
The weight of pseudo-objectivity

Jean Vogt

Rue du Docteur Woehrlin 1, Strasbourg-Robertsau, France

Abstract

For nearly twenty years, numerous methodological problems of historical seismology, sometimes set in a wider frame, have been discussed by a continuous sequence of papers. One or another paper emphasized the role of pseudo-objectivity without, however, pronouncing these ominous words. Ultimately the need was felt to deal with the weight of pseudo-objectivity in a straightforward way, although scientists, as one enlightened reviewer of this paper wrote, often do not *dare* to raise problems of a more or less psychological and epistemological kind. Besides general statements, a random set of surprising examples is given, from the author's own experience or from his readings. The major problem of computerized catalogues is emphasized. Even modern macroseismic enquiries, sometimes considered a routine work, don't escape the pitfalls of pseudo-objectivity. While subjectivity is despised, with sometimes extreme statements, condemning historical seismology on the whole, a kind of «constructive subjectivity» with an ability to master complex problems, seems preferable to the dangers of a growing pseudo-objectivity.

Key words *historical seismology – pseudo-objectivity*

1. Introduction

- *Sachez que je n'aime pas les coupeurs de cheveux en quatre!*
- *Perrey a tout dit!*
- *C'est du bluff!*
- *C'est du fascisme!*

Unforgettable words uttered by a famous seismologist trying by all means, even by intimidation, to stop an unavoidable revision of the historical seismicity of France, in the Seventies.

Pseudo-objectivity is a fundamental problem in a wide range of fields, although narrow-minded specialists are often unaware of it and its consequences. In so-called «soft fields», like, for instance, economic and social history, with its source problems, it is clearly a form of

«daily bread». To a large extent, its weight is also heavy in earth sciences. Personally I have experienced it in many instances in the course of geological mapping (conception of mapping itself, contours, legends, etc.), research on mineral deposits (pseudo-bed rocks of bauxite, geochemistry, etc.), discussion of neo-tectonics (for instance in New Caledonia), etc.

Having dealt for two decades with macroseismology in a general way, but mostly with historical seismology, although as an outsider, I have been astonished from the beginning by this major problem. While this complex, interdisciplinary field is often considered by scientific minds and in many cases by specialists of instrumental seismology as *subjective*, actually a common place, it suffers from an excess of what seems to be *objectivity*, but in fact is in many cases *pseudo-objectivity*, a typical paradox of science.

In the last fifteen years this word has not been used by a succession of methodological papers on historical seismology, but the development of this field is such, with a growing set of problems, that the ominous word cannot be

Mailing address: Dr. Jean Vogt, Rue du Docteur Woehrlin 1, 67000 Strasbourg-Robertsau, France.

avoided any longer and should, on the contrary, be emphasized.

However I do not intend to spend much time on a «lower level» of pseudo-objectivity, although examples are numerous, some of them looking like jokes.

2. Some examples

Let us give two typical examples. Dealing with an earthquake of importance for a controversial French nuclear plant a geologist concluded from the absence of information from a given village, some 170 years ago, that it had not been felt there, putting a Zero on the map, despite evidence from the neighbourhood, without even thinking of a question mark. A recent national catalogue, relying to a large extent on a neighbouring countries, rather ancient catalogue, took over, for a 17th century earthquake, a sketch, without adding anything new (a problem we are not discussing here), but altering it, by some misplaced reflex, as follows: while the sketch clearly traced a «limit of knowledge», this line became an isoseist III, discouraging *ipso facto* further research on the macroseismic area, a most important feature in the given case. One of my papers discusses a vaguely cross-like feature shown by the same catalogue for the same event and demonstrates that it is no more than an «artefact» produced by a rather poor historical seismology. Nevertheless a reader wondered why I did not use such a feature for a seismo-tectonic discussion (Vogt, 1994).

This summary interpretation considers mostly an «upper level» of pseudo-objectivity, although such a denomination may sound caricatural. Twice I have heard from «men of science» that genuine seismology and even engineering seismology (yes!) should limit themselves to instrumental data, escaping *ipso facto* the dreaded subjectivity. Asked how he would discuss the seismic risk for a given nuclear project with a «controlling earthquake» from the 15th century, one of them, a C.N.R.S. physicist, was of course unable to give an answer (Vogt, 1993a). Actually a luckily unpublished report on the seismic risk of a part of

Eastern France is based mostly on instrumental seismology, clearly with an inability to master its historical seismicity, despite numerous publications. In a less drastic, but nevertheless typical way, authors of papers and catalogues make, by some kind of automatism, a sharp cut in 1900, a magic year, with the birth of instrumental seismology. All have in common one idea, more or less: subjectivity before 1900, objectivity afterwards. They seem to forget the complex history of instrumental seismology, with its own shortcomings, for many years and even nowadays, with its own problems of pseudo-objectivity. They should have a look at the printed reports from Strasbourg (*Monatsberichte*) from the beginning of the century. They would find on one side instrumental results, on the other numerous unrecorded events, mostly from newspapers (yes!) under the heading: «*Andere nur durch spürbare Bewegungen wahrgenommene Erdbeben*» (Other earthquakes only known from having been felt).

3. Parametric catalogues

Let us however consider the shortcomings of many catalogues. Sometimes introductions do not even explain how they were prepared. Typically this is the case of a so-called European catalogue (Van Gils and Leydecker, 1991, on whose subject see Camassi *et al.*, 1994) which should have remained confidential, for the sake of science. Nowhere is it explained that while most countries provided all known events over a certain threshold, whatever the state of knowledge and the degree of reliability, France limited its information to earthquakes with the best reliability ratings. While a concern for reliability should be highly praised in a general way, it led in the present case to a wholly false idea of the seismicity of France and to a distorted view of the seismicity of Europe, with border problems. Whatever the reasons for France's choice, with, possibly, a rather technocratic view of *objectivity*, it contributed to a large extent, among other reasons, to the *pseudo-objective brand* of this European catalogue even in the instrumental field (Van

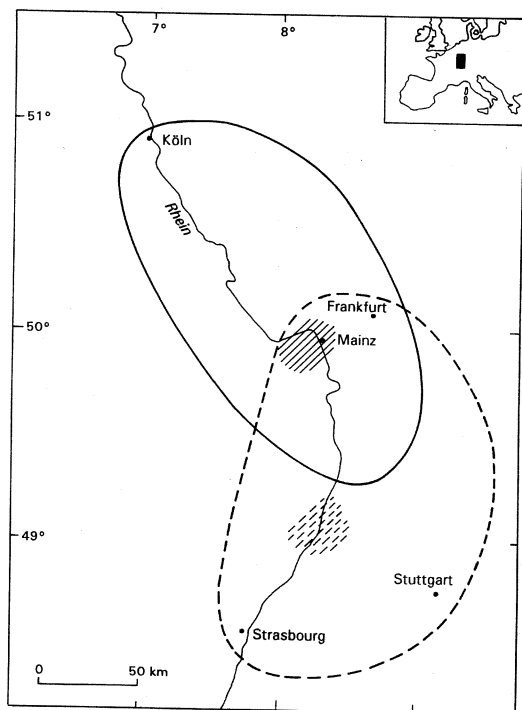


Fig. 1. Reinterpretation of the 28 November 1642 Rhenish earthquake (adapted from Vogt, 1994a). Full line shows the minimal macroseismic area proposed by traditional catalogues, with an epicentral area in the region of Mainz. Broken line gives the results of recent research, with a macroseismic area further south, overlapping the former, and a proposal of an epicentral area halfway between Mainz and Strasbourg. Seismo-tectonic conclusions are still to be drawn from a set of reinterpretations at such a scale.

Gils and Leydecker, 1991). Besides, unhappy wordings by French bodies led to ironical comments on unscientific behaviour and «state secrets», at the very antipodes of objectivity.

The problem of reliability should, however, be discussed in a more general way. Actually, ratings of reliability are absent from most printed catalogues, although question marks are present here and there, and, even worse, among other shortcomings, from listings, a result of hasty computer work, without a critical mind. Typically, the author of a listing asks the

reader *to believe* its content. So we are leaving the field of science for the field of religion, thanks to computers' virtues. In this case, plain belief is actually misplaced since errors of 100 km or so have recently been demonstrated (Vogt, 1994a) at the scale of the Rhine graben for the location of epicentres of several 17th and 18th century earthquakes (fig. 1). Besides, intensity thresholds feed pseudo-objectivity to a large extent. Information for catalogues should instead be adapted to levels of seismic activity of units, possibly seismo-tectonic ones, at different scales. As an example, a threshold could be high in Greece for some purposes, giving nevertheless a satisfactory idea of its seismicity, while it should be low elsewhere, for instance in Great Britain or in the Netherlands. To prevent the dictatorship of thresholds, with its sterilizing effects, not only important, but also *significant* earthquakes should be considered. For practical purposes, magnitude thresholds of course raise even more problems. Uniform thresholds over large areas are typical of a pseudo-Cartesian state of mind like, for instance, the uniform scale of geological maps whatever the degree of complexity of the different areas. In one word, many listings are suspected of being *monuments of pseudo-objectivity*.

4. More examples

Let us turn to more specific problems of historical seismology, with some typical examples of pseudo-objectivity. First let us consider the famous 1580 earthquake, traditionally located somewhere near the Straits of Dover with, extrapolating from damage in Kent and on the continent, an epicentral intensity of VIII or so. Afterwards its interpretation had a stupendous history, shortly related. First, a hasty work led, besides other shortcomings, to a systematic overestimation of intensities with, for instance, an intensity IX for Lille, this degree being based on one piece of second-hand information on the collapse of a church-tower (Neilson *et al.*, 1984). Actually a first-hand source shows that this church suffered only very minor damage, leading, among other evi-

dence, to an intensity VI-VII. Further, an unpublished French report dealing sharply with these misinterpretations, fell into several «pits» of a similar kind. A proposal of a barycentre in the heart of Artois, proposed as a possible alternative for the location of the epicentre, is based once more on several overestimations, mostly in Picardy. Intensities VII at Amiens and Beauvais are the result of an utter inability to interpret sources while a degree VI at Airaines, between Amiens and Abbeville, is clearly inspired by a fine testimony on a 1680 (and not 1580) earthquake. While a first-hand source from Abbeville led twenty years ago to a degree IV-V, the neighbouring uncritical overestimations led the same report to consider this point as an exception. Actually this intensity is perfectly coherent with intensities V or so at Amiens, Beauvais and other points over a large area. As well as the degree VI-VII at Lille, this is one of the conclusions of recent intensive research based on numerous contemporary sources, confirming on the whole, with more and better arguments, the former pattern (Levret *et al.*, 1994) (fig. 2).

Two comments should be made. First so-called barycentres should be handled carefully. Often they are no more than the result of the needs of computer-work. The way the 1470 Rhenish earthquake has been dealt with by an unpublished listing is a typical example of pseudo-objectivity, despite a low reliability rating. This event is known from Basel, with an intensity of V or so, and from a mention in the Kaiserstuhl, with an unknown intensity. Nevertheless, an epicentre is proposed halfway. Actually it could be located anywhere ...

Further, differences of assessments of intensities, two degrees and more, should be emphasized. Indeed such cases are numerous. Recently it has been shown that the 1612 earthquake in Bielefeld areas has been underestimated by catalogues, giving a degree V, instead of a possible epicentral intensity VII (Vogt and Grünthal, 1994). Similar problems arise in North Africa, the West Indies, etc. For the 1827 Martinique earthquake degrees VII, VIII and VI are proposed first by Robson, then by Feuillard and now by myself, after another checking (Vogt, 1994b). Clearly such differ-

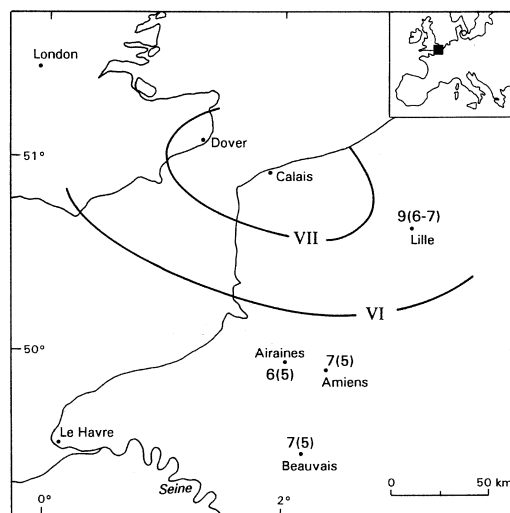


Fig. 2. The devils of overestimation of intensities illustrated by the 1580 earthquake near the Straits of Dover, with an (extrapolated) epicentral intensity of some VIII, with well-established isoseists, VII and VI being shown, as well as degrees at some points (between brackets). However hasty research had led to two sets of overestimations, by two degrees, IX at Lille, on one hand, VII at Amiens and Beauvais on the other, for example, with strange isoseists and even a proposal of an epicenter in Artois (not shown).

ences are not acceptable, neither by science nor by engineering, especially in areas with problems of seismic risk for nuclear plants, etc.

The way the so-called Romara earthquake, Rif, 1909, has been dealt with is another fine illustration of the achievements of pseudo-objectivity. Years ago I expressed doubts about it, considering it as one of the many landslides or rockfalls of this utterly sensitive region. Repeating each other, all catalogues long listed this «earthquake» with an intensity IX, despite a knowledge limited to one point south of Tetuan, without the least information from Tetuan itself, not to speak of Ceuta, Tangiers, etc., from where high intensities should have been reported. Nobody cared until a recent Moroccan catalogue proposed a degree VII (Cherkaoui, 1988). Recently it has been suggested that a huge landslide could have been

triggered by a modest earthquake with an epicentre at some distance. New research however confirms that the Romara event is no more than a major landslide occurring during heavy rains. Probably wordings by newspapers, as «severe earth shock», explain a hasty seismological interpretation by uncritical minds, although the newspapers' accounts themselves, for instance, outstandingly, despatches from the *New York Times* (better than catalogues!) clearly describe a landslide. Discarding «fake quakes», to which specialists often cling like children to their toys, is indeed an arduous task.

While the more amateurish part of historical seismology sometimes looks like a playground with a stupendous display of pseudo-objectivity, we should insist on the fact that this field is increasingly confronted with problems of responsibility and should be dealt with accordingly. In a more general way the credibility of seismology is at stake.

Strangely enough modern macroseismic enquiries also sometimes raise drastic problems of pseudo-objectivity. As an example, it was thought that French enquiries, from the twenties to the seventies, were reliable. An arduous checking of numerous events showed that many of them are far from being so, for examples in Alsace and the Pyrenees. As an example, the way the effects in France of the so-called Rastatt earthquake, in 1933, has been dealt with is astonishing. Without even considering other sources, a checking of original forms allowed to substitute harmonious features to a strange cross-like pattern (Vogt, 1986) (fig. 3). The «official» pattern of a 1967 Southern Alsace earthquake shows from North to South, three separate areas of intensity V, with no possibility to locate an epicentre from macroseismic information. Once more, the checking of forms brought forth one homogeneous area, covering only parts of the former ones, with a rather precise location of the macroseismic epicentre (Massinon and Vogt, 1985, see sketch on p. 181). In the Pyrenees a large reinterpretation of numerous events has been undertaken the same way, with, among other results, a better differentiation of complex sequences, for instance during April and May 1936 (Vogt, 1993b).

The reasons for such shortcomings are clear. In the 1580 case, among many others, insufficient craftsmanship is clearly the main factor, with a lack of interdisciplinary approaches, critical sense and even common sense, leading, through a kind of blindness, to a triumph of pseudo-objectivity. In a more general way, the handling of intensity is responsible for many shortcomings. So the French enquiries were often unable to master, among others, the statistical aspects of the former intensity scale. Typically, cases of degree IV are often sparse while degrees III and V are numerous. Nobody seems to have cared about

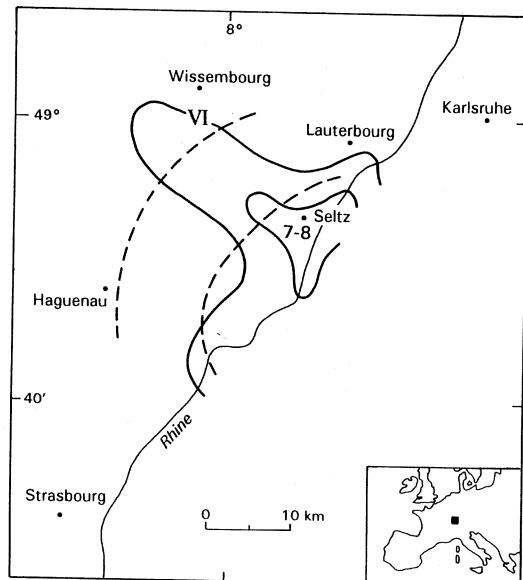


Fig. 3. Reinterpretation of the effects of the 1933 Rastatt earthquake in northernmost Alsace (adapted from Vogt, 1986). Full lines are a selection of isoseists from *Annales de l'Institut de Physique du Globe de Strasbourg* (former scale). Broken lines show the result of a critical reexploitation of enquiry forms, using the MSK scale, with (thick line) an area with mostly intensities VI and some points VII and (thin line) the minimum area of intensity V. The cross-like pattern disappears while the utterly precise isoseists cannot be maintained. So an apparent regression of knowledge is in fact a progress, getting rid of pseudo-objective assessments.

such a statistical anomaly. Worse, the words «few persons» often lead to a degree II even if only few persons are present, for instance in a mountain chalet or in a forestry house. Such a problem seems to be responsible, *pro parte*, for the cross-like pattern alluded to *à propos* of the 1933 Rastatt earthquake, among other cases (Vogt, 1986). Common sense is needed for the best use of intensity scales. As an example the discussion of chimneys is rewarding, a problem raised by G. Grünthal during the arduous revision of the MSK scale (Grünthal, 1993). While a scale cannot differentiate types of chimneys, their diversity should nevertheless be considered. As an example chimneys of old West Indian sugar mills are massive, rather low stone structures. They should not be considered, for the assessment of intensities as proper chimneys, like our slender and flimsy ones, but rather like stone houses. In the course of the reinterpretation of some West Indian earthquakes, for instance in Guadeloupe, it seemed to me that intensities partly based on such chimneys have been underestimated in some cases, by one and even two degrees (Vogt, 1994c). In the Swiss Alps, wooden chalets have not only sturdy chimneys, but also massive ovens. Actually 16th and 17th century sources emphasize damage suffered by such structures, an outstanding criterion for high intensities with, once more, a risk of underestimation.

Of course, seismo-geological effects are a most rewarding field for pseudo-objective achievements. Former scales discussed them in a *qualitative way*, thus allowing discussion of geotechnical backgrounds with the interdisciplinary mind prevailing formerly. Conceived by more specialized people, the MSK scale adopted in a more general way *quantitative criteria*. While they proved rewarding for the statistical demographic approach – and in fact the quality of French enquiries suddenly rose – they are highly questionable for the assessment of seismo-geological features. The value of its utter statistical approach is limited to a large extent by logical flaws and by barriers built between an autarkic seismology and other specialists. To limit a huge risk of pseudo-objectivity the revised scale proposes for each

process a range of intensities and insists, of course, on the need for interdisciplinary work (Vogt *et al.*, 1994).

Whatsoever, a mechanical and *ac cadaver* implementation of intensities scales, as well the questionable as the best ones, led and leads to numerous shortcomings in the course of interpretation of not only former, but also of modern earthquakes. Knowledge of backgrounds, for better differentiation of chimneys, seismo-geological processes, etc., a critical mind and common sense are «added values» to scales. Far from being subjective, as some seem to fear, for instance for the appraisal of seismo-geological effects, they limit the weight of pseudo-objectivity and open the way for more genuine objectivity. Of course, simplified procedures with, it seems, the help of «artificial intelligence» would lead us a long way back to pseudo-objectivity and years of efforts for more objective approaches would be destroyed.

Of course, the problem of pseudo-objectivity appears *en filigrane* in many methodological discussions, even if the word is not pronounced. As an example such discussions have been fed during several years by Alexandre's outstanding contributions on the seismicity of Europe in the Middle Ages (Alexandre, 1990), a period most seismologists are not able to master by themselves. A thorough knowledge of sources, considered reliable or unreliable, with a lot of complex cases, led Alexandre to discard many earthquakes listed by catalogues and listings without proper research, not to speak of drastic relocations. What was evident to him did not at all seem so to several seismologists. Dismayed by the perspective of losing major events used for seismo-tectonic discussions, appraisal of seismic risk, etc., they suspected Alexandre of pseudo-objectivity and asked for demonstrations adapted to their own requirements of objectivity. So the problem of pseudo-objectivity is the heart of a kind of «cultural confrontation» of people with different backgrounds and different perspectives, with nevertheless, from the latest news, a growing convergence, thanks to good will on both sides.

5. Conclusions

Of course pseudo-objectivity is, in this particular field, different from error. Errors are unavoidable, with the best will. Pseudo-objectivity is avoidable and should be battled against with the utmost energy. It is closely linked to a state of mind combining factors like uncritical approaches and even credulity – rather paradoxical in science! – blind submission to narrow formulas and questionable rules, lack of cultural background – frequent in science, most harmful in seismology! – splendid isolation, etc. Partly such a state of mind is covered by the French adjective *scolaire*, at the very antipodes of *scholarly*. Of course mercantile and opportunistic behaviours, as well as so-called *délicatesse*, asking people to swallow anything without protesting, lead to more complex patterns. To avoid the pitfalls of pseudo-objectivity, a quickly growing danger thanks to hasty and irrational computer-work, a kind of *constructive subjectivity* is needed, in a seemingly paradoxical way, with an ability to master complex problems in a critical and interdisciplinary way, a modest approach towards more objectivity, not incompatible at all with the French expression of *libre arbitre*.

POST-SCRIPTUM

Originally not the least reference had been given, in a deliberate way, to stay aloof, applying the «no names» principle, avoiding giving an impression of distributing bad ratings to some bodies and individuals, among many others. The author also felt that quoting his own papers would be misplaced. However reviewers insisted on references. So some are given, with the exception of unpublished reports, reluctantly indeed, in an arbitrary way, with the author's apologies. Instead of quoting once more several of his own papers he privileges forthcoming ones. The other alternative would have been to rewrite the paper, depriving it of its spontaneity. Further, a methodological paper – some would say «epistemological» – should give a set of examples without delving into them, avoiding the risk of becoming a rather specific one, with the usual standards.

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