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Reflection and Remarks on the Conversation "The Past, Present and Future of the Nature of Science Research" with Norman Lederman

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The conversation between myself and Professor Lederman took place in Malmö, Sweden on August 25, 2007. The purpose of the conversation was to elaborate on the past, present and future of nature of science research and also to provide insights for beginning researchers. In this text I am providing the questions about which we talked and my reflections based on my own experience together with some elaborating remarks from the science history.

Keywords: Nature of Science, Science History, Testing Assumptions

INTRODUCTION

Dr. Norman G. Lederman is currently Chair and Professor of Mathematics and Science Education at the Illinois Institute of Technology. He has taught a full range of graduate (Masters and Doctoral) courses in secondary science education and supervised teaching interns. Dr. Lederman received his Ph.D. in Science Education from Syracuse University (1983); M.S. in Secondary Education from Bradley University (1977); M.S. in Biology from New York University (1973); B.S. in Biology from Bradley University (1971).

Before arriving at his present position, he was Professor of Science and Mathematics Education at Oregon State University since1985, Assistant Professor of Teacher Education, SUNY/Albany (1984-85) and Assistant Professor of Science Teaching, Syracuse University (1983-84). Dr. Lederman taught high school biology as well as college level biology for many years. Throughout the years he has received several awards and recognitions for his teaching and research.

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Dr. Lederman is internationally known for his research and scholarship on the development of students' and teachers' conceptions of nature of science and scientific inquiry. He has also studied preservice and inservice teachers' knowledge structures of subject matter and pedagogy, pedagogical content knowledge, and teachers' concerns and beliefs. Dr. Lederman has been author or editor of 10 books, including an elementary science teaching methods textbook. Recently he co-edited "The Handbook for Research in Science Education." He has written 15 book chapters and published over 170 articles in professional refereed journals. In addition, Dr. Lederman has made over 500 presentations at professional conferences and meetings around the world. In the 2007 ESERA Conference he delivered a keynote speech entitled "Science Education as a Discipline: Does Our Research Meet Our Aspirations?"

Dr. Lederman has served in different capacities in professional organizations as member of board of directors, division director, regional director, etc. Most notably, he has also served as President of the National Association for Research in Science Teaching (NARST), the Association for the Education of Teachers in Science (AETS), and the Oregon Educational Research Association. Dr. Lederman is presently the Editor of the journal *School Science and Mathematics* and serves, or has served on the Editorial Boards of the Editorial Boards of many international academic journals in our field including Science Education; Journal of Research in Science Teaching; International Journal of Science Education; Journal of Science Teacher Education; Science and Education; and Eurasia Journal of Mathematics, Science and Technology Education.

Below you will find some snapshots of our conversation and my reflection and remarks on selected issues that we discussed. During our conversation I directed several questions to Professor Lederman. For the record they were the followings:

- What marked the beginning of interest and research in nature of science as far as science educators are concerned?
- What elements of nature of science did the early researchers consider?
- How was the gender issue in the early times? For teaching kids for becoming a scientist, do you think that both genders were encouraged equally?
- How did the NOS research evolve during the past several decades and what role do you think you played in it?
- Are you happy with the current status of nature of science research? That is, did we come a long way and what was it like?
- Today, do you think the NOS research draws attention that is deserves?
- How do you see the future of the nature of science research?
- How do you see the international contribution from other geographical regions and cultures to NOS research?
- What suggestions do you have especially for young researchers? And for starters of nature of science research, what would you recommend?

Reflection

One of my questions was about the gender equity problem in science education and for preparing future scientists. Professor Lederman gave examples of unfair treatment that he came to know. Surely there exist positive examples as well. We hope that the situation is getting better around the world with the growing awareness and concerns. As to the situation in Turkey, both in the past and today, I cannot say that it is perfect. However, it can be stated here that today according to a report[1] on women's employment in Turkey the percentage of women employed at Turkish universities is 36% and they comprise one fourth of all professors. Also, women have a 31% representation among architects and 29% among medical doctors and surgeons in Turkey. Moreover, in 2004 50% of doctorates were awarded to women in Turkey (OECD, 2007, p.43). These figures, although by no means satisfactory, might show a better status for woman as compared to many countries, including the developed ones. However, it should be noted that problems and cases of the type mentioned in the conversation are also visible in Turkey.

The types of contributions to a body of literature may come through different paths. A path could be via expanding the scope of the literature (theory) either by broadening or deepening it. Adding new and novel instances of an observed (known) phenomenon and explaining how they fit in a theory can be beneficial[2]. On the other hand, one can try innovative and original methods to the cases even if they were researched by other methods before. Conversely, existing methodologies can be applied on different or unusual samples that have never been researched until now. In this sense Professor Lederman and his group contributed to the nature of science research by creating pioneering assessment techniques (see Lederman, Wade and Bell (2000) for a review in this area). To this end, I can also add my own contribution (Tasar, 2006a; 2006b) to the nature of science literature: assessing prospective teachers' understanding of tentativeness in science by using scientific vignettes[3]. In this work I utilized a piece published in the National Geographic magazine (Newman, 2005) in which a present-day story was told about how and why two scientists were disagreeing on a subject (i.e. the reason for toxicity of the fugu fish). The results showed that this could be a fruitful method of probing participants' understandings and their ways of reasoning about nature of scientific knowledge. Now this line of research can be further expanded in two different ways: first, by using the same vignette to probe understandings of different samples (e.g. younger pupils, in-service teachers, etc.); second, creating new vignettes that can be utilized in probing different aspects of nature of science and scientific knowledge.

Professor Lederman very rightfully indicated in our conversation that developing an expertise in a field requires a lot of hard work within a narrow area and one cannot become an expert on everything. He also stressed the need for a focus in researchers' scope. Science education is a broad area within itself too. Even when we think of the nature of science literature we can find several threads in there. For example, I tried to locate my dissertation study within the science

[1] http://www.kssgm.gov.tr/istihdam.html

[2] See for example Little (1964). The first three sentences of the abstract are as follows: "London's idea that superconductivity might occur in organic macromolocules is examined in the light of the BCS theory of superconductivity. It is shown that the criterion for the occurence of such a state can be met in certain organic polymers. A particular example is considered in detail."

[3] It should be noted here that this type of vignettes are different than the interactive historical vignettes which were created and used by Wandersee and Roach (1998).

education literature by giving a brief summary of the science education research in the first chapter. I also tried to show how my focus in the dissertation was related to the bigger ideas (Tasar, 2001, p. 6). Although, other classifications may exist (e.g. Duit, 2007) I divided the major research areas in science education into six categories: the nature of content area and subject matter, the nature of science and scientific knowledge, the nature of teachers and learners, the nature of teacher training, the nature of teaching and learning, the nature of teaching and learning environments (Tasar, 2001, p.3).

When I began my doctoral studies I was a complete stranger to the field. At that point in time the only relevant journal I knew was, as a physicist by training, the American Journal of Physics which in addition to physics papers also publishes physics teachers' and professors' own work about teaching and learning physics. Hence the very first paper I read was teaching mechanics at high school level (i.e. Wells, Hestenes and Swackhamer, 1995). As a rookie, I was real worried about being in that situation and did not even know where to begin. I only hoped that things would unfold by themselves. But I did one thing right: I read a lot about my own field, which is physics education. Soon I discovered all the major journals in the field and very often when I went to the library inspected each of the past volumes and issues. Another thing I did right was when I was reading a paper, I carefully examined the given references and tried to obtain and collect the ones that I saw referenced often in the works that I read or thought were important. In this way I tried to close the gap in my readings since I wanted to develop a good knowledge of the existing literature base. I picked a few major topics as my areas of interest during my doctoral studies: physics education (mainly teaching and learning of topics in mechanics), philosophy of science (the history and nature of science), and cognitive basis of learning. Later, these became my supporting areas during my doctoral studies and I grouped the courses I took under these topics.

Whenever I asked my doctoral adviser Professor Vincent Lunetta about what I would do for my dissertation he always non-hesitantly replied: "something you can do and something worth to do." This was very frustrating indeed. It happens at times that a doctoral student suddenly becomes a part of a research project which I think the case for many engineering or science studies. When a graduate student enters a lab with the director of the lab being her/his advisor, then most things concerning the candidate's future research are readily set. With an ongoing research project the new recruit is somehow fit into a part of it. What is expected of her/him is not to do a separate work, but rather to complete a definite part of the puzzle in hand. However, if the grad student has to identify a problem and a set of research questions in an area, things don't come very easily. But one should always keep in mind that 'good things come to those who wait for them with patience and perseverance; but the rushing and impatient ones will go from trouble to trouble.' Likewise, expertise does not develop overnight by itself and one cannot figure out immediately a viable and worthy set of research questions to pursue.

Remarks

Professor Lederman also draws attention to "testing assumptions" that still exist in the literature as possible areas for extension and improvement. One of such assumptions he mentioned from education was the taken for granted linear relationship between teachers' and students' knowledge. An example of this sort of scientific activity can also be seen in James D. Watson's Double Helix:

> "Conversations with Cavalli, nonetheless, hinted that Joshua was not yet prepared to think simply. He liked the classical genetic assumption that male and female cells contributed equal amounts of genetic material, even though the resulting analysis was perversely complex. In contrast, Bill's reasoning started from the seemingly arbitrary hypothesis that only a fraction of the male chromosomal material enters the female cell. Given this assumption, further reasoning was infinitely simpler.
>
> As soon as I returned to Cambridge, I beelined out to the library containing the journals to which Joshua had sent his recent work. To my delight I made sense of almost all the previously bewildering genetic crosses. A few matings still were explicable, but, even so, the vast masses of data now falling into place made me certain that we were on the right track. Particularly pleasing was the possibility that Joshua might be so stuck on his classical way of thinking that I would accomplish the unbelievable feat of beating him to the correct interpretation of his own experiments." (p.92)

A success story that involves testing a prevailing assumption is that of Alex Müller and Georg Bednorz, researchers at the IBM Research Laboratory in Rüschlikon, Switzerland then in 1986. They made a truly breakthrough discovery in the field of superconductivity by creating a brittle ceramic compound (containing lanthanum, barium, copper and oxygen) that showed an unexpectedly high transition temperature into the superconducting phase (until then the highest transition temperature was observed in Nb_3Ge at 23.3 K and the new ceramic compound exhibited a transition temperature at around 30 K which was an astonishingly high temperature to show a superconducting property for any known substance at the time). The reason for Bednorz and Müller's discovery for being so remarkable is the fact that ceramics are normally insulators. At normal temperatures materials of this type do not conduct electricity well at all. So, researchers until that time had not considered this class of materials as possible candidates for superconductivity and as a consequence did not study such materials. However, by synthesizing and testing the electrical properties of these materials known as cuprates

In their lecture delivered on the occasion of the presentation of the 1987 Nobel Prize in Physics Bednorz and Müller (1988) explain in detail the path they followed which eventually led to the prize. In that lecture they also openly state that they tested the idea of producing superconductivity in non-metallic substances (i.e. oxides) with the following words:

> "And indeed, for somebody not directly involved in pushing T_c's to the limit and having a background in the physics of oxides, casual observation of the development of the increase of superconducting transition temperatures, shown in Fig. 1, would naturally lead to the conviction that intermetallic compounds should not be pursued any further. This because since 1973 the highest T_c of 23.3 K (Muller, 1980; Beasley and Geballe, 1984) could not be raised. But nevertheless, the fact that superconductivity had been observed in several complex oxides evoked our special interest. (…)
>
> Since the publication on the existence of this new class of materials, the interest and work have far exceeded the expectations of the laureates, whose aim was primarily to show that oxides could "do better" in superconductivity than metals and alloys. Due to this frenzy, progress on the experimental side has been rapid and is expected to continue."

Vitaly Ginzburg is one of the physicists who worked in the field of superconductivity for so long and contributed to our understanding of the phenomenon so immensely. He also draws attention (Ginzburg, 2004) to testing assumptions in his 2003 Nobel lecture in physics. Let's read:

> "The following fact serves to illustrate the accidental, to a certain extent, character of the discovery of high-temperature superconductivity. As far back as 1979, in one of the institutes in Moscow they produced and investigated (Shaplygin *et al.*, 1979) a $La_{1.8}Sr_{0.2}CuO_4$ ceramic, which was close to that investigated by Bednorz and Muller, with $T_c \approx 36$ K (Cava *et al.*, 1987). However, Shaplygin *et al.* (1979) measured the resistance of their samples at temperatures not lower than the liquid-nitrogen temperature and therefore did not discover their superconductivity. From the above one may draw a trivial conclusion that all newly produced materials should be tested for superconductivity. Also evident is another conclusion, namely, that even today it is possible to make a major discovery and next year be awarded a Nobel Prize for it without gigantic facilities and the work of a large group. This should be a source of inspiration, particularly for young people."

It is indeed worth testing the assumptions we have and, in my opinion, the beginning researchers are and should be more inclined to do so since the old guns usually have a predetermined mind set or prefer to give way to testing assumptions to beginners perhaps since they think no desired result is likely to be produced in such an endeavor.

Professor Lederman has contributed to the field enormously in different capacities during his carrier as a professor and researcher and brought his insights to the conversation as well. Overall, I believe that our conversation has important clues for researcher especially for the beginning.

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