ΣΕΙΣΜΟΛΟΓΙΑ. — **Basic principles for evaluating an earthquake prediction method,** by *P. Varotsos, K. Alexopoulos, K. Eftaxias and M. Lazaridou**, διὰ τοῦ Ἀκαδημαϊκοῦ κ. Καίσαρος Ἀλεξοπούλου.

ABSTRACT

A 3 year continuous sample of official earthquake predictions based on the observation of Seismic Electric Signals in Greece was recently published. During the last year four independent groups analysed this sample and concluded that the success rate of the predictions is far beyond chance. On the other hand a fifth group claims that these predictions can be ascribed to chance. In the present paper we examine the origin of this disagreement from general principles. By assuming that an ideally perfect earthquake prediction method exists, we show that exactly for **the same set of ideally perfect predictions one can extract contradictory results**, i.e. that they either can be ascribed to chance or far beyond chance, by just selecting **different** magnitude thresholds for the earthquakes and predictions. Furthermore we indicate that one can be led to several erroneous conclusions, e.g. that **true** precursory phenomena are «post-seismic effects», when making an inappropriate use of Poisson distribution in conjuction to the non selection of a common magnitude range for the earthquakes and predictions.

In evaluating the probability of a prediction being successful by chance one has to consider the product of three probabilities (in respect to time, space and magnitude). The primary importance of this essential point is illustrated by a series of characteristic examples.

The remarks emphasized in this paper are of general use when examining the correlation beyond chance between earthquakes and various geophysical phenomena.

INTRODUCTION

During the last years official predictions are issued by the VAN-group in Greece for earthquakes (EQ) with (surface wave) magnitude (M_S) larger (or equal to) 5-units that are based on the observation of electrical precursors, i.e. the so called Seismic Electric Signals, SES (Varotsos and Alexopoulos 1984, 1986). They forecast the epicentral location and the magnitude of the impending EQ within a certain time-window Δt; for the two more destructive events public warning was also made (Varotsos and Lazaridou 1991; Varotsos, Alexopoulos and Lazaridou 1993). A three year continuous sample of these predictions has been recently published by Varotsos and Lazaridou (1991) that was evaluated by five independent groups which followed different statistical methods. The conclusions drawn by these groups are as follows:

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(i) Hamada (1993). «... for EQs with Mb (USGS) $\geqslant$ 5.0 the ratio of the predicted to the total number of EQs is 6/12 (50%) and the success rate of the prediction is also 6/12 (50%) with a probability gain of a factor of 4. With a *confidence level of 99.8%, it is rejected that this success rate can be explained by a random model of EQ-occurrence* taking into account a regional factor which includes high seismicity in the prediction area...».

(ii) Shnirman, Shreider and Dmitrieva (1993). «... the earthquakes and the VAN prediction telegrams are in obvious correlation if we select both for strongest magnitudes...».

(iii) Nishizawa, Lei and Nagao (1993). «... Results show that the model assuming seismic electric signals as precursors of EQs gives the best fit to the data...».

(iv) Uyeda (1991). «... the actual success rate and alarm rate... are both estimated to be about 60%...».

(v) Mulargia and Gasperini (1992) (MG). «... the claimed success can be very confidently ascribed to chance; ... VAN predictions show a better association with the events which occurred before them...».

It is obvious that the MG disagree with the results of the other four groups the conclusions of which practically **coincide**. In view of the MG-claim, a recent comprehensive review on low frequency electrical precursors (Park, Johnston, Madden, Morgan and Morrison, 1993) noticed that no consensus has yet been reached among researchers concerning the SES-success rate beyond chance. It is therefore worthwhile to examine here some basic principles that have to be followed when studying the correlation beyond chance between geophysical phenomena and earthquakes. The paragraphs of the present short comment do not deal at all with the details of the application of the procedure followed by MG to the SES predictions but examine the validity of the MG-analysis from a general standpoint. More specifically in the first paragraph the non-selection of a *common* magnitude range for earthquakes and predictions is applied to an **ideally perfect earthquake prediction method**, **IPEPM**, (i.e. to a method which, of course, does not still exist but by definition achieves the prediction of **all** earthquakes —above a certain magnitude— with a reasonable accuracy as far as the time, epicentral coordinates and the magnitude are concerned and furthermore it does not issue any false alarm) which leads to the «conclusion» that this IDEAL method can be rejected. In the second paragraph, after recalling the basic principles of the Poisson distribution, we indicate that this distribution should not be used for the evaluation of an IPEPM when mainshocks and aftershocks are involved in the calculation. In the third paragraph we indicate that the calculation of the probability of a prediction being successful by chance is a product of three probabilities P_T, P_e and P_M in respect to time, epicenter and magnitude respectively. The major importance of P_e is emphasized with specific examples. In the last paragraph we indicate that Poisson distribution in the way used by MG cannot be applied to earthquake prediction data when various time-windows have to be considered.

1. Consequences of the non-selection of a common magnitude range for earthquake predictions when applied to an IPEPM. We shall «evaluate» the results of an IPEPM by employing *different* magnitude thresholds for the EQs and for the predictions respectively. We shall see that the *result drastically depends on the thresholds chosen.*

Assume a total observation period (T) of approximately 3 years, e.g. 1100 days, during which two strong EQs ($M_S = 7.0$) occurred at two remote seismic regions A and B. Except of these two EQs a number (e.g. 12) of smaller shocks (NOT aftershocks) with M_S ranging from 5.1 to 5.5 occurred at different (remote) seismic regions. For example four of them had $M_S = 5.5$ and 8 smaller with M_S between 5.1 and 5.4 (Fig. 1). Assume now that for ALL these 14 EQs the IPEMP issued predictions with an excellent accuracy in the epicentral location (i.e. $\Delta r = 0$) and with a small time window, Δt ($\ll$ T), e.g. between a couple of hours and 22 days i.e. $\Delta t \leqslant 22$ days).

As for the magnitude determination we assume that for all 14 predictions the deviation ΔM was less than (or equal to) 0.7-units as follows: (i) for the two strong EQ_S the predicted M_S-value was 6.3 and (ii) for the 12 smaller EQs the predicted M_S-values were $M_S \geqslant 5.1$ (i.e. assume that it *has been agreed* that IPEPM should issue prediction only when it estimates that the expected magnitude is, at least 5.1 or larger) but for all cases $\Delta M \leqslant 0.7$.

Let us apply now the *same procedure and thresholds as in Table 2 of the MG-publication* in order to evaluate the above results. They selected a magnitude threshold of $M_S \geqslant 5.8$ for the EQs and a **different** threshold of $M_S \geqslant 5.1$ for the predictions (pred.). With these thresholds one has 2 EQs and 14 predictions so that the two «mean» probabilities P_{EQs} and P_{pred} are:

P_{EQs} = 2 EQs / 1100 days and P_{pred} = 14 pred / 1100 days
Therefore the quantity ($\Delta t\ P_{EQs}\ P_{pred}\ T$) calculated by MG is:

$$(\Delta t\ P_{EQs}\ P_{pred}\ T) = 22 \text{ days} \ . \ (2\text{EQs}/1100 \text{ days}) \ . \ (14 \text{ pred}/1100 \text{ days}) \ . \ 1100 \text{ days} = 0.56$$

The significance level (s.l.) is calculated from the (upper part of the) cummulative Poisson expression:

$$(\text{s.l.}) = \sum_{n=x}^{\infty} \frac{(\Delta t\ P_{EQs}\ P_{pred}\ T)^{x}}{x\,!}\, e^{-(\Delta t\ P_{EQs}\ P_{pred}\ T)} \qquad (1)$$

where n denotes the number of the successfully predicted EQs. By inserting into Eq(1) the above value of ($\Delta t\ P_{EQs}\ P_{pred}\ T$) = 0.56, one finds for n = 2, a result of around 0.1. (see the Tables of Abramowitz and Stegun 1970). As this s.l. value is larger than 0.05, the MG-procedure «concludes» that *the excellent predictions of IPEPM depicted in Fig. 1 could be achieved by chance. Attention is drawn to the following paradox: When in Fig. 1 we consider a larger number (e.g. 20) of earthquakes with* $M_S \geqslant 5.1$ *— two of which have* $M_S \geqslant 5.8$ *— and hence a larger number (i.e. 20) excellent predictions (with* $M_S \geqslant 5.1$*) one finds s.l. = 0.2 (with thresholds 5.8 and 5.1 for EQs and predictions respectively) which «means» that when an IPEPM achieves more successes (for independent earthquakes) they can be ascribed to chance to a higher degree.* (The origin of this unacceptable result will be discussed below).

Let us repeat the MG-calculation of s.l. by taking exactly the **same data**, i.e. those of Fig. 1, but now changing the magnitude threshold of the EQs from 5.8 to 5.5. In this case Fig. 1 shows that the number of EQs with $M_S \geqslant 5.5$ is 6 and hence the exponent becomes (we recall that the number of predictions is still 14 because we assumed that we have issued **totally** 14 predictions and hence no predictions with $M_s < 5.1$ exist):

$$(\Delta t\ P_{EQS}\ P_{pred}\ T) = 22 \text{ days} \ . \ (6\text{EQs}/1100 \text{ days}) \ . \ (14 \text{ pred}/1100 \text{ days}) \ . \ 1100 \text{ days} = 1.68$$

and then the (s.l.) is calculated to be (for n = 6): (s.l.) = 0.036 which means that the predictions of the EQs depicted in Fig. 1 *CANNOT be attributed to chance.* This result however is different than that obtained in the previous case when we selected magnitude thresholds: M_S (EQs) $\geqslant$ 5.8 and M_S (pred) $\geqslant$ 5.1. We stress that in **both** cases we have used exactly the *same set of ex-*

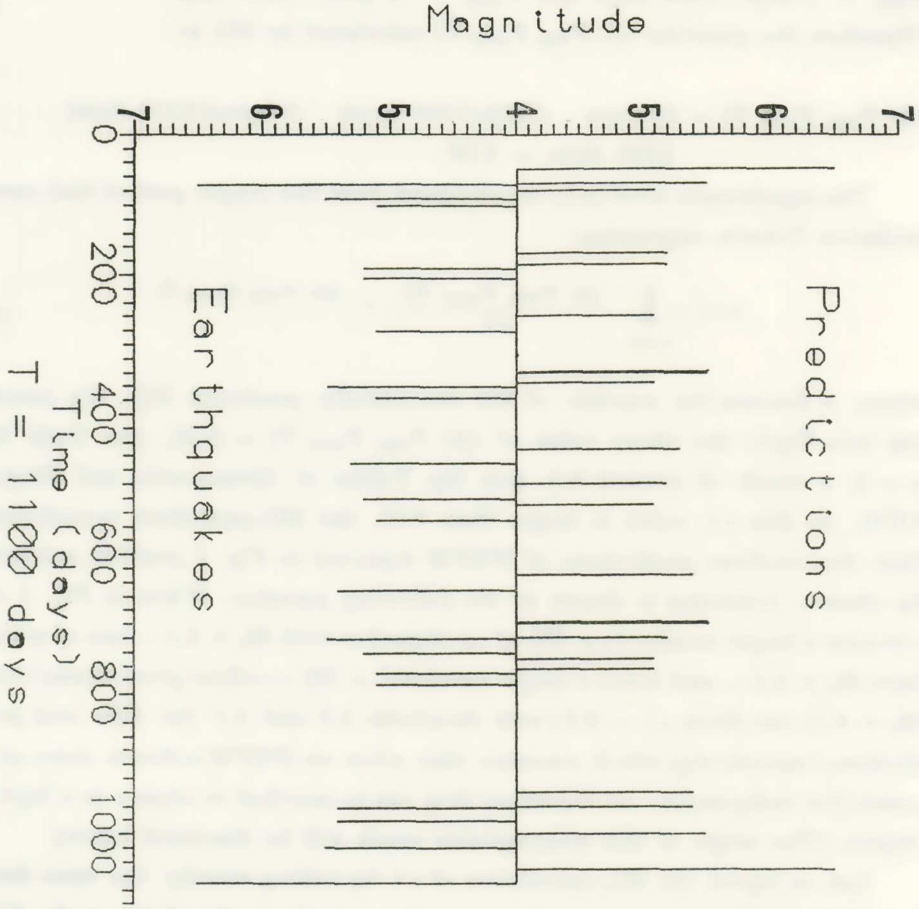

Fig. 1. It is assumed that within almost 3 years (i.e. T = 1100 days) an IPEPM issued 14 successful predictions (upper time chart), preceding (each one with Δt ⩽ 22 days) 14 **independent** EQs (lower time chart) with magnitudes from 5.1 to 7.0. For the two 7.0 EQs the predicted magnitude was 6.3 and hence ΔM = 0.7.

perimental data; the difference in the results come (among other basic reasons) from the selection of *different* magnitude thresholds for EQs and for predictions when checking their association.

Attention is drawn to the point that when we select in the example of Fig. 1 the *same magnitude range*, i.e. 5.1 ⩽ Ms ⩽ 7.0, both for EQs and predictions we find, *as expected*, a s.l.-value appreciably smaller than 0.05.

2. Restrictions under which Poisson distribution is applicable.

The Poisson distribution is derived under the fundamental constraint that the events occurred *independently of each other* and that the probability did not change with time. Therefore the Poisson distribution can be used ONLY when mainshocks (but NOT aftershocks) are considered in the calculation. (Aki, 1956). However MG use in their evaluation *both*, the mainshocks and the aftershocks. It is therefore worthwhile to indicate with three examples *how one can derive questionable conclusions after misusing Poisson distribution* by considering significant number of aftershocks and also selecting *different* magnitude thresholds for earthquakes and predictions.

1st example

Let us consider the case of Fig. 2 in which each at the two remote 7.0 mainshocks is followed by 6 aftershocks with the following magnitudes: two with $M_S = 5.5$ and four smaller EQs with $M_S = 5.4, 5.3, 5.2$ and 5.1 respectively. For all these EQs we assume that an IPEPM issued successful predictions (totally 14 predictions) with $\Delta t = 22$ days, $\Delta r = 0$ and $\Delta M \leqslant 0.7$. For the sake of convenience we assume that the predicted magnitude for the two mainshocks was 6.3 (i.e. $\Delta M = 0.7$) and the predicted magnitude values for the aftershocks were lying between 5.1 and 5.5. By following the MG procedure and considering the thresholds M_S (EQ) $\geqslant 5.8$ and M_S (pred) $\geqslant 5.1$ we have exactly the same numbers as in the case of Fig. 1, i.e. $n = 2$ EQs and 14 predictions, and hence we find the same s.l. value ($= 0.1$). This result is not physically acceptable because to achieve correctly the predictions of 14 independent EQs (e.g. with epicenters at different seismic areas lying far away by hundreds of km) of Fig. 1 is appreciably more difficult than the 14 predictions of Fig. 2 corresponding to two remote mainshocks and 12 aftershocks. Therefore the two cases should have different s.l.-values whereas MG-procedure gives the same. Furthermore in *both* cases the s.l.-value should result appreciably smaller than 0.05 because it is obvious that these predictions cannot be ascribed to chance. One of the reasons due to which the MG-procedure fails to obtain the appropriate s.l.-value is the deletion of a very critical factor that accounts for probability of determining the epicenter by chance (see below).

2nd example

Assume that each of the two remote mainshocks depicted in Fig. 2 was followed by 3 aftershocks (instead of 6) with M_S-values ranging between 5.1 and 5.5 and that they were all predicted exactly as mentioned above. When repeating the MG-calculation under the **same conditions** as above, M_S (EQs) $\geqslant 5.8$, M_S (pred) $\geqslant 5.1$ (and $\Delta t = 22$ days, $\Delta M \leqslant 0.7$, $\Delta r = 0$) we find that the quantity:

$$(\Delta t\ P_{EQs}\ P_{pred}\ T) = 22 \text{ days} \ .\ 1100 \text{ days} \ .\ (2 \text{ EQs}/1100 \text{ days}) \ .\ (8 \text{ pred}/1100 \text{ days}) = 0.32$$

which gives for $n = 2$ a s.l.-value *smaller than 0.05* (and hence the predictions cannot be ascribed to chance). In other words the MG-procedure gave the following result: when we successfully predict two remote 7.0 EQs with 12 aftershocks, their predictions *can be ascribed to chance*; on the other hand when these two EQs have been followed only by 6 aftershocks their predictions are found *to be far beyond chance.* The lack of self-consistency is obvious.

3rd example

Let us examine now in Fig. 2 the «backward» time association of these

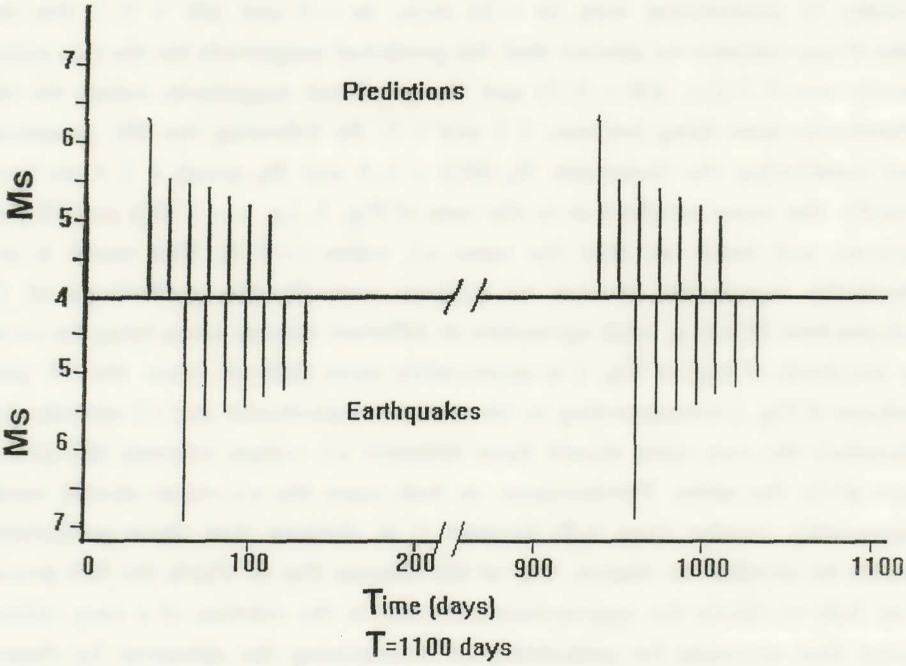

Fig. 2. It is assumed that within $T = 1100$ days an IPEPM achieved 14 successful predictions preceding (each one by $\Delta t = 22$ days) 14 EQs with magnitudes from 5.1 to 7.0. The two **remote** 7.0 EQs are mainshocks and each of them was followed by 6 *aftershocks* (we assume that each aftershock occurs 12 days after the previous EQ). For the two mainshock the predicted magnitude was 6.3 and hence $\Delta M = 0.7$. For the aftershocks the magnitude reported in each prediction may has any value between (or equal to) 5.1 and 5.5.

events under the following condition: examine «if each prediction is **preceded** by an EQ, by a time-lag up to 22 days and with ΔM ($= M_{EQ}\text{-}M_{pred}$) smaller than (or equal to) 0.4-units». We find 8 such «backwards time associations» and hence the MG-procedure gives:

$$(\Delta t\ P_{EQs}\ P_{pred}\ T) = 22 \text{ days} \ . \ 1100 \text{ days} \ . \ (6 \text{ EQs}/1100 \text{ days}) \ .$$
$$(14 \text{ pred}/1100 \text{ days}) = 1.68$$

which, for $n = 8$, gives from Eq(1) the value s.l. << 0.05. This value can be *misinterpreted as indicating that the successful predictions* (achieved by a IPEPM) depicted in Fig. 2 are «post-seismic effects». This misinterpretation should not be done because it is clear that when we have an aftershock sequence and a corresponding number of predictions, it is expected to find a «backwards» association with a small s.l.-value depending on the density of the (non-independent) events. This emphasizes again the critical importance of considering independent events when we study the association of various geophysical phenomena with earthquakes.

3. Probability of a prediction being made by chance.

Varotsos and Alexopoulos (1984) have indicated that when EQs occur at various seismic regions, and the predictions determine time, epicenter and magnitude in advance, the probability of achieving a successful prediction by chance can be described as a product of 3 probabilities (i.e. for time, space and magnitude respectively). For example when we issue predictions within an area of 500 km × 600 km in order to predict the epicenter **only** of one **independent** EQ with an accuracy e.g. $\Delta r = 50$ km, the probability P_e of predicting this epicenter by chance is approximately given by (provided that the seismicity is distributed roughly «homogeneously» in the area of 500 km × 600 km).

$$P_e = \frac{\pi \ (50\text{km})^2}{500\text{km} \times 600\text{km}} = 2.6 \times 10^{-2}$$

(Varotsos and Alexopoulos, 1984, have emphasized that this factor is still important even when studying smaller areas, e.g. 150km × 150 km, because then we find for $\Delta r = 50$ km, $P_e = 0.35$). Such a very critical factor has not been considered by MG-procedure. This point has been also recently emphasized by Takayama (1993) and Lazaridou (1993) who found that a number of s.l.-values calculated by MG (when they analysed the data of Varotsos and Lazaridou 1991) change dramatically after considering this factor. For example for the case of $M \geqslant 5.8$, $\Delta t \leqslant 22$ days, $\Delta r \leqslant 30$ km (Table 1 of MG) the MG-analysis found s.l. = 0.859 (i.e. appreciably larger than 0.05) in contrast to Takayama's value of 0.020 (similarly under the same conditions MG finds, for $M_S \geqslant 5.3$, the value s.l. = 1.00 whereas Takayama's is 0.035).

The deletion of the above decisive factor by MG is one of the main reasons due to which MG-procedure led to physically unacceptable results when we studied above the cases of Fig. 1 and Fig. 2.

4. Can we use Poisson distribution when analysing earthquake prediction data with various time-windows?

As an example we refer to the prediction data presented by Varotsos and Lazaridou (1991). These predictions are based on the observation of the following phenomena: (1) gradual variation of the electric field of the earth (GVEF) for which Δt = a couple of weeks, (2) single SES with $\Delta t \leqslant 11$ days and (3) SES electrical activities with Δt = a few weeks (e.g. 22 days). Each prediction clarifies, well in advance, on which phenomenon it is based. The MG-procedure however, i.e. Eq(1), is based on a certain Δt-value i.e. they consider **or** $\Delta t = 11$ days **or** $\Delta t = 22$ days. Therefore when they consider $\Delta t \leqslant 11$ days they «reject» a number of predictions associated with SES electrical activities and with GVEF. Furthermore when they assume $\Delta t \leqslant 22$ days the allow a significantly larger uncertainty (in the time domain) «by chance» $\Delta t/T$ for the predictions based on single SESs.

In conclusion MG-procedure is simply based on the estimation that the probability of a prediction being fullfilled by chance is $N_{EQs} \cdot \Delta t/T$. This estimation however gives ONLY the probability (P_T) in respect to time and hence MG-procedure **cannot be used** for the analysis of a prediction method that simultaneously determines time-window, epicenter and magnitude. Furthermore even in the time-domain, the MG-procedure uses the relation $N_{EQs}\, \Delta t/T$) by considering as N_{EQs} the totality of EQs i.e. mainshocks and a (larger number of) aftershocks. The latter however is not allowed when using Poisson distribution because in this distribution we have to use ONLY independent events.

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ΠΕΡΙΛΗΨΗ

Σφάλματα κατὰ τὴν μελέτη τῆς συσχέτισης δύο χρονοσειρῶν

Ἕνα τριετὲς συνεχὲς δεῖγμα ἐπισήμων προγνώσεων σεισμῶν, βασιζομένων στὴν παρατήρηση σεισμικῶν ἠλεκτρικῶν σημάτων, ἐδημοσιεύθη προσφάτως. Κατὰ τὸ παρελθὸν ἔτος τέσσαρες ἀνεξάρτητες ὁμάδες ἀνέλυσαν αὐτὸ τὸ δεῖγμα καὶ συνεπέραναν ὅτι ὁ ρυθμὸς ἐπιτυχιῶν εἶναι πολὺ πέραν τοῦ τυχαίου. Ἐν τούτοις μιὰ πέμπτη ὁμὰς διατείνεται ὅτι οἱ προβλέψεις μποροῦν νὰ ἀποδοθοῦν στὴν τύχη. Εἰς τὴν παροῦσα ἀνακοίνωση ἐξετάζουμε τὴν πηγὴ αὐτῆς τῆς διαφωνίας ἀπὸ ἀπόψεως γενικῶν ἀρχῶν. Δεχόμενοι ὅτι ὑπάρχει μία ἰδανικῶς τελεία μέθοδος προγνώσεως, δείχνουμε ὅτι αὐτή, δι᾽ ἀκριβῶς τὴν ἴδια σειρὰ ἰδανικῶς τελείων προγνώσεων, μπορεῖ νὰ δώσει δι᾽ ἐκλογῆς διαφορετικῶν κατωφλίων σεισμῶν καὶ προγνώσεων ἀντιφατικὰ ἀποτελέσματα, δηλ. οἱ προγνώσεις εἴτε νὰ ἀποδίδονται στὴν τύχη εἴτε νὰ ἀποκλείεται τὸ τυχαῖο. Πέραν αὐτοῦ δείχνουμε ὅτι μπορεῖ νὰ ὁδηγηθεῖ κανεὶς σὲ πολλὰ ἐσφαλμένα ἀποτελέσματα, ὅπως π.χ. ὅτι πραγματικὰ πρόδρομα σήματα εἶναι μετασεισμικὰ φαινόμενα, ἐὰν γίνει χρήση ἀκατάλληλος τῆς κατανομῆς Poisson σὲ συνδυασμὸ μὲ τὴν χρησιμοποίηση διαφορετικῶν κατωφλίων.

Κατὰ τὴν ἐκτίμηση τῆς πιθανότητος μιὰ πρόγνωση νὰ εἶναι ἐπιτυχὴς κατὰ τύχην, πρέπει νὰ μελετᾶται τὸ γινόμενο τριῶν πιθανοτήτων, σχετικῶν μὲ τὸν χρόνο, τὸ χῶρο καὶ τὸ ἐπίκεντρο. Ἡ πρωταρχικὴ σημασία αὐτοῦ τοῦ οὐσιώδους σημείου ἐπιδεικνύεται σὲ μία σειρὰ χαρακτηριστικῶν παραδειγμάτων.

Οἱ παρατηρήσεις ποὺ τονίζονται σὲ αὐτὴ τὴν ἀνακοίνωση εἶναι γενικῆς ἐφαρμογῆς κατὰ τὴν ἐξέταση τῆς συσχετίσεως τοῦ τυχαίου μὲ τὰ διάφορα γεωφυσικὰ φαινόμενα καὶ τοὺς σεισμούς.