

Nachlese zur Verleihung des Rebeur-Paschwitz-Preises an Dr. Walter Zürn

Es ist schon erstaunlich: Im 19. Jahrhundert wurde Erdbebenforschung überwiegend in Japan und Italien betrieben, während das erdbebenarme Deutschland dafür wenig Interesse zeigte. Weit stärker wurde dagegen die Forschung in der Astronomie und Geodäsie vorangetrieben, wobei diese Forschungsgebiete von der in Deutschland weit entwickelten Feinmechanik profitierten. Mit hochpräzisen Beobachtungsinstrumenten begann v. Rebeur-Paschwitz seine Untersuchungen zum direkten Nachweis der Deformation des Erdkörpers durch Gezeitenkräfte. Verblüffenderweise gaben seine Experimente zunächst auf einem ganz anderen naturwissenschaftlichen Gebiet den entscheidenden Impuls, nämlich der globalen Seismologie. Für viele von uns mag dieses Beispiel als Paradigma gelten, dass sich wissenschaftliche Erkenntnis im Wesentlichen durch schwer oder nicht vorhersagbare Entdeckungen vermehrt. Heute geht der Trend in der Finanzierung von Wissenschaft klar zugunsten von Großprojekten mit gut ausgestatteten Budgets und klar umrissenen Zielen, während die sogenannte „small science“ mit weit weniger finanzieller Unterstützung rechnen kann. Wäre Rebeur-Paschwitz die Entdeckung von Fernbeben gelungen, wenn sein Experiment im Rahmen eines Großforschungsprojektes stattgefunden hätte? Welchen Wert eine gute Idee für die Wissenschaft hat, ob durch „big-“ oder „small-“ Science gewonnen, lässt sich immer nur in der Retrospektive erkennen. Walter Zürn hat in seiner Dankesrede zur Verleihung des Rebeur-Paschwitz-Preises auf den Wert von sogenannten „unexpected discoveries“ hingewiesen und dabei den Artikel von Hiroo Kanamori zitiert. Auf vielfachen Wunsch des Auditoriums wird dieser Artikel im Folgenden abgedruckt.

Hinzufügen möchte ich noch, dass der Trend der Forschungsförderung zur Unterstützung von „angewandter“ Forschung diese Tendenzen noch verstärkt (vergl. 6. Rahmenprogramm der EU): Durch die Beschränkung auf Forschungen, die möglichst schnell ökonomischen Nutzen erbringen, werden die Chancen auf überraschende Entdeckungen (*unexpected findings*, s. unten) deutlich verringert. Um bei der Gezeiten-Forschung zu bleiben: Gerade die Geräte, die zur Erforschung der globalen Elastizität der Erde über die Gezeiten-Deformation entwickelt und ständig verfeinert worden sind, ermöglichen heute – neben den seismischen Netzen - die Überwachung von Erdbeben-Gebieten oder dienen als Referenz für Satelliten-gestützte Verfahren.

Es ist schade, dass die heutige Politik der Forschungsförderung eher kreative Ansätze einschränkt, als sie zu unterstützen. Insofern betont Kanamori mit seiner Befürwortung des Nachwuchspreises der National Science Foundation (*NSF's Presidential Young Investigators Award*) den richtigen Weg. Allerdings darf sich diese Art Förderung nicht auf eine Medaille beschränken, sondern bedarf auch einer entsprechenden finanziellen Ausstattung.

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Nachdruck mit freundlicher Erlaubnis des Autors und der Seismological Society of America

Small Science and Unexpected Discoveries in Seismology

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Bulletin of the Seismological Society of America, Vol. 76, Nr. 5, pp. 1501 - 1503, October 1986

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 with an earthquake in Tokyo on 18th 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 Geographic 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 Mohorovicic (1910) also seems to be somewhat accidental. He found a distinct discontinuity in the slope of travel-time curves obtained from a Balkan earthquake on 8th 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, Mohorovicic 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 16th June 1929, Buller (New Zealand) earthquake (M=7.6) recorded at distances of 110 to 140 degrees. On the basis of these

phases and other phases observed at distances of about 150 degrees, 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 degrees 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 the 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 science 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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