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SMALL SCIENCE AND UNEXPECTED DISCOVERIES IN SEISMOLOGY

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Since seismology deals with natural phenomena which are often uncontrollable, it is not always possible to perform controlled experiments. In many other fields, controlled experiments are an essential part of research. In some branches of seismology, controlled experiments are possible, but very often we face unexpected situations. Because of this unique nature of the field, researches in seismology are usually performed in two ways.

In the first case, we know approximately what we want to discover to begin with; then we design experiments, write a proposal for funding, and, if funded, conduct the planned research projects. Very often this type of science requires multi-personnel or multi-organizational efforts, and is generally termed "big science."

In the second case, science is done more or less accidentally. While making routine observations, or doing some research, we often come across something unusual or unexpected. Most often they turned out to be a relatively trivial thing. However, history shows that these unexpected findings occasionally led to important discoveries in seismology. In order to make the initial unexpected finding a major discovery, a substantial amount of effort and time, in addition to the investigator's imagination, are required. However, the research is done essentially on an individual basis without a large organization involved and may be called "small science."

Unfortunately, the latter type of activity does not always receive enough support because we cannot write a well thought-out proposal for something completely unexpected. In view of the proven merits of this type of research, I take this opportunity to reiterate its importance, illustrating some historical discoveries made in this fashion.

One of the most celebrated examples is the discovery of teleseismic signals by Paschwitz (1889). While making tilt measurements in Germany, using Zollner horizontal pendulums, to study earthtide, Paschwitz observed a peculiar signal. He later found that the time of this event coincided approximately with that of an earthquake in Tokyo on 18 April 1889. He concluded that these disturbances recorded in Germany were caused by this earthquake in Tokyo, thereby demonstrating that seismic waves can travel through the earth's interior over a large distance. Since Paschwitz did not use the correct standard time for Tokyo (see Knott, 1889), he obtained a rather low average velocity of propagation, about 2 km/sec. Regardless of this error, this discovery had an important impact on the later development of seismology. It encouraged seismologists to use seismic waves to explore the earth's interior. (In the resolution which was drafted by Paschwitz and submitted to the Sixth International Geographical Convention held in London in 1895, it is stated that "... it is certain that the elastic movement emanating from the earthquake focus propagates through the earth's body..., seismological observations provide a means to indirectly obtain information on the condition of the earth's interior....") Since Paschwitz's primary objective was to study earthtide, this was truly an unexpected discovery.

The famous discovery of the Moho discontinuity by Mohorovičić (1910) also seems to be somewhat accidental. He found a distinct discountinuity in the slope of travel-time curves obtained from a Balkan earthquake on 8 October 1909 and other events. In this case, he may have had a clear objective to discover velocity discontinuities when he examined the seismograms. If so, the discovery may not be completely accidental, but the main earthquake which provided him with a key data set for his discovery was not a planned event. More importantly, having been motivated by this finding, Mohorovičić made an extensive study on reflection and refraction of seismic waves at a discontinuity to strengthen his conclusion. In fact, his study of this problem seems to have as strong an impact on seismology as his discovery of the discontinuity itself.

Around 1910, several important studies were made which led to the discovery of the earth's core. The papers by Oldham (1906) and Gutenberg (1913, 1914) are most frequently quoted in the seismological literature. These studies are based upon the travel-time data obtained by earlier studies, e.g., those by Milne and Wiechert and his colleagues. As clearly stated in Wiechert and Geiger (1910), the main purpose of constructing travel-time curves was to determine the structure of the earth's interior. In that sense, the discovery of the core was by no means accidental. However, it must have been difficult for these authors to predict in the beginning exactly what was to come out from the travel-time data they were diligently collecting from one earthquake to another.

The discovery of the inner core by Lehmann (1936) appears somewhat more accidental. Lehmann (1930) drew attention to seismic phases which appeared on the seismograms of the 16 June 1929, Buller (New Zealand) earthquake ($M = 7.6$) recorded at distances of 110° to 140°. On the basis of these phases and other phases observed at distances of about 150°, Lehmann (1936) suggested the existence of an inner core. Of course, Lehmann had long experience in looking at core phases and must have been examining them with the hope of finding something new. Yet, the occurrence of a relatively large earthquake in New Zealand at distances of 110° to 150° from a group of high-quality stations where vertical component seismographs had just become available was something of a coincidence. These core phases are large on vertical components so that the existence of vertical component seismographs was quite essential to Lehmann's discovery.

These are just a few examples. There are many discoveries of this sort; of course no discovery is completely accidental. There is no question that these discoveries were possible only through the diligent observations and creative minds of the great seismologists. However, even for these great seismologists, it would have been very difficult to work through the details without adequate support.

I have some concern that the recent trend in funding is not quite adequate to support this type of research. There are efforts to promote big projects. I see nothing wrong in promoting well, thought-out big projects. They will lead to great discoveries and to promotion of seismology and geophysics in general. My concern is that the importance of "small science" which may lead to unexpected important discoveries often tends to be obscured in the shadow of "big science." One of the practical difficulties is to write a strong and persuasive proposal on something unexpected. Also, the present funding situation is such that everyone is so busy writing proposals and reports that even if something that looks unusual is found, it is difficult to pursue it, unless it is directly related to the project being proposed. Once it is put aside, it tends to be forgotten forever. In order to promote this type of "small science," the idea behind the NSF's Presidential Young Investigators Award (PYIA)

is excellent. The only problem is that, it is awarded to only a very limited number of young scientists. There are also "old" investigators who, however, do not qualify for it.

In conclusion, I propose, on the basis of the historical evidence presented here, that every effort be made to support "small science" which will promote creative and innovative sciences leading to important discoveries. Universities and discretionary funds, largely from private foundations and industry, can play a role here as important as federal funding sources.

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