ flow through a creek

In Chapter 1.3 we mentioned the case of working on a hot topic, where many researchers are trying to address the same question. It is possible, as mentioned in the Prologue, that one is thinking what

others have not thought and one has an out-of-the-box idea. If this thought process is at variance with what the many working on this problem are thinking, then questions will be raised in the review process when you submit your paper for publication. This is because the reviewer would be working in this hot topic, and biased towards the current thinking. And if there is any chink in your armor that is even not relevant to the new idea you are testing, like the sample you are making measurements on corresponds to what was state-of-art a year back but is not of the same quality as what others are currently using, then your paper could be rejected on those grounds because of 'reviewer bias'. And not because the reviewers can find fault with your out-of-the-box idea! The idea needs support from experimental data, and you are in a catch-22 situation.

The way out is to flow through a creek, and I illustrate this with our experience. In the period 1995-1999 there was a lot of research on the so-called 'half-doped manganites'. These were materials that showed a metal-to-insulator transition together with a magnetic transition from ferromagnetic to anti-ferromagnetic phase. The huge magnetoresistance associated with this transition raised the possibility of better memory-devices, and these materials were a hot topic of research.

Under certain conditions the two magnetic phases, and the two diametrically opposite electrical phases, were observed to coexist down to the lowest temperature achievable. This 'phase coexistence phenomenon' was intriguing, was being addressed by a large number of experimental groups, and theorists were trying to

understand its origin. The experimental groups were growing, and doing measurements on, the cleanest possible single crystals. We had an out-of-the-box explanation, attributing the coexistence of the two phases to an out-of-equilibrium state arising from the kinetics of the phase transition being hindered or arrested. We needed to support our explanation with new experiments, and with experimental observations that could provide a test to choose between various explanations. Unfortunately we did not have *single crystals* of these materials, could not establish a collaboration with those who could grow them (such researchers were in great demand!), and were conscious that any measurements on our polycrystalline samples would not be accepted by reviewers of well-established journals.

We had with us polycrystalline samples of another material that showed a magnetic transition from ferromagnetic to anti-ferromagnetic phase together with a resistive transition that was much weaker than a metal-to-insulator transition. The magnetoresistance was much smaller, the application potential of the 'half-doped manganites' was missing, and the research activity was not very intense or competitive. Dr Sindhunil Roy had done extensive work on these materials, with many papers published in well-established journals. The samples we had were thus already well-studied and would help us pass critical reviewers, as will be discussed in Chapter 3.1. We decided to follow Chanakya and use these 'doped $CeFe_2$' samples as a creek. Our ideas would be tested since the relevant physics of these 'doped $CeFe_2$' and 'half-doped manganites' was similar. But since the jump in electrical resistance was much smaller for 'doped $CeFe_2$', the potential for applications

was minimal and there was not any intense experimental activity with single crystal samples. The reviewers would not be unnecessarily critical and we would be able to publish our new ideas as we follow Chanakya and 'nibble at the edges' on the real physics issues in doped $CeFe_2$, while ignoring the 'capital' of magneto-resistance devices in the manganites.

During 2001-2002 we were able to publish our measurements, following unusual thermo-magnetic histories, in support of our idea that the coexistence of the two phases to an out-of-equilibrium state arose from the kinetics of the phase transition being hindered or arrested. Two of our papers appeared in well-established journals of the American Physical Society [3,4], and one in a well-established journal of the Institute of Physics of UK [5]. In these papers we emphasized the similarity of our samples with the half-doped manganites, and the relevance of our explanation for those materials. So, we were only using the creek to publish our out-of-the-box physics idea! We continued to pursue our idea, but were happy to note that our 'Chanakya' strategy was working. Magen et al [6] published in 2003 their work on Gd_5Ge_4, a material with potential for magnetocaloric applications, where they observed signatures of phase coexistence, and attributed this to kinetic arrest. Our three papers of 2001-2002 had influenced their work because these three papers were cited by them in detail, and the similarities with the half-doped manganites were also brought out by them. These concepts have gained further acceptance, with experimental support in many more materials, in subsequent years.

2.5 Ethical research

It is extremely important that one pursues research in an ethical manner. This is specifically important in the life sciences, or in the study of any living organism as also in behavioral research. It is necessary to report all results and measurement/calculation conditions truthfully so that validation by other experts is possible. One should not hide data points that are not fitting into the explanation that one is providing; it could be that these data points are signatures of the need for a new understanding. It is also important to give credit where due, and we shall discuss this in Chapter 4.

We must also not give credit dishonestly by having authors who made no contribution. Unethical research is when one indulges in fabrication of data, or falsification in the stating of our results or in reviewing existing research; or if one indulges in plagiarism while reporting our research. Of the various unethical practices, plagiarism takes place only after the research is completed and when we are writing up our research paper or report.

Plagiarism is defined as 'the appropriation of another person's *ideas*, processes, results, or *words* without giving appropriate credit'.

I have put emphasis on 'ideas' and on 'words'. All researchers would agree that between these two contents of a research paper, the ideas are the real claim to originality. Ideas being the first entry, and words the last, is because of the perceived importance

and not because of the fortuitous alphabetical ordering. As we stress the need for knowledge creation from our universities, we are obviously looking for ideas rather than words. Our leadership is emphatically encouraging original thought, or the creation of new (and 'out-of-the-box') ideas. We must worry about our ideas being plagiarized. We shall discuss this starting Chapter 6.

We are attempting to answer some question posed, or left unanswered, by existing knowledge. As noted earlier, the question itself might have been mentioned in the conclusion of an earlier thesis, or in a review article, and sometimes even in a journal paper. Since other research groups could be trying to find an answer, there is a competition to be the first to publicly declare our answer; we try to work fast and with minimal disclosures. *This competition and desire for priority necessarily creates the burden of doing one's own crosschecks to ensure reliability of results and conclusions.* This will be discussed in Chapter 3.

2.6 Researching with available capabilities: the realistic approach

In Section 2.2 we discussed the need for augmenting our capabilities so that we can provide a convincing answer. In Section 2.3 we discussed the benefit of developing collaborations, while in the previous section the competitive aspects and the desire for priority. These practical aspects can keep us from postponing our research till we have augmented our capabilities to the desired level, and also from seeking collaborations. We now discuss this

aspect with an example where immense contribution was made (and was so recognized) with far from optimum experimental facility (or research capability).

The example I shall describe is the path-breaking work of Bednorz and Muller [7] that discovered high-T_C superconductivity in copper oxides. This discovery created such frenzy, and was of such immense proportion, that the duo was awarded the Nobel Prize just one year after their initial paper was published [8].

The problem they were addressing was of universal appeal, viz. to try and find materials that undergo the transition to the zero-electrical-resistance superconducting state at higher temperatures than the then known limit of about 23K. The route they were following was unusual, but definitely not unique. While most were working on metallic materials, there were a small number of otherwise very prominent groups working on oxide materials with very low carrier density of conduction electrons. Subsequent to an 18-month sabbatical by the older K A Muller, in which he picked up interest and capabilities on granular superconductors, he and his younger colleague J G Bednorz started working on oxide materials towards the end of 1983 [8]. The measurement capabilities required for identifying the superconducting state were four-probe resistance measurement (for detecting the onset of zero resistance) and magnetization measurement (for detecting the diamagnetic signal of Meissner effect). For over a year these researchers were sharing another group's resistance measuring equipment during evening hours (and got their own in 1985), and they did not have a set up for measuring magnetization until September 1986 [8]. The

work was pursued, and communicated for publication in April 1986, with experimental capabilities being much less than what they were trying to establish for a comprehensive research effort.

Under such sub-optimal conditions they prepared a material that they knew had three (or more) phases, and based on data that would have been termed as incomplete by the so-called high-impact journals, they wrote up their conclusion that they had discovered a new superconductor with T_C around 30K in an oxide material [7]. They realized the importance of their breakthrough and performed checks to substantiate their conclusion. We shall discuss this part in Chapter 3. They also pushed for disseminating their research output in an era when the internet, with its concomitant possibilities, did not yet exist. We shall discuss this part in Chapter 6, which I consider as a sterling example of how to safeguard against getting scooped.

We conclude discussion for this chapter, drawing the conclusion from Bednorz and Muller's work, that one cannot suspend research until we get all the desired capabilities; one can fruitfully proceed even with limited capabilities as long as one is aware of these limitations, and is ready to think hard. The "thinking hard" part of Bednorz and Muller's work [7] will be discussed in Chapter 3.

References

[1] Kurt Wuthrich, (2018) as quoted on *https://telanganatoday.com › Hyderabad*

[2] R Kochhar, **Current Science 98** (2010) 1549.

[3] M.A. Manekar, S. Chaudhary, M.K. Chattopadhyay, K.J. Singh, S.B. Roy, and P. Chaddah, **Phys. Rev.B 64** (2001) 104416.

[4] K.J. Singh, S. Chaudhary, M.K. Chattopadhyay, M.A. Manekar, S.B. Roy, and P. Chaddah, **Phys. Rev.B 65** (2002) 094419.

[5] M.A. Manekar, S. Chaudhary, M.K. Chattopadhyay, K.J. Singh, S.B. Roy and P. Chaddah, **J. Phys.:Condens. Matter 14** (2002) 4477.

[6] C. Magen, L. Morellon, P.A. Algarabel, C. Marquina and M.R. Ibarra, **J Phys: Cond Matt 15** (20038) 2389.

[7] J G Bednorz and K A Muller, **Z. Phys. B 64** (1986) 189.

[8] J G Bednorz and K A Muller, **Nobel lecture**, December 8, 1987.

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3. Checks before dissemination

3.1 Recognizing what is new

Do not get scooped; do not get plagiarized

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Do not get scooped; do not get plagiarized

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necessary provisos. (The work on Cold Fusion by Pons and Fleischmann, who disseminated their work in a press conference as mentioned above, provides probably the best known example of the pitfalls of trying to impress non-experts!) Dissemination in an 'open-to-the-layman' forum can be justified only ***after*** anonymous (and confidential) pre-publication review, or post-publication open review, by experts have taken place. As was noted in Chapter1.1, one counter-example is enough to challenge a theory. Supportive comments from non-experts may be morale-boosters, but only experts and researchers in the area can find errors.

The necessity of sufficient checks before dissemination cannot be overemphasized. However, post-publication evaluations are much more thorough than pre-publication reviews, especially if the research results are significant. I also expect that in the near future some counterpart of social-media networks will develop for academic and research groups. To quote from the report of a British Parliament Committee [12], "the growth of post-publication peer review and commentary represents an enormous opportunity for experimentation with new media and social networking tools. Online communications allow the widespread sharing of links to articles, ensuring that interesting research is spread across the world, facilitating rapid commentary and review by the global audience. They also have a valuable role to play in alerting the community to potential deficiencies and problems with published work. We encourage the prudent use of online tools for post-publication review and commentary as a means of supplementing pre-publication review." Post-publication review comes in traditional journals only in papers which cite your work. These are

few and far between. Some journals are now allowing post-publication comments in their on-line versions. This practice is expected to increase.

References

[1]https://www.nobelprize.org/nobel_prizes/chemistry/laureates/2002/wuthrich-bio.html

Do not get scooped; do not get plagiarized

4. No plagiarism: quote and refer

4.1 Misconduct in research

Do not get scooped; do not get plagiarized

Do not get scooped; do not get plagiarized

Several articles are exhorting researchers to paraphrase; it is worrying that these articles are not restricting themselves to suggesting paraphrasing in the introductory section only. The benefit of this suggestion will be reaped by those who have good command over the language, and probably not by most of the young researchers in smaller towns of non-English speaking countries. *I would exhort researchers to be generous and give credit wherever it is due, rather than paraphrase and avoid giving credit!* It is best to quote from an earlier work, delineating with quotes the text corresponding to idea you are using, rather than paraphrasing. This is especially true if we are using earlier published research as a template; we must be extra cautious if we are pursuing 'me-too' research.

4.4 Plagiarism of results or ideas

Results and ideas are the essence of a research paper, and should only be re-used with generous citation and giving full credit. These are also where every researcher wants to protect ownership. We shall address these in great detail in subsequent chapters. At this stage we just note that there is presently no software to check plagiarism of results or ideas. There are only copyright laws that protect results that are presented as a figure. If we copy a figure or table, and cite the source, we are not guilty of plagiarism but we can be acted against for copyright violation! So use the data, redraw the figure, and cite the source; or take permission from the publisher of the earlier paper and they will also tell you how to cite it.

Do not get scooped; do not get plagiarized

The message on plagiarism is that we must ensure that others' credit is protected in our research output.

References

[1] https://journals.aps.org/prl/edannounce/PRLv93i13.html

[2] P Chaddah **Nature 511** (2014) 127.

[3] J Bailey https://www.plagiarismtoday.com/2017/12/12/the-weaponization-of-plagiarism/

[4] Editorial titled "Repeat after me", **Nature 488** (2012) 253.

[5] T V Padma, **Nature Medicine 13** (2007) 392.

[6] P Chaddah, **Current Science 106** (2014) 349.

[7] S B Roy, P Chaddah and Sujeet Chaudhary, **J.Phys.: Cond. Matt. 10** (1998) 8327.

Do not get scooped; do not get plagiarized

5. Precautions against 'self-plagiarism' charges

5.1 Why be careful about self-plagiarism

As was discussed in Section 2.4, plagiarism is defined as 'the appropriation of another person's ideas, processes, results, or words without giving appropriate credit'. Given the reference to "another person", self-plagiarism is often thought of as an oxymoron. While it is true that one cannot misappropriate or steal one's own work, it is also true that a research report is about creating new knowledge. It cannot be a rehash or a simple repetition of what is already published by us. Publishers of journals take serious objection to self-plagiarism, and the recently released draft of UGC Policy on Plagiarism in Higher Education Institutions states explicitly that "it also includes data plagiarism and self-plagiarism". Even the publications of the Indian Academy of Sciences state [2] "The editors of all the journals of the Indian Academy of Sciences take a very serious view of any evidence of plagiarism including self-plagiarism in manuscripts submitted to them." Self-plagiarism is an important allegation; it must be avoided.

Self-plagiarism refers to the manuscript having an overlap with earlier papers by the same authors, or by some of the present authors. Journals are very careful about such cases because it could involve copyright issues with other publishers, and accompanying legal hassles. Instances of self-plagiarism often result in the publishers retracting the paper with a prominent notice [3, 4]. The retraction notice usually does not refer to the copyright issues but

stresses the fact that a submission to the journal contains the undertaking that the authors are submitting original work [3]. Within this clause of lack of originality, self-plagiarism appears to fall in the same category as plagiarism of someone else's work. The accusation of stealing (from someone else) is, however, replaced by the accusation of submission under false pretense!

Since plagiarism and self-plagiarism are treated on the same footing, charges of self-plagiarism are also used as a 'weapon' in the same way as described in the previous chapter. As discussed in Chapter 4.2 social policing and attempts to 'name and shame' target the more prominent author [5]. Accusations of self-plagiarism can really damage one's career anytime in the future, and it is necessary to take precautions.

5.2 Precautions with submissions at conferences

We have to take special care when we present our research output in Conferences and seminars. As is well-recognized [6], we want our students to participate in conferences and present initial results for discussion and feedback. The work being presented must be already at the level of what can be submitted for publication in a peer-reviewed journal, and the conference provides an opportunity for discussion with experts that will be more intense and interactive than may be possible with the reviewers of a journal. The problem arises because of the details of our work being distributed by the Conference in the proceedings of the conference.

Many conferences publish their proceedings through international publishers that have separate 'conference series'. Organizers ask for manuscripts to be submitted before (even months) the conference. Acceptance for publication is often announced months after the conference. The visibility of proceedings being low, the data is often made part of a paper submitted by the researchers to a regular journal. Since the acceptance for publication in the proceedings takes time, there is some uncertainty on how to cite the submission to the conference in the manuscript submitted to a journal. This is where a window can open for allegations of self-plagiarism, and extreme caution is essential.

The paper that will appear in the Conference Proceedings, and the paper that will appear in the regular journal, are two publications on the same work. There is bound to be some overlap, and one of them (the later one) must cite the other. The sequence of events dictates that the paper in the regular journal must cite the paper in the conference, even if the paper for the Proceedings has not yet been accepted. This overlap must be brought out very clearly by stating that "the work reported in this paper was presented earlier in the following conference .." as a footnote in the manuscript submitted to the journal. If this is done then it preempts any accusation of self-plagiarism. Of course, all these issues become redundant if the Conference does not publish the proceedings!

Sometimes a Conference that publishes a proceeding has a good reputation and attracts many experts as participants. We participate to benefit from discussions, but can refrain from submitting our paper for such publication [6]!

5.3 Precautions while pursuing a problem, with a series of papers

We usually do not give up working on a problem after one publication; we pursue the same problem in a series of papers. In this case there is bound to be overlap in the introductory section. Often the results of an earlier paper have to be included to make the present report complete, especially since a common reviewer comment is that the reader cannot keep looking up your earlier works. This is particularly true if a new concept or protocol was introduced in the first paper. The reviewer rightly asks that the second paper must introduce or justify it in the subsequent paper also. We must refer to the earlier works; we must use quotations and cite the earlier papers as needed, to ensure that possible future allegations of self-plagiarism are avoided. Reusing of text within quotations, rather than paraphrasing it, is a good idea especially if a new phrase or keyword has been introduced in earlier papers by the same authors.

Data and figures are reused often and consciously whenever research groups pursue a problem and are also reused consciously when a review is being written. The reuse would require permission from copyright owners, with proper citations. Text may also be reused, either consciously for the reason mentioned in the previous paragraph, or subconsciously because the same or related question was addressed in earlier papers. To recognize such reuse, it is suggested that one can use the software that checks for text-plagiarism. When applied to the manuscript, we can cite our earlier papers wherever text-similarity shows up!

In all cases it is only ethical to make the reviewers aware of what is new, and what is not. This does not result in the manuscript being rejected. I can cite examples of papers in which I was an author where we reused figures after slight modification [7,8]. In these cases the figure caption (and the text) stated clearly where the data (or schematic figure) had first appeared. There was, of course, substantial new work to advance new conclusions. The reviewers accepted the advances and the papers were published. Similarly, I have been an author on introducing two new phrases that we repeated in our subsequent publications. We had reformulated the Critical State model for the magnetic response of hard superconductors [9]. We reasserted this in quotation marks, with an elaborate justification, as we applied this to a case where the original formulation was difficult to solve [10]. This helped us much later in necessitating a correction [11] when another author used our reformulation without giving us credit!

In another example, we had created a new measurement protocol that corresponded to cooling in a magnetic field where the analog of a glass could form, but heating in a different magnetic field in which the glass would devitrify. Following an advice of Kurt Wuthrich that we shall discuss in Chapter 11, we decided to identify our protocol as “cooling and heating in unequal fields” with the acronym CHUF [12]. We then used this protocol in subsequent papers naming it with the above six-word string and the acronym [13, 14]. It was also used by others who were sympathetic [15], the name caught on and so did our ownership of this idea! When other authors used “cooling and heating in unequal fields (CHUF)” without attributing credit to us, the correction came fast

[16]. By giving credit to our first paper we not only avoided allegations of self-plagiarism, but also ensured our credit!

The message on self-plagiarism is that refer to your earlier work not just to avoid charges of self-plagiarism, but more importantly to highlight your continuing and sustained contribution! A researcher reading one recent paper of yours should not miss linking to your earlier papers.

References:

[1] The draft University Grants Commission (Promotion of Academic Integrity and Prevention of Plagiarism in Higher Education Institutions) Regulations, 2017

[2] http://www.ias.ac.in/Journals/Overview/Academy_Policy_on_Plagiarism

[3] See e.g., N.K. Sahoo, S. Thakur and R.B. Tokas, **Appl Surface Science 427** (2018) 1280. The retraction notice from the journal states "One of the conditions of submission of a paper for publication is that authors declare explicitly that their work is original and has not appeared in a publication elsewhere. Re-use of any data should be appropriately cited."

[4] See e.g., S. Saha and T.P. Sinha, **Phys Rev B 75** (2007)069901.

[5] https://retractionwatch.com/2017/05/12/top-physicist-loses-another-paper-duplication-tally-now-7/

[6] P. Chaddah, **Current Science 102** (2012) 379.
[7] A. Lakhani et al, **Appl Phys Lett 99** (2011) 242503.
[8] S.B. Roy et al, **Phys Rev B 75** (2007) 184410.

[9] P. Chaddah et al, **Physica C 89** (1989) 570.
[10] K.V. Bhagwat and P. Chaddah, **Physica C 92** (1992) 444.
[11] A.A. Tulapurkar, **Phys Rev B 65** (2002) 099902(E).
[12] A. Banerjee, K. Kumar and P. Chaddah, **J. Phys.: Condens. Matter 21** (2009) 026002.
[13] P. Kushwaha et al , **Phys. Rev. B 80** (2009) 174413.
[14] V.G. Sathe et al, **J Phys Cond Matter 22** (2010) 176002.
[15] S.B Roy & M K Chattopadhyay, **Phys. Rev. B 79** (2009) 052407.
[16] T. Sarkar,V.Pralong and B.Raveau, **Phys Rev B 84** (2011) 059904.

Do not get scooped; do not get plagiarized

6. Disseminating new knowledge: Importance of priority and visibility

6.1 'First-to-disclose' gets priority

Ownership of all research output is as important as ownership of potentially patentable applied research. While most researchers are not seeking economic benefits, IPR (Intellectual Property Rights) implies that credit must always be assigned where it is due. In the case of patentable research discussed in Chapter 1.2, we do not try to widely disseminate our output. As has been noted recently by our Prime Minister [1] "what will drive innovation is IPPP- Innovate, Patent, Produce, and Prosper". Filing for a patent is obviously deemed to be more important than disclosing or disseminating the innovation or invention.

In the case of non-patentable research we try to ensure widespread dissemination of our research output, seeking acceptance from our peers and hoping for follow-up from other researchers. Dissemination for validation is really important, but it also helps us to ensure priority. In research there is no counterpart of a patent office where one can formally file and be the 'first-to-file'. Priority is given by the peer group, and is given to those who are the 'first-to-publish' or the 'first-to-disclose'. This can have some sanctity only if the 'disclosure' or dissemination must be date-stamped and citable.

Questions about the ownership of research output are not handled with the legal sanctity given to patents. However, that research

output is owned is clearly recognized by the fact that research papers are published with author names being displayed prominently. The authors are assessed, and their career growth is determined by their research output. Even further, universities and institutes are assessed and ranked by the research output of their faculty and scientists. Claiming ownership is necessary for ranking of individuals as also for ranking of institutions. Ensuring that we are not deprived of this ownership is thus very important. Just as the first-to-invent is expected to be the first-to-file, we also expect the first to conduct the research to be the first-to-disclose. The difference between 'disclose' and 'publish' is emphasized when delays in a journal review process result in losing 'ownership'. The group which published first can claim, often legitimately, that they were unaware of the work of those who may have been the first to submit and, thus, the first to disclose to the editorial office of a journal! This requirement of establishing priority brings us to the first part in the subtitle of this book, viz. 'do not get scooped'.

To establish priority and ensure ownership, the time gap between completing the report on our research output, and its being disclosed at an academically recognized forum must be kept as short as possible. We even cite an example from the nineteenth century to highlight that this has been recognized, and is reported to have worried path-breaking researchers like Marie Curie [2]. In recent times also, scientists use various available forums to disseminate, and it has been recognized by NIH that the outlet used is chosen *"to speed dissemination, establish priority, obtain feedback, and offset publication bias"* [3]. We shall discuss how evolutions on the internet have provided legitimate avenues for

dissemination with smallest possible time delay. Many of these avenues, because of various search engines that attempt to cover any document freely available on the internet, help in claiming priority almost immediately after the research report leaves our desk! While many such avenues are available, not all have acquired legitimacy in the academic world. In Chapter 8 we shall discuss some avenues that have acquired legitimacy.

6.2 Ensuring visibility by publishing in suitable journals

This section deals with an issue where research is clearly distinguished from inventions ─ a person filing a patent is not actively seeking a feedback. There are however lessons to be learnt from how patents are filed. We note that filing a patent requires that we clearly state what is novel. Similarly we must also be very clear about what the new contribution in our research output is before we start writing our manuscript, or disseminating our research results in conference presentations. In our attempt at gaining priority and visibility, we have to ensure when we write a report to disseminate our research output that anyone who comes across our report should be able to easily grasp what our new contribution is. Further, anyone who is searching literature for keywords related to our new contribution should get a link to our report. This would ensure that anyone interested in the problem that we have addressed does not miss our research report, or that our research report has high visibility.

There are preferred journals for each specialty that experts scan at regular intervals. Since we are doing literature survey before and

during our research, we must be aware of these relevant journals, including the type of reports each publishes. We must choose a suitable journal for communicating our research report, and not submit to a journal that is unlikely to be scanned by those we wish to target. *Since we want our peers to read our paper, we try to publish in journals that our peers read!*

We must realize, and be careful, that publication could get delayed in the refereeing process. We must choose a well-read journal, but also not submit to a journal that is likely to reject our paper as 'not of enough novelty'. Such rejections are done after some time-consuming review, and appealing against a rejection by reviewers cost us even more time. This translates to loss of priority and, as discussed in Chapter 3.2, exposes us to the risk of having leaked our results and getting scooped [4]. The system for evaluation of researchers, in many countries like China and India, goes by where you publish rather than by what you publish. This puts a premium on publishing in high impact-factor journals. Researchers often start with submission to a high-impact-factor journal, keep getting a rejection as we submit to journals of decreasing impact-factor, until we finally submit to a suitable but lower impact-factor journal! The contrast between choosing a suitable journal straightaway and finding a suitable journal only after sequential rejections makes the difference between getting credit for being the first, and being an also-ran. We have to strike a balance and submit to the journal of highest visibility amongst the journals that are likely to accept our paper!

There is still a nagging worry that we have published our paper in a journal where it does not get the visibility it deserves. This can be taken care of by maintaining our own mailing list of experts in the specialty. Before the onset of emails and the internet we used to wait for our paper to be published, order reprints from the journal where it appeared, and mail (post) these to all on our mailing list. While this appears to be an effective way of increasing visibility, we actually publish also for those who are new entrants to the specialty. These were obviously not included in our mailing list of experts, and new entrants could not be informed of our work through this practice of mailing reprints.

In the internet era, we try to ensure that the full paper is easily available to anyone who is slightly curious. This can be done by uploading our manuscript on preprint –hosting sites, or by making our paper in a journal openly accessible. The latter option is an 'author-pays' mode of publication, in contrast to the conventional 'reader-pays' mode where the institutional library subscribes to the journal. While the financial aspects of publishing evolve, the correlation between the 'reads' that a paper gets and the impact factor of the journal in which it was published, is bound to weaken. It is recognized that the internet has transformed the role of scholarly journals. Their role has shifted from disseminating research to putting a stamp of validity ─ we shall discuss this further in the next chapter.

6.3 Enhancing visibility using the internet

The advent of emails made it very convenient to mail copies of our published paper to our personal list of experts, both in terms of effort and cost. The creation of the internet allows us to post these papers on our institutional websites, on our personal URL sites, or on sites like researchgate.net. However, there are copyright issues involved and the as-published version cannot be uploaded without permission. Self-archiving policies of individual journals are available (see [5]) and have to be complied with.

The advantage of uploading on such sites is that the papers are then freely available, full-text, to search engines like google. It is important that keywords are given, and the novelty of the research is brought out in the abstract. Literature survey in the present internet era is through these search engines, and your paper will be amongst the top 'hits' of a search process if the words being searched for occur in the paper at such places of prominence. Such uploads can allow a visibility that could be higher than that of the journal your paper appeared in, because the detailed paper published in the journal is behind a pay-wall and may not be seen by various search-engines. Enhanced visibility is essential if you want credit for your original contribution to knowledge, and provides moral legitimacy similar to what patents provide to inventions.

Why is it necessary to ensure credit for the original contribution to knowledge? Assigning due credit for knowledge creation is an ethical responsibility of a knowledge driven society. More

importantly, ownership of an idea is in itself a reward and its acknowledgement serves as a driving force for normal humans. Intellectual Property Rights (IPR) is discussed in international forums in an academic spirit, without reference to its actual monetary implications. International evaluations rank Universities and this is determined also by the new knowledge that their faculty and students create, and by its impact. The impact will be there only if due credit is being given. Thus for a university to get its rightful ranking, it is essential that credit for research is appropriately apportioned and that it is not wrongfully appropriated. Claiming ownership while creating new knowledge must be a primary concern and responsibility of every research supervisor, and of every university. As argued earlier, ideas and results are the essence of new knowledge. Unfortunately, software checking for plagiarism cannot establish that credit, for ideas and for results, is being correctly apportioned. At present, authors have to put in some effort to ensure that they get due credit. Such efforts will be discussed in later.

Young researchers, and researchers from new universities, are more susceptible to their credit being usurped. *In my opinion, scientists who plagiarize ideas and results are usually established researchers who can assess the validity of published work and probably also find it easy to have publications of their own accepted and cited.* [6]

6.4 Register priority while avoiding visibility ─ to gain an edge

We may not always want high visibility. In Chapter 2.4 we discussed a situation where we were avoiding visibility but only from a highly competitive, and probably opinionated, group. We now discuss a situation where the researchers knew they had a breakthrough, needed to record it to establish priority, but were avoiding visibility to gain time (or have a head start) over others who might start competing in subsequent studies as soon as they became aware of the breakthrough.

As was described in Chapter 2.6, Bednorz and Muller had concluded that they had found superconductivity at an unexpected high temperature, even though they did not have measurements to confirm the Meissner effect. In their words [7] "*we rated the importance of our discovery so high that we decided to publish our findings, despite the fact that we had not yet been able to perform magnetic measurements to show the presence of the Meissner-Ochsenfeld effect.*" They communicated their paper on 17th April 1986, and the Meissner effect test became possible much later as "*in September 1986, the susceptometer had been set up and we were all ready to run the magnetic measurements*" [7]. It is to be noted that they did not seek a collaborator's facilities for these measurements, probably to retain their ownership. That they had submitted their manuscript to a lower impact-factor journal gave then a head start, but they were still worried as soon as their paper was published even in this low-visibility journal. To quote them, "*Realizing that our first paper had appeared in the open literature,*

we rushed to get the results of our susceptibility data written up for publication" [7].

This was a text-book example of how low-visibility dissemination was used to establish priority, and to gain time for a head start to complete other possible experiments. It is an excellent example of planning competitive research, and planning its speedy dissemination!

References:

[1] https://www.narendramodi.in/innovation-has-the-power-to-overcome-the-challenges-our-world-faces-pm-modi-at-smart-india-hackathon-539505

[2] https://en.wikipedia.org/wiki/Marie_Curie

[3] NIH (2017) Reporting preprints and other interim research products. https://grants.nih.gov/grants/guide/notice-files/NOT-OD-17-050.html

[4] C Laine, **Ann. Intern. Med. 166** (2017) 149.

[5] http://www.sherpa.ac.uk/romeo/

[6] P. Chaddah, **Nature 511** (2014) 127.

[7] J G Bednorz and K A Muller, **Nobel lecture**, December 8, 1987.

Do not get scooped; do not get plagiarized

7 Dissemination and Publication: comparisons

7.1 Dissemination is necessary for validation

The instant communications over the internet have revolutionized interactions and information dissemination. The ability to handle large data stored over the internet has made instances of cut-and-paste plagiarism easy to detect. The distinction between formal and informal modes of dissemination is continuously reducing. The importance given to accusations of self-plagiarism in formal dissemination require us to be careful while disseminating continuing research, to ensure that we do not disseminate even the same word-string twice without appropriate citation. We shall take a detailed look also at what are (or have been) considered as informal modes of dissemination, specifically addressing whether these can help us fight back if we are plagiarized. On the other hand, we worry if some of these informal modes of dissemination make us susceptible to being scooped.

As was discussed in Chapter 1, dissemination is necessary before any research output can be tested independently and accepted as valid, and is a pre-requisite for our research output to be accepted as an addition to human knowledge. Dissemination is also the only means to claim priority and thus claim ownership.

For nearly a century now publication in scholarly journals had been considered to be the accepted means of dissemination of new research. Publication implies submitting a research paper to a scientific journal, where some expert (or a few experts) forms an

opinion on it that could vary from 'publish as submitted', to 'publish with minor modifications', to 'publish with major modifications and after some more research' to 'not worth publishing in this journal'.

This process is referred to as 'pre-publication review' and is supposed to validate the research output [1]. Peer review is supposed to determine the relevance of articles, check their originality including whether credit has been given where due. It is supposed to prevent plagiarism of results and ideas as experts should be conversant with developments in the field, going well beyond the robot-like process of checking text-overlap as is done by software that have become so popular. As we shall discuss below, peer review cannot be expected to check deliberate fraud. The Schon scandal [2], exposed in 2002, emphasized through the retraction of over twenty papers in high-visibility journals, such frauds have been discovered only when other researchers have been unable to reproduce the published work.

The present convention of publications in research journals is a benchmark of the scientific method and its origin can be traced back about three centuries ago, but the advent of the internet and the growth of networking tools have led to serious discussions on replacing this with post-publication peer-review [3]. A move forward has occurred with the growth of preprint repositories [4], with very limited checks before a research paper is uploaded (as will be discussed later, various repositories also have a 'prestige-index' depending on their popularity). A step backward has occurred with the advent of the so-called predatory journals, which

publish for a small fee but provide visibility on the internet [5,6]. Scientific publishing is currently in a flux as even reputed publishers have started charging enormous publication charges, ostensibly for providing open-access to readers. But they also ask the reviewers to be less strict. It is against this backdrop that the UGC has put out a list of 'approved list of journals' for evaluation of Ph D students and of faculty. But there are criticisms that many journals in this list are actually predatory and are unacceptable [7].

We must realize that publishing in this increasing list of journals, with questionable review procedures, makes us susceptible to losing credit. Many researchers would assert that they do not look at papers published in such journals. Any new idea published in such journals can thus be picked up (most of them are 'open access' and a search engine would read them) and used by unethical competitors without attribution. And they would claim that the idea was developed independently. Since no peer would actually be perusing such journals, it would be difficult to have experts defend your claim. I may mention here the example 3 shown in Appendix. The correction published in the highly established journal Physical Review B gave credit to our earlier reports in a Conference in India, and in a journal that was not highly visible [8]. Fortunately, experts were aware of our work because I had sent them reprints. Else, it would not have been easy to get the correction that acknowledged our priority.

Dissemination of new research results can be informal, with no way of claiming priority for a result or for an idea. The most common occasions where one makes informal presentations are

while giving a seminar, or while making a presentation for funding of a project, or while seeking time on a big-science or a central facility. In each of these cases the presentation is planned in advance, but there is no record that can be used to claim priority. The possibility of being scooped rises with the fraction of unknown/unfamiliar experts in the audience. One must be cautious while planning such presentations, especially that no first-time releases of new ideas or plans for research are made to committees while seeking time on a big-science or a central facility. Your request for time can be rejected, leaving you without the facility required for pursuing your idea and allowing competition to step in. Competitive research can be cut-throat! *No one should steal my idea*.

Dissemination of new research results can be uncontrolled, as while showing a visitor around, or while commenting or asking a question in a seminar. An uncontrolled release of results implies that you are some time away from writing up your report, and again no record is being kept of such dissemination. These spontaneous interactions require some care.

The next level of informal dissemination is when one makes a presentation in a Conference where the proceedings are not being published. Scientific discussions at such meetings are an essential ingredient of research. I will suggest some precautions in Chapter 9 so that one can pursue such scientific discourse without becoming paranoid [9].

Special caution applies when the conference organizers ask for manuscripts, release them in electronic form to participants, but do not publish them. This does save the authors from possible accusations of self-plagiarism, but does not protect their research from being usurped by other participants (or, more worryingly, by non-participants who have access to the soft copy of the manuscripts). I would strongly urge desisting from participating in such ventures without suitable safeguards discussed in Chapter 9.

7.2 Validation prior to dissemination: refereeing by journals

The formal and traditional method of dissemination is through publication in a peer-reviewed journal. I have already discussed the uncommon case when a reviewer becomes unethical. Every reviewer is required to assure the publishers that (s)he has no conflict of interest, and that the manuscript received for review will not be shared with anyone. The former may not be true if the reviewer is an expert in the same area (a requirement desired for a thorough review), and the problem attacked is of great interest. The reviewer is anonymous but cannot misuse the privileged information obtained on reading the unpublished manuscript because the access provided to the reviewer is on record with the journal. This has allowed misdemeanors to be identified and corrections obtained, as was discussed in Chapter 3.3. In one such well-documented case, the aggrieved authors were from an Indian university, and the errant reviewer was from a developed country [10]. However, some visitor to the reviewer's group (from another group with similar interests) can obtain unauthorized access, and

that would not be on record with the journal that sent the manuscript for review.

There are also occasions when an under-review manuscript is delayed for some reason, and a paper appears in another journal with similar results from another group. While we may suspect unethical behavior, it is also true that authors from less-established institutes (as common to India) often find papers submitted later than ours appearing earlier. There is more skepticism amongst referees who ask more searching questions, and need to be convinced about our experimental capabilities!

A third issue is how reviewers deal with claims of novelty. One of the papers from a established group had claimed visually striking results with a "specially designed" measurement protocol, without attributing the creation of that protocol to anyone, thus implicitly usurping credit for it. The reviewers let that pass, maybe biased by the eminence of the authors, even though that protocol had been published in various journals (including the same journal) for over two years. This is euphemistically referred to as 'publication bias'!

In some cases where the journal concurs that an ethical misdemeanor has occurred, it asks the authors to publish a correction. In many cases the content of the correction published by the journal does not meet the expectation of the aggrieved authors. In particular, the journal should ensure that every download of the errant paper is accompanied by simultaneous download of the correction. This can now be done by simply making the correction a part of the pdf file of the main paper.

Unfortunately, many established journals are not agreeing to this simple suggestion.

Since corrections presently offered by various journals are not prominent (probably because such corrections do indicate some lacunae in the review process of the journal), there has recently been a call that an ethics body must post all journal errata that add new references to Indian work [11]. *As we value papers published in an established international journal, we must remember that these journals are hesitant to accept mistakes in their review process as these might tarnish the publisher's reputation*.

7.3 Is thoroughness of refereeing linked to reputation of journals?

We have given one glaring example of the generally accepted fact that flawed results still get published [2]. Publication only implies that a small number of experts have not found a flaw.

The second problem is that the peer review system does not easily accept unconventional ideas, and stifles innovation or out-of-the-box thinking. In many cases where reviewers ask for modification, some additional research work is asked for. But in many cases the reviewer wants that some specific earlier papers be referred to, and the conclusions be modified in the light of those earlier papers. The reviewers also state that the paper can be published once these changes are made. It is a common experience that in the process of ensuring publication authors, especially the young and less established researchers from developing countries, often dilute/modify their conclusions. They succumb to subtle or less

than subtle pressure exerted by reviewers'/editors' against their new ideas that question the commonly held view/s. This is because of the lacunae in our assessment system, where young researchers get some benefit once the paper is published. Unfortunately, such pressures from reviewers work easier if the journal has a high impact factor!

Some journals, on the other hand, look for earth-shaking results, tempting scientists to make claims that are unjustified. Such attempts are usually due to one or the other kind of material benefits to researchers that follow their publication. There have been reports [12] that journals with higher perceived prestige value also have higher retraction rates! This has been attributed, on the negative side, to authors being less honest and cutting corners to get a publication in such prestigious journals. On the positive side, this has also been attributed to higher visibility of the given journal resulting in a higher level of scrutiny.

Such retractions of published papers are examples of post-dissemination (or post-publication) review at work. It follows that *dissemination without delay but with a high level of visibility ensures both (i) ownership of the researchers and (ii) a proper post-dissemination validation and evaluation of the research output. Validation of major path-breaking research output has always been linked to the post-publication acceptance by the community of researchers in the field, and not just to its being published in any journal, however 'reputed' it may be.* What we publish will, definitely in the long run, be more important than where we publish it.

References:

[1] M. Baldwin, **Physics Today 70** (2017) 44-49.

[2] M.R. Beasley et al, "Report of the Investigation Committee on the possibility of Scientific Misconduct in the work of Hendrik Schon and Coauthors" *(pdf). (2002) Bell Labs.*

[3] House of Commons Science & Technology Committee Report (2011) https://www.publications.parliament.uk/pa/cm201012/cmselect/cmsctech/856/856.pdf

[4] P.D. Cara, R. Ciriminna and M. Pagliaro, **ACS Omega 2** (2017) 7923-7928.

[5] R.V. Noorden, **Nature 495** (2013) 426-429.

[6] S.C. Lakhotia, **Proc Indian Natn Sci Acad 83 83** (2017) 33.

[7] S.C. Lakhotia, **Proc Indian Natn Sci Acad 83** (2017) 513, B. Patwardhan et al, **Current Science 114** (2018) 1299.

[8] W.R McIntire, **Phys. Rev. B 16, 2994 (1977).**

[9] P. Chaddah, **Current Science 102** (2012) 379.

[10] P. Chaddah, **Current Science 111** (2016) 979.

[11] P. Chaddah, **Current Science 106** (2014) 927.

[12] C. Woolston **Nature 513** (2014) 283.

Do not get scooped; do not get plagiarized

8. Highlighting new knowledge in e-print repositories

8.1 Modus operandi of e-print repositories

Authors sometimes share their manuscripts with known specialists in the expert community for obtaining comments and feedback, prior to submitting to a journal. As discussed earlier, one has to tread with caution if the results being distributed through this informal route are surprising, or the ideas are path-breaking.

Preprint libraries have existed in a few specialties for a long time. Preprints were distributed and records maintained for priority, and the numbers given could be used for citation purposes. There was no such relevant topical library in condensed matter physics when Bednorz and Muller submitted their findings, but one such library was created in the flurry of activity that followed their work! The High-T_C Newsletter was launched prior to the advent of the internet and used to be delivered by post. It contained titles of preprints submitted (again, by post) to the administrator, with commentaries on those of them that the administrator considered interesting.

The advent of the internet changed things. An electronic bulletin board was created in 1991, for uploading articles in theoretical high-energy physics, which would be stored for a short period of a few months [1]. The initial site hep-th@xxx.lanl.gov has evolved to become http://arXiv.org and has become the trend-setter in sites that host and archive preprints. This site now hosts uploaded preprints for posterity, and no article uploaded in over 25 years of

its existence has ever been deleted. This arXiv site is central to how researchers in physics, mathematics, biology, and, increasingly, researchers in other disciplines function. After initial reluctance, more and more journals are now accepting manuscripts that have already been uploaded, and thus 'published', on this site. The founder of arXiv had expressed doubts on the future of traditional journals that are published through funding by readers, and in the newer 'open-access' models, by authors. The most famous case is that of the mathematician Perelman, who published proof of the Poincaré conjecture exclusively on arXiv. This proof won the Fields Medal and also the Millennium Prize, and never published in a peer-reviewed journal, stands out as a landmark case of post-publication review!

Such preprint archives have not yet been able to challenge the existence of journals for the reason that most assessing agencies still go by where a work is published and not by an expert assessment of what is published. Hopefully, this will change.

Preprints are manuscripts that are not yet peer-reviewed. Authors use these to get informed feedback from a large number of peers, helping them revise articles before submission for formal publication. The present modus operandi of arXiv provides a platform for permanently storing soft copies of such manuscripts, so that they can claim priority. As early as in 2005, the American Physical Society had acknowledged this role by stating "Citations should be as complete and up to date as possible and can be drawn from e-print archives as well as peer-reviewed journals" [2].

These archived preprints are thus also citable like any other published paper, and as per the 2005 notice cited above, need to be taken cognizance of while attributing credit (or ownership). These pre-print archives provide a major advantage to authors since the submitted manuscripts become available freely within a working day of being uploaded, subject to some essential and sensible restrictions. This is important when there is a time gap between the first submission of the paper to a journal, and its final publication. It is during this time interval that we worry most about credit being usurped by someone gaining unauthorized access to the manuscript. This speedy dissemination particularly helps researchers who are not yet established to ensure that their papers do not get delayed by skeptical reviewers, who ask questions and seek assurances that they would not do with established researchers. These pre-print archives can thus prevent delays in establishing credit for original ideas.

The restrictions on uploading are implemented by moderators who only ensure that the submissions are suitably classified, have proper content, and check for levels of text overlap with earlier submissions. In the case of text overlap the authors are asked to make modifications, otherwise a comment is put with the uploaded title indicating the extent of overlap and the preprint with which the overlap occurs. This comment, like other comments, appears with the title and is not hidden in any search result. This is an ethical practice that can have far-reaching consequences.

Once a preprint has been uploaded on arXiv, the manuscript cannot be withdrawn; it remains on the internet for perpetuity. This feature ensures that authors maintain some standard because their reputation is at stake. The pre-print repositories allow modifications, with all the versions remaining freely available for perpetuity. Besides corrections, they can also include supporting results obtained subsequent to the publication of the corresponding journal paper; in a journal you have to submit a fresh paper, which is likely to be rejected on the grounds that this has only incremental value and is not a substantial piece of work. The arXiv.org model of providing the updated version as the default download should, eventually, also be pursued by all reputed journals, especially in updates that correct for possible plagiarism.

When the pre-print manuscript or its modified version gets published in a formal journal, author/s can add a note on the archived pre-print that provides link to the published paper. They can then provide open-access manuscript versions of papers published in journals that are 'reader-funded', or can be downloaded only against a subscription or on payment to the publisher. These manuscript versions should, in the default case, be the version of the manuscript as was initially submitted to the journal. Some journals may object to uploading any version that has been modified as a result of the review process.

Just as journals are proliferating, so are sites where preprints can be uploaded. It is thus natural that one has to choose an appropriate preprint repository for uploading. One set of guidelines has been made available by NIH [3], but there are no accepted norms yet.

8.2 Increasing acceptance of e-print repositories

As has been noted above the highly popular preprint repository arXiv.org was launched in a small way in 1991, and physicists and mathematicians were quick to accept this practice of widespread pre-refereed distribution of research papers. Purely because of the way physicists accepted it, arXiv provides a 'date-stamped priority claim' [4], which is now accepted by established journals like of the American Physical Society and, more importantly by the community of physicists. As noted by Ginsparg [5] "the scholarly benefit of voluntary participation in the incipient version of arXiv.org in 1991" were obvious and, according to him, are paralleled by the enthusiasm with which users post videos on YouTube! While there was some hesitation if established journals would accept manuscripts that had already been uploaded on arXiv on the ground that the results were already circulated and thus did not meet the criterion of originality (as noted in Chapter 5.1 this clause is often used by publishers while retracting papers for self-plagiarism), Ginsparg [4] notes that "arXiv was a fait accompli before any journals were online. Authors had established their clear preference to continue using it, and journals cannot risk alienating their authors". In fact established journals talk of journals and arXiv complementing each other in that "results are already being disseminated while the peer review proceeds at its deliberate and thorough pace" [1]. Many journals already allow online submission by simply submitting the arXiv reference, while others hope to reach that stage [1]!

This happy coexistence between arXiv.org and physics journals has been accepted as evident by a correction in Physical Review B that acknowledges three papers on the arXiv that were uploaded in the period between the submission of the original and revised versions of the manuscript [6].

The widespread use of arXiv.org by physicists is also influencing other disciplines. The repository bioRxiv was launched in November 2013 by the nonprofit Cold Spring Harbor Laboratory as a biologists' version of arXiv, and has become quite popular. The recent recognition by NIH of the reasons why preprint repositories are becoming popular is bound to encourage biologists to "speed dissemination, establish priority, obtain feedback, and offset publication bias" [3]. And, as noted by Cara et al [7], "Preprints have the intrinsic ability to solve the main problem of the peer-review process, such as a delay in publication. Such delays can be a significant challenge. Some authors are concerned about a refereeing bias, against authors from emerging bylines, or when authors propose ideas different from the mainstream [8]. It is perhaps not surprising that the need for a national preprint repository was first identified for a huge and rapidly developing country such as India, urging young minds to preprint their work on the new national online archive to ensure priority [6]."

Cara et al [7], while making an analysis of why the chemistry community has been slow in accepting preprint repositories, also list a large number of preprint repositories that are now available. As with journals, so also with preprint repositories; authors will gravitate to those where their preprint will gain maximum

visibility. The repository market is growing rapidly, and some agencies are now suggesting guidelines for choosing amongst them [3].

8.3 Cross-checks and metrics in reputed repositories

The most reputed preprint repository is arXiv.org and some others are following its lead. As an example Paul Ginsparg, the physicist who started arXiv, serves on bioRxiv's advisory board. We briefly note, therefore, some features that arXiv has that are not common among standard peer-reviewed journals.

First is the minimal listing of papers, corresponding to the contents page of a traditional journal. In addition to the title and names of the authors, this has an entry titled 'Comments'. This is an entry not encountered in journals, and here the authors can write about 20 words (though no rigid limit is stated). It has been used by some authors to present an abridged abstract containing the essence of the novelty in the paper, though most just mention the number of pages or number of figures in the manuscript! The arXiv admin checks all submissions for text overlap with other arXiv articles. If any overlap is found then there is a mention 'text overlap' or 'substantial text overlap' in the 'Comments'. It also mentions whether there are no common authors between the two articles. No such information is available in regular journals, and definitely not in the Contents page. One of the greatest differences is how arXiv.org handles ethical corrections. Most journals have such corrections, with an apology or regret, as separate entries;

arXiv.org puts it in the 'Comments' where it cannot be missed and draws immediate attention, as in ref [9].

Through its linkage with NASA ADS, arXiv.org also provides metrics like year-wise number of downloads, and year-wise information on citations to the paper. Also, a full list of papers that cite your upload is available.

Just like this preprint site was a trail-blazer in online publishing, we can expect that many of these features of this preprint site will be followed by established publishers.

References:

[1] Editorial "Keep posting", **Nature Physics 12** (2016) 719.

[2] https://journals.aps.org/prl/edannounce/PRLv93i13.html

[3] NIH (2017) https://grants.nih.gov/grants/guide/notice-files/NOT-OD-17-050.html

[4] P. Ginsparg, (2017) arXiv: 1706.04188.

[5] P. Ginsparg, (2011) arXiv: 1108.2700; **Nature 476** (2011) 145.

[6] P. Chaddah, **Proc Indian Natn Sci Acad 82** (2016) 1167.

[7] P.D. Carà, R. Ciriminna and M. Pagliaro, **ACS Omega 2** (2017) 7923.

[8] P. Chaddah, **Current Science 103** (2012) 350.

[9] T. Sarkar, V. Pralong and B. Raveau, (2011) arXiv: 1106.6253.

9. Presentation at conferences: advantages & cautions

9.1 Obtaining feedback

Scientific discourse with peers is one of the joys of a research career, as one interacts with bright minds. Collaborations will be built on such discussions, and are sustained by mutual trust. We do want students to participate in conferences where they discuss their initial results, and these must evolve into a publication in a peer-reviewed journal. The recently notification by the MHRD of UGC regulations has codified this tradition into a rule [1]. As stated therein *"M.Phil scholars shall present at least one (1) research paper in a conference/seminar and Ph.D. scholars must publish at least one (1) research paper in refereed journal and make two paper presentations in conferences/seminars before the submission of the dissertation/thesis for adjudication, and produce evidence for the same in the form of presentation certificates and/or reprints."* Papers and research output is now required to be presented at conferences.

In addition to the joys of participation mentioned above, conferences provide a learning experience through formal lectures, paper presentations, and poster presentations. Statements made during presentations and discussions while explaining a poster correspond to 'informal dissemination', as do the highly stimulating discussions that can take place at such meetings. I received an email from a Japanese researcher in forensic medicine (this cuts across all disciplines), who felt that his presentation at a

Conference had been used by a competing group to complete their own research. They published their own paper and made no reference to his conference presentation, and he had been scooped. When he confronted this competing group, they ultimately told him that it was his fault for presenting without publishing. I had to explain that the competitors were unethical, but they had told him the harsh truth. We have to learn to protect our priority.

Discuss, but with judicious restraint. Let me illustrate. If you have some anomalous data, or data which you are able to explain with an unconventional idea, then present your result without an explanation. This will give you an opportunity to learn if someone else is aware of a more conventional explanation. Or someone may point out some experimental artifacts that could be the possible cause of your anomalous data. You could check for these, and the novel explanation may not be required.

Of course, the conference provides others with an opportunity to use you also as a bouncing board! I agree with and reinforce all standard benefits of active participation in conferences.

We now discuss some precautions that are necessary while submitting papers to conferences [2-4].

9.2 Ensuring priority and avoiding being scooped

There are many reputed conferences in India that require submission of detailed manuscripts before they will accept your paper, and allow you to present your work. This implies limited

release of your manuscript a few months before the actual presentation. I am hesitant about this, because there is no record of who has access to my manuscript during this long period [4].

This drawback can be circumvented by the following actions

- *Upload the manuscript on an E-print archive simultaneously with submission to the Conference.* Priority is established because all experts in the field have access to your manuscript at the same time as the Conference organizers. The uploading on the E-print archive has provided you a date-stamped priority claim. You can still submit the manuscript to a regular journal at any subsequent date.

- Present all details in the manuscript at the Conference and have a complete and fulfilling discussion with experts. If you learnt during these discussions, modify your manuscript accordingly (and acknowledge those with whom such discussions took place) and upload a revised version (v2) on the E-print archive. Both versions on the E-print archive are date-stamped for legitimate priority. The acknowledgement for discussions is a time-honoured practice that also puts some responsibility on those you had discussions with; they cannot feign ignorance of your conference presentation!

9.3 Avoiding self-plagiarism concerns

I also emphasized in Chapter 5.2 the possibility of being later accused of self-plagiarism, when you convert your conference

presentation into a regular paper [2-4]. Many of our national conferences now publish proceedings through 'conference series' of international publishers. Because of this, the authors have to worry about charges of self-plagiarism. Basically, if any of the data are to be incorporated in the journal submission, the journal manuscript must cite the conference paper which is 'published' in the 'conference series', even though the impact of this 'conference series' is near zero (the impact factor is usually listed as 'not available'!). There is a distinct possibility that the reviewers of the journal may then reject the journal submission claiming that there is not enough new data or analysis, beyond what is being published in the 'conference series'. The alternative of presenting very little data in the conference, so that the journal submission has a lot of new data, is also not correct because it defeats the purpose with which the student attends a conference.

Since the proceedings are usually published many months after the conference, the research scholar may submit the journal paper without being able to properly cite the conference paper, and the reviewer may accept the journal paper. This is dangerous because the overlap will be on record once both papers have been published, and can be detected by hobbyists or political opponents many years later, and damage a career. Some years ago, as commercial software for checking text-plagiarism was being introduced, there was a report in Current Science titled 'Publish and perish' that asked 'Should one's career be ended or marked forever due to a few misdeeds?' [5]. It is a real worry, because the 'misdeed' of self-plagiarism was done as a Ph D student, and

hobbyists and political opponents will raise the issue decades later when you occupy an important position!

These are drawbacks of presentation at such Conferences, some of which are very reputed and are attended by senior scientists. The drawbacks can be circumvented by uploading the manuscript on an E-print archive simultaneously with submission to the Conference. If the E-print archive has a larger readership than that of the Conference Proceedings, then there is no scientific./academic benefit of publishing in the Proceedings except that it may appear on the list of UGC-approved journals and have consequent career benefits. But the UGC-approved list is under discussion, and may be corrected [6,7], and the drawbacks of having published in such a Conference Proceedings would be overwhelming. *So, do not publish in the Proceedings. This is done very simply by not signing the copyright transfer form.* And submit the revised version (v2) to a regular journal without any worry about self-plagiarism allegations in future!

References:

[1] University Grants Commission (Minimum Standards and Procedure for Award of M. PHIL./ PH.D Degrees) Regulations, 2016, Clause 9.4.
[2] P. Chaddah, **Current Science 102** (2012) 379.
[3] P. Chaddah, **Current Science 111** (2016) 979.
[4] P. Chaddah, **Proc Indian Natn Sci Acad 82** (2016) 1167.
[5] R. Malhotra, **Current Science 101** (2011) 476.
[6] S.C. Lakhotia, **Proc Indian Natn Sci Acad 83** (2017) 513.
[7] B. Patwardhan et al, **Current Science 114** (2018) 1299.

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10. **Publication in journals**: **cautions while responding to reviewers**

10.1 Peer Review checks for errors, relevance and importance

When a manuscript is submitted to a journal, the journal editorial office sends it to other experts for scrutiny, and anonymity of the experts is strictly maintained. The number of experts in a particular field is usually small, and they are unwilling to publicly make negative comments on someone else within that group of experts. So, a process of 'single-blind' review has evolved in which the reviewer is aware of the authors and their institutions, but the reviewer is known only to the editorial office of the journal. In almost all cases the reviewer does this work gratis. Given the limited number of experts, they are usually overloaded! In many journals the authors have to submit a covering note justifying why their paper should be published by the journal. The editorial staff has to take a call on whether the submitted manuscript should at all be sent to an expert, or whether it can be rejected on the basis of the covering note itself. We must plan our choice of the journal where we submit carefully, and should have arguments based on the readership and authorship of the journal, the contextual relevance and novelty of our research, thought out for the covering note. Clearly we must not only try to publish our paper in a journal where it is most likely to be read by experts in the relevant area, but also submit to a journal where it is likely to be accepted for publication! This is, as we have emphasized earlier, what was done by Bednorz and Muller [1].

In recent years, non-experts like the editorial staff run commercial text-plagiarism checking software on the submitted manuscript, and use that output for rejection without sending for expert review. The argument is that though short strings of copied text do not necessarily mean that the research is of poor quality, but this is a warning sign of sloppiness. With the pressure of too many papers and too few reviewers, editorial staff can still prevent otherwise high-quality research from being examined by experts. So it would be a useful practice to run your manuscript through such commercial software that will help you get rid of any unintentional text overlap.

In the report of the House of Commons of UK [2], some concerns have also been expressed regarding possible reviewer bias. Since the reviewer and authors would belong to a small group of experts in that field, existing relationships or rivalries between reviewer and author could influence judgments. According to evidence submitted by The Royal Society in this report (Ev102), "Some critics of peer review claim that it can be used maliciously (for example, to suppress the work of rivals or to damage a competitor's career)". This is a major drawback of single-blind peer review. Some journals allow authors to provide a list of experts who should not be used as a reviewer; this can be used if you are at loggerheads with the ideas of another expert.

It is clear from what I have written that the quality of research performed is by itself not enough to ensure publication in a reputed journal. *Some effort has to be put in having a strategy to bring this research to the world's notice*! We need to discuss and learn how

to respond to reviewers of a journal. It has been stated often that we must be able to make the world understand what we claim to have found, that we must put in effort to make our talks or seminars understandable, to make figures in our slides clearly legible, and so on. Having a strategy for disseminating our research output is necessary. Somehow, we are aggressively taught that we must release our findings only after ensuring ownership if we are filing a patent, but we are not taught the same message of releasing our findings only after ensuring ownership when we are publishing a research paper!

Before concluding this discussion on strategizing for publishing our research output, we will emphasize one case where the lack of a proper strategy changed the paper from being a discovery announcement from a group that was first-past-the-post, to being a me-too paper from also-rans! ISRO's Moon Impact Probe (MIP) aboard Chandrayaan-1 had made the path-breaking discovery of detecting water on the lunar surface. As recorded by Kochhar [3], "They sent their paper to Science in December 2008, which however rejected it in March 2009. The Indian authors then sent their paper to Nature in April 2009, which also rejected it, ..". The result was announced at a press conference on 25 September 2009; some hours after it was announced that NASA's Moon Mineralogy Mapper (M3) had discovered water on the Moon. Kochhar [3] comments that the result could have been published in Current Science, and opines that such results also enhance the standing of lesser known journals. This is exactly what the Bednorz-Muller paper did to Zeitschrift fur Physik! We shall discuss such delays, related to editorial review process, below.

10.2 Bias in the review process

The imprimatur bestowed on peer review has been recently critically analyzed by Baldwin [4], and has been more directly questioned by others [2] as described in the previous section. Researchers from developing countries often worry, when submitting research papers for publication in international journals, whether reviewers will be more skeptical because of their byline. There is also a belief that peer review is not very supportive of paradigm-shifting breakthroughs, but easily accepts incremental research output even if it is mediocre.

The byline-based bias is reflected in the average time that elapses between submission of a paper and its acceptance for publication. This time delay can, as described in the water-on-the-moon case, make the difference between our report being a discovery, or being a supporting paper. As discussed in Chapter 8, preprint archives now come to the rescue by speeding dissemination and offsetting publication bias. In addition to the bias against unknown bylines, there is a bias for prevailing ideas and against out-of-the-box ideas. This comes into effect as described below.

If a paper has been submitted to a 'standard' journal, and the reviewers' reports are conditionally positive, the authors feel the pressure to publish in a journal having a respectable Impact Factor. It is believed to be easier to publish from a less-established byline if the submitted paper is generally in agreement with current thinking than if its conclusion is drastically different from current thinking [5]. The reviewers often suggest dropping a new idea, or

explaining the observations within prevailing ideas, but the 'path-breaking submission' will then be accepted for publication as a supportive paper! The premium on publication in such journals makes our young researchers to be more compliant to the comments of the reviewers. They prefer to modify the manuscript than to 'fight it out'. Thus conditionally positive reports from established journals are often complied with, and with alacrity. This includes agreeing to refer to papers suggested by the reviewers, and, more worryingly, to dilute conclusions to make them more in line with those suggested by the referees, etc. *Such bias in the review process can lower the level of our research output by dilution at the publication stage.*

10.3 Responding to suggestions that can cost time and priority

My suggestion is that recourse to a preprint repository is a must at the first sign of a delay in the publication process. A delay is a cause for worry because, as mentioned earlier, priority has not been established and unauthorized access to our manuscript may take place. There are occasions when an under-review manuscript is delayed for some reason, and a paper appears in another journal with similar results from a competing group. Actually, given the advantages listed in Chapter 8, *I would suggest a preprint repository submission must always be done concurrently with submission to a journal*.

I have already discussed in Chapter 5.3 one example that falls in this category. We had submitted a manuscript to the APS Journal Physical Review B, confident that the 'Cooling and Heating in

Unequal Fields (CHUF)' protocol that we had created and used, gives visually striking evidence of the existence of kinetic arrest of a first order magnetic transition. And the reviewer rejected our paper! We immediately uploaded it on arXiv, and made our first use of the 'Comment' entry to highlight what was new in this paper. We continued working with our new idea, published many papers in other journals [6-8], gave talks at conferences describing this idea, and had the pleasure of seeing some others use and cite our work [9]. Until three years later a well-established group from France published a paper in the same APS Journal Physical Review B presenting this protocol as if it was their own! We were very thankful that we had uploaded on arXiv and continued our research without bothering to convince the editorial office of this journal.

We have discussed the situation where reviewers/editors give a conditional acceptance subject to our modifying or diluting some observations or conclusions. One sometimes needs to publish and close a research project, so as to proceed on to newer problems. Repositories like arXiv would still host the original submitted version of the manuscript, and it can now carry the comment 'this is the original submitted version of what was modified during review process and then appeared as (journal reference)'. We can cite both (arXiv and journal) versions in our future papers if we wish to still pursue the original idea that the referee was unwilling to accept!

References:

[1] J G Bednorz and K A Muller, **Nobel lecture**, December 8, 1987.

[2] House of Commons Science & Technology Committee Report (2011) https://www.publications.parliament.uk/pa/cm201012/cmselect/cmsctech/856/856.pdf

[3] R. Kochhar, **Current Science 98** (2010) 1549.
[4] M. Baldwin, **Physics Today 70** (2017) 44-49.
[5] P Chaddah, **Current Science 106** (2014) 1337.
[6] A. Banerjee, K. Kumar and P. Chaddah, **J. Phys.: Condens. Matter 21** (2009) 026002.
[7] P. Kushwaha, A. Lakhani, R Rawat, and P Chaddah, **Phys. Rev. B 80** (2009) 174413.
[8] V.G. Sathe et al, **J Phys Cond Matter 22** (2010) 176002.
[9] S.B. Roy and M K Chattopadhyay, **Phys. Rev. B 79** (2009) 052407.

Do not get scooped; do not get plagiarized

11. Ensuring credit: suggestions for preventing idea-plagiarism

"To scientists, plagiarism of an idea strikes at the heart of research as a creative enterprise" [1]. Can one take some precautionary steps so that it will be difficult for others to usurp credit for my idea?

11.1 Put essence in title, summarize in abstract

Before writing up the manuscript for submission or uploading, the authors must be clear about what is the major new finding in your work. A few words in the title can bring out what is really new, like the landmark paper of Bednorz and Muller carried the words 'high T_C superconductivity' in the title.

Some journals ask for keywords; we usually hesitate to create new keywords. But some keywords do get created as one pursues a common thread in our research, and one such keyword from the present authors work has been listed as a keyword viz. 'kinetic arrest' even by non-overlapping authors. As a suggestion, we should not hesitate to list new keywords. The arXiv, as an exception, does not ask for keywords. It allows 'comments' where, as a mundane suggestion, the uploading site states that one can give the number of pages and figures in the manuscript. Most authors follow this mundane suggestion! The administrator, however, uses this entry to list possible ethical misdemeanors [2]. It is strongly suggested that authors use this entry to highlight the novelty of their paper.

The abstract has to be written with utmost care, it is the preview which can make the reader continue to a concentrated reading. Express the highlights of the work clearly in a few sentences in the abstract. There should not be any hiding behind ambiguous statements. One must have the courage to accept that one can be wrong [3] because, as we have stressed throughout, validity can be established only post-publication.

11.2 Create keywords, and pronounceable acronyms

If your new contribution can be summed up in a phrase, make a pronounceable acronym of that phrase. Try to use that acronym in your follow-up work, and whenever you make oral presentations. The Nobel laureate Kurt Wuthrich developed many two-dimensional NMR techniques for his award-winning studies of biological macromolecules. He gave acronyms to each of these techniques, and was highlighting this, in one of his talks that I attended, as a method to retain credit for original contributions.

Some years later when we had been pursuing our work on interrupted first order transitions, we had also created a new measurement protocol that brought out novel features of this in a visually drastic manner. After a few papers using this protocol, I remembered Wuthrich's talk and decided to identify our protocol as 'cooling and heating in unequal fields' with the acronym CHUF. We used this acronym in many subsequent papers, as did some others who gave us due credit. Some others have used this protocol without naming it, without describing it appropriately, or without giving us due credit. One of them claimed it as a specially

designed technique, used our description of the technique and even our acronym. The journal ensured an apology [4].

I thus reinforce Wuthrich's proposal of identifying every new creation in a few words, with a pronounceable acronym. The acronym should be used in talks, and in subsequent papers on follow-up research. This brings me to my next suggestion.

11.3 Do not "hit-and-run" ─ pursue your research idea

Try to pursue a line of research so that you are not restricted to one publication. Persist and show tenacity; do not hit-and-run! As you persist, you will likely develop new collaborators, or at least other researchers will start following your work.

Even if your research interest shifts, do search for publications in your earlier area. A new work may throw up an interesting point, and you may feel like revisiting the area. Such revisits are always productive because your past expertise allows you to make a contribution with much less effort.

This 'staying in touch' will also let you check whether your work is being followed by others, and if you are being cited. *If you have a genuine grievance, and if feel your idea has been plagiarized, approach the editors of the journal in which the errant paper appeared. Avoid direct contact with the errant authors.*

It is very important to have a feeling of ownership about your original contributions. Somewhat like what a mother feels about her baby!

11.4 Role of 'social internet' and post-publication comments

In this section we gaze at the crystal ball, and try to foresee how some current efforts on the internet may evolve. I start by quoting from the two page summary at the beginning of the report HC 856 [5] referred earlier: "*While pre-publication peer review (the first records of which date back to the 17th century continues to play an important role in ensuring that the scientific record is sound, the growth of post-publication peer review and commentary represents an enormous opportunity for experimentation with new media and social networking tools. Online communications allow the widespread sharing of links to articles, ensuring that interesting research is spread across the world, facilitating rapid commentary and review by the global audience. They also have a valuable role to play in alerting the community to potential deficiencies and problems with published work. We encourage the prudent use of online tools for post-publication review and commentary as a means of supplementing pre-publication review.*"

Online comments with social networking tools have a very short life. Many journals close the uploading of comments after a few weeks, or after a couple of months. Closure in a very short time can lead to a research report with a new idea being trashed or buried by those who hold conventional ideas. Whereas ideas change not because experts with old ideas change their ideas but

because young entrants to the field test the predictions of the new ideas and find the new ideas to be correct. Thus new ideas, that are not having obvious errors or inconsistencies, need to have a long enough lifetime before they are buried! Short lifetime social networking or media is thus not appropriate for evaluation of research or for evolution of human knowledge.

I will thus concentrate on more lasting 'social' internet usages (E-print archives fall in this category and have already been discussed). Another such category is that represented by Wikipedia which, however, does not upload original ideas and does not protect ownership. The site has pages (or links) on scientific topics, and covers research interests. This has probably become the widest used encyclopedia, but those who run this both welcome and control posts put up by the readership. There are various ways in which the uploaded material is controlled. Raw opinions are not allowed; relevant content should have been published in a regular publication (not a blog). The monitoring of uploaded content appears very thorough, and there are advertised attempts to raise the level. I will give two examples that I am familiar with.

First example is a page with the link http://en.wikipedia.org/wiki/Scientific_plagiarism_in_India. The content here has obvious social implications, and persistent attempts are often made to insert, delete, or modify existing text. The reasons based on which the content is monitored and controlled is obvious when one links to the revision history at http://en.wikipedia.org/w/index.php?title=Scientific_plagiarism_i

n_India&action=history. One finds that reasons are given for refusing to accept modifications in text.

The other example I will use to highlight future possibilities is http://en.wikipedia.org/w/index.php?title=Phase_transition. This page on phase transitions is purely scientific, with no obvious social implications. There is a link from this page to http://en.wikipedia.org/wiki/Talk:Phase_transition. It is stated on this link that "**Phase transition** has been listed as a level-4 ***vital article*** in Science. If you can improve it, please do. …." Wikipedia has clearly taken a judgment call that this topic is very important, and is attempting to improve the content with online participation (and monitoring). This is where post-publication comments are more important than comments in pre-publication reviews!

To conclude this brief crystal-ball gazing, there will definitely be increased use of online tools for post-publication analyses and comments. This will definitely supplement pre-publication reviews; and the pre-publication reviews being confidential and not in public domain will lead to their becoming irrelevant in the post-publication assessment. This brings us back to the discussion in Chapter 10, where we discussed the role such reviews often play in our students diluting the scientific claim that was made in the original manuscript. I reemphasize the need for having the original manuscript in public domain through an appropriate mode like an E-print archive.

11.5 Follow rules on plagiarism: neither a perpetrator nor a victim be!

This section summarizes what has been discussed in this book, highlighting the action points.

As law abiding citizens, *we always follow existing rules even if we think they are wrong and need change*. We should use software that check for text-plagiarism to ensure that our manuscript becomes "plagiarism-free". Some of the commercial software check five iterations of the manuscript on one payment; the University facility is for unlimited use and free. Make full use of it to ensure that you have not inadvertently plagiarized text.

- Get a certificate stating the date on which the final check was completed, to handle future accusations. Unfortunately, such future accusations will come from non-experts with a 'political agenda'. Such a certificate will help you assert your innocence, and keep you from getting distracted from your intellectual pursuits.
- Be generous and give credit wherever it is due. If what was said earlier was very novel then you should not try to paraphrase; rather put the text that you are reusing in quotation marks and italics.

And now, some tips to avoid being accused of self-plagiarism.

- *Always refer to your own earlier papers that are relevant***.** Or to the most recent one and add 'and references therein!
- If you are reproducing a figure from your earlier paper, give the reference in the caption. If you are making a new figure

with your earlier published data, say ‘data is taken from’ and give the reference to your earlier paper.

I have been asked by reviewers to reproduce schematic figures published earlier in the same journal to make the present paper ‘complete’ for the readers! Being asked to again explain your idea is very common; remember to refer to your earlier papers. This way you can also write regular papers using the data you presented at a conference.

After discussing action points so that you are not accused of being perpetrators of plagiarism, I now remind you that you should not become helpless victims of idea-plagiarism. You will be able to fight back if you follow the suggestions given in this book.

My parting message is: *We plan our research; we plan how and where to publish. We must also plan how to protect ownership about our original contributions irrespective of possible economic benefits or “fruits”. Like patents, research papers must be proudly owned!*

References:

[1] P. Chaddah, **Nature 511** (2014) 127.
[2] T. Sarkar, V. Pralong and B. Raveau, (2011) arXiv: 1106.6253.
[3] P. Chaddah, **Current Science 108** (2015) 313.
[4] T. Sarkar, V. Pralong and B. Raveau, **Phys. Rev. B 84** (2011) 059904.
[5] House of Commons Science & Technology Committee Report (2011) HC 856 https://www.publications.parliament.uk/pa/cm201012/cmselect/cmsctech/856/856.pdf

Appendix

Errata in Physical Review B to give us credit

(links are given to each Erratum)

1. **Phys. Rev. B 84, 059904 (2011)** by Tapati Sarkar, V. Pralong, and B. Raveau, **Laboratoire CRISMAT, UMR 6508 CNRS ENSICAEN, 6 Bd. Mar´ echal Juin, 14050 Caen, France.**
   Title Erratum: Formation of magnetic glass in calcium-doped $YBaCo_2O_{5.5}$ cobaltites [Phys. Rev. B 83, 214428 (2011)].
   Content: "The authors did not cite a relevant and important reference. The cooling and heating in unequal field (CHUF) protocol that has been used and described in Sec. III B 2 of this paper was first published in Ref.1. We apologize for this omission."
   Ref.1 A. Banerjee, Kranti Kumar, and P. Chaddah,J. Phys. Condens. Matter21, 026002 (2009).

http://journals.aps.org/prb/pdf/10.1103/PhysRevB.84.059904

2. **Phys. Rev. B 71, 229902 (2005)** by Y. Q. Zhang, Z. D. Zhang, **Shenyang National Laboratory for Materials Science, Institute of Metal Research, Chinese Academy of Sciences, 72 Wenhua Road, Shenyang 110016, People's Republic of China and International Centre for Materials Physics, Chinese Academy of Sciences, 72 Wenhua Road, Shenyang 110016, People's Republic of China** and J. Aarts, **Kamerlingh Onnes Laboratory, Leiden University, PO Box 9504, 2300 RA Leiden, The Netherlands.**

Title Erratum: First-order nature of a metamagnetic transition and mechanism of giant magnetoresistance in $Mn_2Sb_{0.95}Sn_{0.05}$ [Phys. Rev. B 70, 132407 (2004)].

Content: "The purpose of this erratum is to clarify the relationship of our paper to reference 10 [1] and to other papers by the authors of that reference. While the results of these two papers concern two different compounds, there is a similarity in the underlying physics involved as they both concern first-order phase transitions. A portion of the text of our paper, including most of the second column of page 2, and the first full paragraph of page 3, is very closely correlated with text which also appears in reference 10 [1]. While that reference was made at the end of this portion of text, there is no indication how closely correlated the text is. In addition, two other references to papers by those authors, which appear in the correlated portion of text in reference 10 [1], were omitted from our paper. These references are cited below [2,3]. We regret any confusion which has been caused by incomplete referencing of the proper work."

Ref.1 Meghmalhar Manekar, Sujeet Chaudhary, M. K. Chattopadhyay, Kanwal Jeet Singh, S. B. Roy, and P. Chaddah, J. Phys.: Condens. Matter14, 4477 (2002).

Ref.2 Meghmalhar Manekar, S. B. Roy, and P. Chaddah, J. Phys.: Condens. Matter12, L409 (2000).

Ref.3 Meghmalhar Manekar, Sujeet Chaudhary, M. K. Chattopadhyay, Kanwal Jeet Singh, S. B. Roy, and P. Chaddah, J. Phys.: Condens. Matter12, 9645 (2000).

http://journals.aps.org/prb/pdf/10.1103/PhysRevB.71.229902

3. **Phys. Rev. B 16, 2994 (1977)** by W R McIntire, **Department of Physics, University of Houston, Houston, Texas 77004.**

Title Erratum: Energy resolution and angular broadening effects in Compton-profile anisotropy measurements [Phys. Rev. B 14, 4386 (1976)].

Content: "It has been pointed out that some observations in the paper had been stated previously[1,2] and appropriate references should be added."

Ref.1 P. Chaddah and V. C. Sahni, in Nuclear Physics and Solid State Physics (India) 18c (1975).

Ref.2 P. Chaddah and V. C. Sahni, Phys. Status Solidi A 32, 677 (1975).

http://journals.aps.org/prb/pdf/10.1103/PhysRevB.16.2994.3

4. **Phys. Rev. B 65, 099902 (2002)** by A. A. Tulapurkar, **Department of Condensed Matter Physics and Materials Science, Tata Institute of Fundamental Research, Colaba, Mumbai 400005.**

Title Erratum: Critical current density from magnetization hysteresis data using the critical-state model[Phys. Rev. B 64, 014508(2001)].

Content: "The reference number 8 [**P. Chaddah, K. V. Bhagwat, and G. Ravikumar, Physica C159, 570 (1989)**] is missing at the end of the third sentence in Sec. II titled ''Calculation of the magnetization curves.'' The third sentence should read as:

> The direction and the magnitude of the shielding current, for any change in the external field, is assumed to be such so as to minimize the change of the total flux contained in the sample [8]."

http://journals.aps.org/prb/pdf/10.1103/PhysRevB.65.099902

Annexure

Related material by Praveen Chaddah (all these are free to download)

1. Not all plagiarism requires a retraction, P. Chaddah, **Nature 511** (2014) 127.

2. On the need for a National Preprint Repository, P. Chaddah, **PINSA 82** (2016) 1167.

3. Lessons on Impact Factor from the 'DBT and DST Open Access Policy', P. Chaddah, **PINSA 81** (2015) 553.

4. Evaluation of research output, P. Chaddah, **Current Science 113** (2017) 1814.

5. The importance of a preprint repository, Praveen Chaddah, **Current Science 111** (2016) 979.

6. Enhancing the efficacy of the 'DBT and DST Open Access Policy', Praveen Chaddah, **Current Science 110** (2016) 294.

7. Do we lack courage in research? P. Chaddah, **Current Science 108** (2015) 313.

8. Improving scientific research, even without changing our bureaucracy, P Chaddah, **Current Science 106** (2014) 1337.

9. Proposed functions of a university's plagiarism cell, P Chaddah, **Current Science 106** (2014) 927.

10. Pursuing knowledge creation, India needs a policy on 'plagiarism cells', P Chaddah, **Current Science 106** (2014) 349.

11. Knowledge creation from our universities, P Chaddah, **Current Science 105** (2013) 566

12. Research publications and templates for incremental research, P Chaddah, **Current Science 104** (2013) 405.

13. Ensuring credit for original thought, P Chaddah, **Current Science 103** (2012) 350.

14. Self-plagiarism and conference proceedings, P Chaddah, **Current Science 102** (2012) 379. Also, see the link to my article "E-print archives ensure credit for original ideas" provided therein.

15. Protecting our 'emerging bylines' from plagiarism, P Chaddah, **Current Science 101** (2011) 1261.

About the Author

Praveen Chaddah did his schooling in Modern School in New Delhi during 1956-1968, B Sc (Hons.) in 1971 and M Sc in Physics in 1973 from St Stephen's College at Delhi University. He then joined the Bhabha Atomic Research Centre at Bombay, and did his Ph D in 1978 from University of Bombay. He was a post-doc at the University of Illinois at Urbana-Champaign during 1978-1980, with another brief stint there in 1982. His research contributions have resulted in over 200 publications. He is a Fellow of the Indian National Science Academy, of the Indian Academy of Sciences, of the National Academy of Sciences of India. He did research at the Department of Atomic Energy for about forty years from 1973 to 2013. He was the Director of the UGC-DAE Consortium for Scientific Research during 2005-2013, where he enabled the setting up of internationally competitive experimental facilities and aggressively facilitated their utilization by researchers from universities and educational institutions.

In recent years he has also been writing on issues related to plagiarism of ideas, arguing that plagiarism of ideas with manipulation of words escapes notice of software checking text similarity. He wrote a 'World View' in Nature in 2014 titled "Not all plagiarism requires a retraction"[1]. He also writes [2] that novel ideas and novel results need to be disseminated rapidly, even without conventional pre-publication review; he argues that the best research is not necessarily that published in the best journals!

[1] P. Chaddah, *Not all plagiarism requires a retraction*, **Nature 511** (2014) 127.
[2] P. Chaddah, *Lessons on impact factor from the DBT and DST open access policy*, **Proc. Indian National Science Academy 81** (2015) 553-555.

Do not get scooped; do not get plagiarized