Research Methodology
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Lecture - 70
Ethical Conduct in Science: Ethics in Scientific Publication, Part 02

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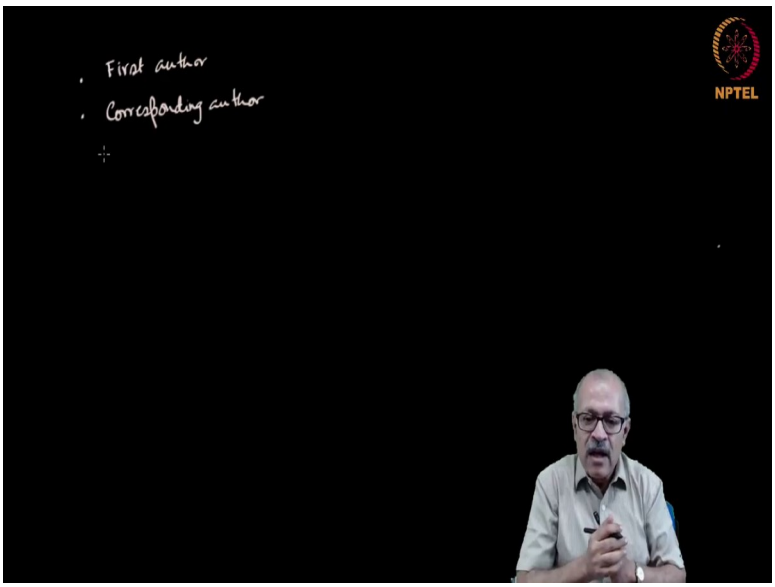

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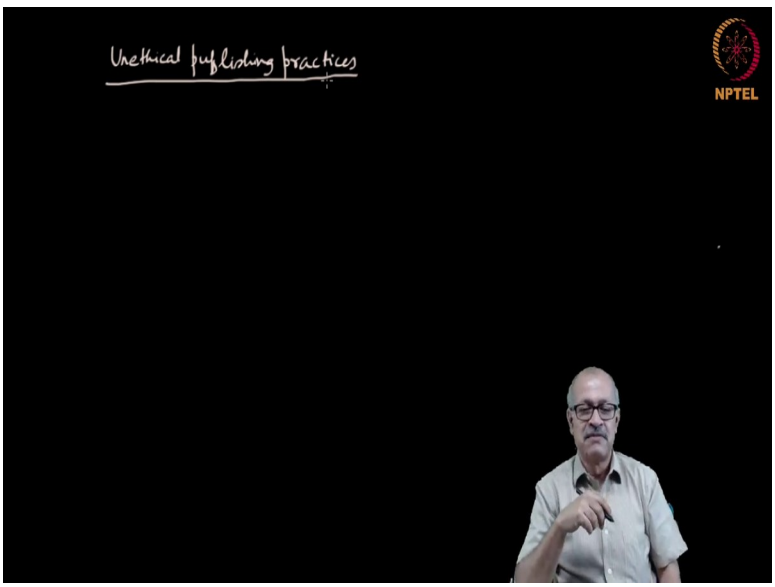

Now, I will come to some unethical publishing practices. These need to be enumerated and known by all, because these are unfortunately somewhat widespread. I will better verbally say, rather than writing that because that will take time.

Submitting the same paper in more than one journal at the same time is not allowed. You might argue that both the journals might not accept my paper, you might argue that if they do accept then I will retract from one and allow another to publish. No, that is not allowed. You cannot submit the same manuscript to two journals in order to be safe that it is published. No, you cannot do that. Only if it is rejected by a journal, then you can submit that to another journal. That is permitted. The rejection comes with some comments and it is expected that you will take care of those comments and you might submit to another journal.

Secondly, not informing a collaborator of your intent to file a patent. The collaborator has the impression that the result of the work will be published in a journal. But you had idea of doing a patent and therefore, you do not want to publish. That is unethical practice. You have to be very clear as to what is the ultimate planned outcome of the work.

Including somebody as a coauthor in return for a favour. He or she has not really contributed to the paper, but you have included his or her name in the paper in return for a favour. This is a typical scientific malpractice that is somewhat widespread and should be conscientiously avoided.

Trimming outliers from a paper in order to make the data satisfy one's expected outcome. This is scientific malpractice. You cannot do that.

Using inappropriate statistical technique. For example, in a particular situation, say, the z test is most applicable. But you find that if you do the z-test, it is going against your hypothesis. But you want to stick to your hypothesis. So, you do another test. This is a typical case of scientific malpractice. We should not do that.

Sometimes papers come to us for review and when we review that, then also there has to be ethics involved. The review has to be completely impersonal. It should not be based on our prior idea regarding that person, that author's earlier work. It should not depend on whether or not that person has cited my papers, and similar issues.

It should be completely based on the merit of the current work that the person has submitted. We have to keep in mind that people improve. Earlier they may might have written some paper which is inconsequential. But later they might improve and so every paper has to be judged on its own merit.

A few other situations are also considered to be scientific malpractice. A researcher publishes a paper resulting out of a collaborative work, without including the collaborator as an author. It is a very important scientific malpractice. This should never be done. Everybody who has contributed in the work should be included as coauthors, with their consent of course.

A student publishes a paper without informing or including the supervisor as a coauthor, without informing the supervisor. It is a scientific malpractice. Similarly, a supervisor publishes a paper which is actually result of the supervisor and the student's joint work. The supervisor publishes the work without including the student as a coauthor. It is also scientific malpractice. These things should be avoided.

When a paper comes to you for review, you have to treat that information as confidential. You should not discuss the content of that paper with other people, not even within your group.

Sometimes people refuse to review and sometimes Associate Editors find it difficult to find reviewers for certain papers. They send to prospective reviewers who have done earlier work in that field, but the person says that, no, I am not prepared to review. This happens because the act of reviewing is a voluntary service of the scientist and the whole scientific enterprise works because we render this voluntary service to the cause of science. An individual scientist gets no career benefit if he reviews a paper, and because of that, there are some people who, even though they are expert in that field, they know that field, they refuse to review, because they consider that as a waste of time.

Remember, when that person submits a paper for publication, the Associate Editor has to find at least three reviewers for him. So, the entire enterprise of science will not work unless every scientist agrees to review at least three papers for every paper that he or she intends to publish. In a year I intend to publish 3 papers and therefore, it is my responsibility to accept at least 9 papers to review. This is how the scientific community

works. Therefore, it is also a part of scientific ethics to agree to review if you are in a position to review.

When are you not in a position to review, sometimes papers come to us from fields in which we are really not expert, we really cannot give expert opinion on that, in that case it is perfectly alright to reject. In another situation, the authors of the paper are known to me. In that case I would state it as a conflict of interest and therefore, I should not review. But in all other cases we should agree to review. That is a part of scientific ethics.

When a paper is published, other people around the globe come to know about it. They normally search papers. If I am working in a particular field there are certain key words in that field and they search by the key words and they might find the paper.

As I said in the part on writing, that normally people first look at the title and then the abstract and then if that catches attention, then they download the paper and read the whole paper. For that reason, brevity matters. Short papers are read more than long papers. People have a natural propensity to read short papers. So, you should try to write as briefly as possible.

Well-written papers are read by more people, understood by more people and therefore, used by more people. If other people read your paper and understand your paper, they will also use your paper to further that work on the basis of the work that you have done. They will ask further questions. They will explore further and when they write papers, they will cite you. That is why citation is considered to be an indicator of the impact of a paper.

For that reason, citation count has become an indicator of the productivity of a person. The citation count of a journal, how many papers published in that journal has been cited by other papers -- that is an indicator of the impact factor of the journal. Often we judge the merit of a paper in terms of the impact factor of the journal in which it has been published, and the number of citations it has attracted.

So far so good. There is no difficulty with it. But since these numbers are used in various exercises, for example, institutional promotions, in awards, in academic fellowships, in this kind of prestigious positions, these counts actually have become important. And

because of that, there is now a tendency of various scientific malpractice in order to push up the numbers.

Journals engage in unscientific unethical practices in order to push up their impact factor. Individual scientists also have been found to do that. I scratch your back, you scratch my back. I cite your papers, you cite my papers -- that kind of unholy alliance are often found which try to push up the citation count artificially. These are typical cases of scientific malpractice. We should not engage in this.

These artificial numbers do not really last long. Ultimately your worth, your scientific work, your scientific contribution, will be valued by people if there is really any worth in it. If it is worthless, then however you may try to enhance the citation count by these artificial means, ultimately it will be ignored in history. So, over time only those works survive in scientists' memory that will really make an impact.

So, you should try to make that kind of impact. Therefore, I will tell you three important points to practice. One, in writing a paper you should cite all the earlier work that are really important for that particular line of work. So, it is expected that the literature survey should be exhaustive and you should not leave any important work unmentioned, because mentioning somebody's work is also a mark of ethical practice.

There are two things. One is that you have done a serious literacy survey; it is a mark of that. The other is that if you cite somebody's papers where credit is due, it actually benefits that person because he gets citation count. So, it is ethical for us to cite appropriate papers. It is necessary.

But one should not unnecessarily cite somebody's paper. One should not cite somebody's paper as a mark of gratitude for a favour received earlier.

So, unnecessary, uncalled for citations should be avoided. Papers that had no impact, that had no relevance to a particular line of work, citing these should be avoided. It is a part of ethical practice. One should not also induce others to cite your paper. They will cite if what your paper had is relevant to their line of work. If they are really using the result of your work in their work, they will automatically cite. But inducing somebody to cite my paper is unethical practice. These are typical unethical practices that should be avoided.

The way science works is that, we do a painstaking research for a long time, ultimately get some results that we reckon that it will enrich human knowledge, and so we write it up, we send it to journals, the journal editor finds reviewers, the reviewers give us comments, we again revise the paper, we submit it again, finally it might be accepted.

Then also it will be replicated by other people around the globe, the readers will replicate the work, and finally, over a period of time, maybe 5-6 years, a piece of work, its real worth becomes salient. So, acceptance by the scientific community is not the same as acceptance by a journal.

Acceptance by a scientific community comes over time. It takes time for some scientific work to be accepted by the scientific community and sometimes there is a lure of easy publicity. One tries to cut corners, one tries to avoid the long process of being recognized by the scientific community. In that case it has been found that many scientists, instead of writing and submitting and then revising, resubmitting -- all that process, and bypassing that process, they simply convene a press conference and announce the work to the press.

Now, that is unethical, because the process of submitting to a journal and the peer review is a scrutiny. A paper, a research work, has to go through the scientific scrutiny before it can be made public. Many scientists, even without going through that scientific scrutiny, try to make it public. This is scientific malpractice. Should not be done.

But in some cases, in some specific situations, announcing through media, through press conferences, may be called for. These are the situations where I have found something which has importance for people's lives. For example, in a particular area a geologist has found that the water table has gone down so badly that, in a short time, people are going to be deprived of even drinking water. Then he or she should try to sensitize the authorities by making the scientific finding public. This is permitted.

Suppose there is an earthquake warning -- some kind of a pre-indication of an impending earthquake. Then immediately the scientist should make it public, so that the authorities can take the precautionary measures. There are similar situations which call for direct public interaction without waiting for a scientific scrutiny. But these are rare cases.

In all other cases, a scientist should first go through appropriate scientific scrutiny and then only even imagine making the work public through a press conference or a newspaper article or something like that. Things should go into the press, things should go into the media, in newspaper, in TV, only when the work has been established beyond any doubt.

The public does not understand how science works, what a scientific scrutiny means -- that a work has to be replicated by scientists all over the world. When the same result is obtained, then only it is assumed to be validated. All this people do not know or understand. In that situation, if a scientist goes to the public and talks about something that is yet under scientific scrutiny, then it is not correct. You are misleading the public; you are making the public believe that something has been discovered while it is still under scrutiny. That should be avoided.

In many scientific areas there are environmental concerns, safety concerns and in those cases there has to be appropriate measures taken and one has to be careful regarding chemicals, regarding radioactive substances. The use of radioactive substances, living organisms, human subjects -- in those cases special care should be taken. In living organisms one has to take special care, so that inhuman treatment of living organism is not done. So, there are ethical issues in dealing with such subjects.

Normally in every institution there are ethics committees whose clearance has to be taken before undertaking this kind of hazardous scientific experiments, (A) involving radioactive material, (B) involving hazardous chemicals, (C) involving live organisms and things like that. These also become a part of scientific ethics.