

EDITORIAL NEEDED A RESEARCH AGENDA

Talk to any academician in Pakistan and he or she would lament about the lack of research. Not enough is being done and even less has any scientific rigor. Very rarely the question of relevance of what we are studying in our research projects is asked. Take an example. The dissertations submitted to the College of Physicians & Surgeons form one of the largest sources of original research work in our country. The significant amount of research work carried out through these dissertations is not realized very often. So far more than 4000 dissertations have been submitted to the College and with increasing number of FCPS trainees these figure may soon double. Most of these dissertations do not form a 'break through' research but these do represent small 'seeds' of scientific progress—all of which will not grow but few definitely have potential to bear the fruit. In psychiatry for example, I can identify a number of papers based on dissertations published in prestigious journals like The Lancet & The British Journal of Psychiatry.

With the mandatory research methodology workshops for trainees and approval of a synopsis by at least two members of research advisory cell of the College, the number of methodologically sound studies will also be increasing. This indeed represents a very significant amount of research for a developing country like Pakistan. However, if you see the topic of these research projects, the situation does not appear to be very encouraging. Mostly these are descriptive studies of various clinical conditions presenting during routine work in medical & surgical departments. One can't deny the significance of such studies but these rarely reflect the health problems or priorities of Pakistan.

It can be argued at this point that with limited or perhaps no resources for the research, what else can be expected. The argument does not stand a close scrutiny. You need as much resources to conduct a methodologically sound study on a topic which is relevant to our needs and settings, as is needed for a less sound and less relevant study. An organized effort is needed in determining the priorities for our researchers. We need to develop an agenda for research which is more relevant to our setting. While conducting the research methodology workshops for FCPS trainees, I am surprised to see that trainees rarely consider common problems we face in our day to day practice as researchable topics. Very rarely, for example, you find any topic related to the health beliefs and practices of our population. Similarly the common problems in our health system, e.g., the reason for lack of follow-up, poor compliance, the concepts of '*perhaiz*' and lack of record keeping etc. are rarely considered worth a research effort. These issues must not be underestimated. Recently in a randomized trial in over 2000 children in Pakistan suffering from pneumonia, major reason for treatment failure was cited as poor compliance¹. This was more surprising in view of the fact that the comparison was between 3 days and 5 days of Ampicillin treatment which is very brief treatment duration. I have taken the research for FCPS dissertation just an example. Unfortunately, the situation is not different even at a much senior level. A cursory look at the index of any medical journal published from Pakistan reveals similar trends.

One must ponder at this point and ask is there a national agenda for research? The answer is yes, but no one seems to know about it. A number of workshops conferences and reports of both the Pakistan Medical Research Council and professional societies of various disciplines in medicine and surgery have produced lists of topics & health issues pertinent to

Pakistan, which need to be researched. In psychiatry for example, there is a long list of topics identified by WHO for research in Pakistan.²

This poses a serious question for the health and researchers in Pakistan; why such a research agenda is not followed? We can't afford to waste our meager and limited resources on studying the topics which are not relevant to our setting. The detailed answer to this question is multifaceted and lies beyond the scope of this article. The most important reason, however is that these priority lists are produced at national level and not at a regional level, far from the actual realities of life. When there is little funding for research and most projects are carried out by individuals solely dependent upon their own resources the priorities for research need to be determined at a local or regional level. The groups of dedicated research workers at the level of say for example, in a medical college, in a postgraduate medical institute or in a particular city need to evolve a list of topics and health issues for research. The list should reflect the common health problems of our populations and the topics which can be researched with in our limited resources. Developing a research agenda at a local level in collaboration with each other will also help to overcome another problem in our research efforts, i.e., lack of collaborative research. Medical research is mostly a team effort. High quality research can be possible even with limited resources when teams of different disciplines collaborate with each other. Cirrhosis, for example can easily be studied by a team of gastroenterologists pathologists and microbiologist and public health physicians. This is a norm in most of the scientific community but unfortunately an exception in our country.

The research efforts guided by a well planned research agenda developed preferably at local levels can help to direct our efforts in a much better way. A meaningful and relevant research will help to guide our policies, thereby using the limited resources with greater efficiency. More importantly, perhaps, it will also result in more interesting studies which should help to stimulate the interest in research—an essential ingredient for the success of any research effort.

REFERENCES

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