A Seismological Retrospective of the Brady-Spence Prediction

Susan E. Hough
U.S. Geological Survey

INTRODUCTION

In the annals of earthquake prediction research a small handful of specific predictions are notable by virtue of their impact in scientific and/or societal circles. Of these, the so-called Brady-Spence prediction that a large earthquake would strike Peru in 1981 stands out both for having been put forward by members of the mainstream earthquake science community and for having generated significant repercussions in the international arena. A well researched book-length retrospective was published in 1989 (Olson *et al.* 1989). This book considers the reaction to this prediction from a social science perspective. The lead author is a political scientist; two co-authors are also social scientists. Olson *et al.* (1989) conclude that the scientific community's response to the prediction serves as an example of the hostile reaction that Kuhn (1962) observes to be frequently engendered by scientific research that challenges existing paradigms.

For the scientists involved with evaluation of the Brady-Spence prediction, the episode was generally viewed as an unpleasant exercise, best forgotten. Perhaps as a consequence, a retrospective of the prediction from a scientific perspective has never been published. The purpose of this report is to rectify that.

BACKGROUND

Although it came to be known as the Brady-Spence prediction, the prediction was primarily the brainchild of Brian Brady, a geophysicist who had earned a PhD in geophysics from the Colorado School of Mines and in the 1970s worked at the United States Bureau of Mines, where his research focused on rock bursts in mines. Founded in 1910, the USBM was for years at the forefront of research in the minerals and mining fields. In September 1995 the Bureau of Mines was closed; the bureau's minerals information functions were transferred to the U.S. Geological Survey. By the mid-1970s, when Brady began to formulate his prediction, the agency's fortunes were in decline; in the words of Olson *et al.* (1989), it had become a "poor-cousin agency" of the USGS. Brady's colleague, William Spence, worked with the USGS office in Golden, Colorado.

Brady's interest in earthquake prediction grew out of his earlier research to understand rock bursts: the spontaneous, often violent fracture of rock that sometimes occurs when deep mine shafts are drilled, reducing pressure on neighboring rock. Brady concluded that rock bursts are preceded by characteristic, identifiable patterns of microfractures as well as *Vp* anomalies. Brady (1974a) shows snippets of data from a mine in Idaho that suggest a decrease in microfractures and 6–8% decrease in *Vp* prior to rock bursts. Brady (1974a) further argues that the inferred precursor time for both a rock burst and a coal mine roof fall followed the same scaling that had recently been inferred for earthquake precursors (Scholz *et al.* 1973). Although in retrospect Brady (1974a) draws conclusions based on limited data, with no rigorous statistical analysis, the paper is not conspicuously different in this regard than any number of earthquake prediction papers that were published during the early to mid-1970s, at a time when the seismological community was generally optimistic about earthquake prediction.

From the apparent consistent scaling Brady (1974a) concludes that "similar processes may be involved during failure of rock both in the mine and in the earthquake region." Brady concludes the paper by noting that he has "developed a scale-independent failure theory governing the initiation and growth of shear faulting in dry rock... A detailed account and implications of this theory will be published elsewhere." These papers, listed as "in the press" by Brady (1974a), appeared in *Pure and Applied Geophysics* in subsequent years (Brady 1974b, 1976a, 1976b, 1977).

Brady's series of papers, which formed part of the basis of his prediction, were all formulated on the principle of scale invariance. Brady (1976a) reasons that there should be no difference between the processes that control rock bursts and the processes that control earthquakes. In Brady's view, conventional ideas about earthquake nucleation were off base because they didn't "address the fundamental problem of *how* the fault gets there in the first place" (see Olson *et al.* 1989).

There is compelling evidence that the principle of scale invariance is valid for earthquakes over a wide range of magnitudes (*e.g.*, Abercrombie 1996), but it does not predict any commonality between rock bursts and earthquakes if they are controlled by different processes. Assuming the contrary, Brady (1976a) uses an equation that describes the energy change associated with a collapsing void space to draw conclusions about precursors to earthquakes.

The theories that Brady laid out in his series of *Pure and Applied Geophysics* papers were largely conceptual. He postulated, based on laboratory experiments, that rocks will form a series of tensile cracks under stress and then go on to develop what he called an inclusion zone—not the cracks themselves,

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but a zone of tension/weakness within and/or around the cracks. Within this framework, Brady explained precursory microcracking as the collapse of tensile cracks and rockbursts as the "implosion" of the inclusion zone. He identified characteristic patterns of precursory microcracking, not only the well known tendency of activity to increase prior to rock bursts but also more subtle patterns involving precursory increases and decreases of activity.

THE PREDICTION

Brady started to look at earthquakes in the early 1970s. Looking at foreshock activity prior to the 1971 Sylmar, California, earthquake, he argued that one could identify the same patterns as those that preceded rock bursts: an increase in small events followed by a marked decrease. He further concluded that pre-mainshock seismicity delineated the extent of the inferred inclusion zone, and therefore the eventual mainshock rupture. He concluded that the earthquake could have been predicted, had the patterns been identified beforehand. His papers also continued to tout purported successes in predicting rock bursts. The predictability of rock bursts remains a subject of research today (*e.g.*, Tan *et al.* 2001). Although some rock bursts are preceded by acoustic emissions, reliable prediction of rock bursts, like reliable prediction of earthquakes, has proven to be more difficult than many believed in the 1970s.

On 3 October 1974, a magnitude 8.1 earthquake struck southwest of Lima, Peru, killing 78 people and causing heavy damage. Brady turned his attention to the region and began to be concerned that he was seeing a pattern of activity that pointed to a much larger earthquake in the near future. In particular he was concerned that the aftershock sequence died down abruptly following a magnitude 7.1 aftershock on 9 November (*e.g.*, Langer and Spence 1995). Based on Brady's observations and theories, this pattern suggested that the preparation phase for a great earthquake had begun.

For his part, Spence's contribution was largely in assessing past earthquake activity and the structure of the offshore subduction zone, which he concluded could indeed host a much larger event. Yet throughout the late 1970s he continued to voice strong support for Brady's theories and prediction. He was the convener for a meeting held 22–24 September 1976, to discuss future research in seismology, with emphasis on earthquake prediction. The conference report (Spence and Pakiser 1978) included prominent discussion of Brady's "inclusion theory of earthquake occurrence." In its view of earthquake prediction as an achievable goal, the report was well-steeped in the optimism of the day: "Today seismologists recognize numerous physical precursors to earthquakes. Earthquake prediction research is respectable in the scientific establishment."

Over the several years following, Brady continued to analyze the data and refine his theory. By August 1977 he formulated his initial specific prediction, namely that a magnitude 8.4 earthquake would strike near Lima in late 1980. After further work the prediction became even more dire. In an internal memo written in June 1978, he predicted that "the forthcoming event will be in late October to November, 1981 and that the magnitude of the mainshock will be in the range 9.2 ± 0.2. This earthquake will be comparable to the 22 May 1960 Chile earthquake" (see Olson *et al.* 1989).

Olson *et al.* (1989) presents a well-researched chronicle of the events and communications that followed; the chronicle is summarized briefly here. By 1978 Brady was in communication with a leading scientist in Peru, Alberto Giesecke, as well as top officials at the USGS. Notwithstanding the keen interest in earthquake prediction on the part of the USGS, their officials and top scientists found Brady's prediction unconvincing. At a key meeting at the USGS in Golden on 24 May 1979, scientists from the USGS Menlo Park office pointed to the lack of published papers explaining the theory in full, let alone papers demonstrating its validity. The meeting also included representatives of the USBM, the USGS Golden office, scientists from the lead geophysical institute in Peru, and the U.S. Office of Foreign Disaster Assistance (OFDA), whose mission is to promote hazard mitigation and disaster preparedness worldwide. OFDA weighed in in favor of not dismissing the prediction.

In particular, OFDA science adviser Paul Krumpe attended the 1979 meeting and found Brady's arguments compelling. In a memo to the director of OFDA he wrote, "Brady's hypothesis incorporates modeling of the energetics of rock failure in mines and earthquakes, including models of the deformation zone, rock elasticity, strain, rupture sequence, crack coalescence, feedback processes in tension/compression stress model thermodynamic stability criteria, Tensor Field Equations, Laws of PIZ mechanics, and regional geometric analyses." Krumpe went on to inform his supervisor: "Several USGS participants admitted an inability to comprehend the Brady working hypothesis, its theoretical basis, and applicability to earthquake prediction. The mathematical equations are exceptionally complex and very difficult to understand." He further noted, "It appeared to me that several USGS participants were not fully aware of the many professional published papers by Dr. Brady in the literature" (see Olson *et al.* 1989). Over the following two years Krumpe would continue to champion the Brady prediction in bureaucratic circles.

In June 1979 Brady circulated an internal USBM memo in which he discussed the so-called Palmdale Bulge (Castle *et al.* 1974, 1976). He argued that the bulge was associated with but not due to the 1971 Sylmar earthquake per se, but signified the start of a preparation process of an $\mathbf{M} \sim 7$ earthquake in the Salton Sea region. Following the *Mw* 6.4, 15 October 1979 Imperial Valley, California, earthquake, which struck the U.S.–Mexico border region ~ 50 km south of the Salton Sea, Spence wrote a memo to then Assistant Director for Research Rob Wesson, noting, "This apparently successful forecast lends credibility to the statements of Brady and Spence on the primary topic discussed at the Peru meeting" (see Olson *et al.* 1989).

As the discussion continued in the United States, word of the prediction started to reach the Peruvian public in late 1979. In February 1980 the president of the Peruvian Red Cross visited the director of OFDA to appeal for U.S. aid, including a list of key preparedness items. Word of the appeal leaked to the

Peruvian media, the attentions of which focused immediately on one particular item on the list: a request for 100,000 body bags.

By the beginning of 1980 talk of the prediction permeated all layers of Peruvian society. Many pointed to the prediction as the cause of a significant drop in foreign tourists, then as now a life blood of the economy. The Peruvian Civil Defense was overwhelmed by requests for information.

By spring 1980 the consensus among USGS experts was that Brady's prediction was not well-formulated in statistical terms or based on a solid physical model. Nonetheless USGS leadership found itself in a delicate position. "First," Rob Wesson wrote in a letter to a colleague (Wesson 1980), "there is a real and substantial earthquake risk in Peru. Second, Alberto Giesecke is addressing the problem of risk and the need to develop an increased level of earthquake expertise in Peru. ... Third, the supporters of the prediction (Brady; Bill Spence, USGS, Golden, and Paul Krumpe, USAID) seem to share an almost messianic belief and fervor. Fourth, considering the hassles and risks involved, no one has yet taken the time and effort to attack the prediction directly. (Who wants to state that there will be no large earthquake in Peru within the next few years?)" By the end of 1980, however, pressures had continued to mount, and a meeting of the National Earthquake Prediction Evaluation Council (NEPEC) was scheduled in January 1981 for a formal evaluation of the prediction.

THE HEARING

The NEPEC had been created in 1976 to "review predictions and resolve scientific debate prior to public controversy or misrepresentation." The council, chaired by Clarence Allen, met in Golden on 26–27 January 1981 to review the Brady prediction. Nine council members attended the meeting: Clarence Allen, John Filson, E. Robert Engdahl, David Hill, Thomas McEvilly, James Savage, Robert Wallace, Lynn Sykes, and C. Barry Raleigh. In addition, a number of experts attended the meeting as guests, by invitation or of their own volition, including Rob Wesson, James Dieterich, and James Rice.

The event, attended by TV crews and other members of the media, was strained from the beginning. Brady had counted on spending two days explaining his prediction; Allen suggested that he and Spence wrap up their presentation in a total of five hours, with uninterrupted hour-long blocks to present material (NEPEC 1981).

Brady proceeded to try to explain his unorthodox, highly conceptual theories to the panel. By Spence's later account, Brady "got his back up" after he tried to explain his theory and found himself cut off at every turn (*Science News* 1981). The transcript of the hearing (NEPEC 1981) reveals that midway through the first day Brady launched into a long discussion of his most recent ideas, which attempted to incorporate magnetic forces, thermodynamic stability, and equations that Brady likened to Einstein's field equation. The theories were highly conceptual. As Brady progressed to talk of cosmological horizons, Clarence Allen cut him off, saying, "There has been a request from the committee that we stop this because no one is understanding what is going on." Several committee members noted that they had received no prior information about the very complex theories Brady was talking about. Allen observed, "I hope you appreciate that you are turning members of the Council off to some degree partly by coming up with information that we have not been advised about despite the fact that it has been known for months that this meeting was going to take place, and the prediction [was made] over a year ago" (NEPEC 1981).

Council member Jim Savage told Brady: "This isn't a criticism, but I say, I don't think members of the panel have understood what you are saying. You are wasting your time, and you had better get to something that is perhaps within our comprehension, or present it more thoroughly so we can understand it."

By the end of the first day all of the NEPEC members had, in fact, concluded that Brady's theories were, in effect, off the rails—not simply flawed, but neither rigorously formulated nor well-grounded in theory or observation, and certainly not an adequate basis for prediction. They saw flaws in the theories based on scale invariance, including an error that had been pointed out by Keiiti Aki in a paper that was published in 1981 but had been circulated in draft form earlier (Aki 1981). When Brady started to expound on a connection between prediction and Einstein's theory of relativity, committee members saw the discussion heading into outer space.

At the end of the very long day, panel member Lynn Sykes decided to fly home (Sykes, personal communication, 2009). Another panel member, Rob Wesson, came to the conclusion that it would be necessary to challenge Brady more directly (Wesson, personal communication, 2009). His and his colleagues' resolve was strengthened, and their commitment to politeness weakened, by evening and morning news stories that in their view portrayed the NEPEC committee as having been unable to comprehend the novel theories presented by a brilliant, maverick young scientist. An article published in the Tuesday morning *Rocky Mountain News* (1981a) described how Brady had used "elaborate mathematical formulas" to develop theories that "the times, locations, and magnitudes of certain types of earthquakes can be predicted with extreme accuracy." The article noted, "Panel members admitted they couldn't understand his mathematics and asked him whether the theories were essential to his prediction." The article went on to quote Paul Krumpe as saying: "This is science in the making. It might as well be Einstein up there." The same tone is evident in the concluding paragraph of one article published after the second day of the hearing: "Brady contended his application of Einstein's theory of relativity to breaking rock and earthquakes was an essential aspect of his prediction and complained repeatedly throughout the hearing that panel members didn't want to hear about it," *Rocky Mountain News* (1981b).

The second day of the meeting was thus marked by a different tone. The committee could not understand the fundamentals of the theory or the logical connection between the theory, the data, and the specific prediction. They pressed Brady on specifics at every turn. As Brady tried to describe the nucleation zone in terms of an A_H and an A_C "cosmological horizon," one panel member pressed Brady to write down

the equations for these "horizons" in the simplest possible case. When Brady equivocated in reply, Wesson asked how the panel could be sure that Brady wasn't getting his prediction from *The Tibetan Book of the Dead*. [1]

James Rice, who had been invited to attend the meeting as a non-voting consultant, pressed Brady on the issue rock fracture research. "Fracture has been studied," he observed, "and it has been a scientific subject for a large number of years. There are many problems... which are reasonably understood, and solved, and I am trying to make some contact with that literature and the body of knowledge and the concepts that you are putting forth here."

The committee pressed Brady on other details of the prediction as well, asking repeatedly to see equations. Brady eventually did produce a very simple equation showing that the time scale for precursory earthquake patterns was proportional to the size of the impending quake. The committee pounced on an apparent contradiction. The precursory pattern that Brady had identified prior to the 1971 Sylmar earthquake had played out over about eight years, yet, while it had been just seven years since the purported pattern had developed in Peru, Brady was predicting a much larger earthquake. Brady replied that "[it] has a lot of things going into it, and you have temperature coming in and you have a precursor loading." Rob Wesson replied in turn, "I guess that is why we need the equation."

Brady's interpretation of seismicity patterns was based in part on ideas drawn from critical point theory. As the crust approached a critical state between stability and failure, Brady reasoned, characteristic seismicity patterns would develop including toggling of activity between distant ends of the future rupture zone (Brady, personal communication, 2008). Critical point theory has been explored in a number of recent studies seeking to characterize seismicity patterns (*e.g.*, Tiampo and Anghel 2005; Kossobokov 2006). Although Brady's prediction was based on ideas drawn from critical point theory, and a short conceptual paper was published more than a decade later (Brady 1994), no publications were available for the NEPEC committee to consider.

As the hearing drew to a close, committee member Barry Raleigh remarked on Brady's failure to present a convincing theoretical explanation to explain his prediction. He also noted that Brady's "previous public work has got errors in it, which we have not discussed, but it doesn't give me great confidence... in the so-called work you are presenting here today." He concluded that in his opinion, "the seismicity patterns that you purport to show here are clearly ad hoc, and I see no relationship to theory."

Following an executive session the council noted that it was impossible to say that a major earthquake would not happen on any given day, but their evaluation of the specific prediction was unequivocal. They had "been shown nothing in the observed... data, or in the theory insofar as presented, that lends substance to the predicted times, locations, and magnitudes of the earthquakes."

THE WINDOW CLOSES

Back in Peru, the NEPEC verdict did not squelch a growing groundswell of anxiety among officials as well as the public. Concern was to some extent stoked by Paul Krumpe, who continued to champion Brady in bureaucratic circles. Krumpe explained: "Brady's current hypothesis appears unique in that it departs from accepted Einstein physics (Field Theory) and classical rock mechanics. He offers a comprehensive rational physical explanation for the following elements which, regardless of scale, contribute to rock failure, rock bursts, and the occurrence of earthquakes" (see Olson *et al.* 1989).

In July 1981, after the incident defused, Clarence Allen wrote to the then-head of USAID to express concerns that Krumpe "had taken it upon himself not only to embrace the Brady prediction, but actually to aid and abet Dr. Brady in its promulgation" (Allen 1981). Allen went on to say that "Mr. Krumpe seems to have perceived his proper role as protecting the brilliant, young martyr from the big, bad scientific establishment." Others wondered about the extent to which Krumpe was seeking to further his own ambitions. NEPEC committee members' views of Krumpe's professional judgment were not improved by his later association with the Montana doomsday cult established in 1990 by Elizabeth Clare Prophet (Allen, personal communication, 2009).

The details of Brady's prediction, including the timing and magnitudes of major shocks, had been a moving target since the beginning; by April 1981 he predicted a major (magnitude 8.2–8.4) foreshock on 28 June and a later, magnitude 9+ mainshock.

By June 1981 William Spence formally disavowed the prediction after a predicted foreshock failed to occur, stating in a memo that he always felt the term "Brady-Spence prediction" overstated his role. Brady remained steadfast, although he did say he would withdraw his prediction if the first predicted shock failed to occur.

In April 1981 the U.S. Embassy in Lima had enjoined John Filson, then deputy chief of the Office of Earthquake Studies at the USGS Reston, Virginia, headquarters, to visit Lima in June. The cable noted, "Embassy staff strongly believes that visit to Peru by Dr. Filson at this time would go long way to help allay public fear and put Brady's predictions in proper perspective" (see Olson *et al.* 1989).

Filson heeded the call, arriving in Lima on 25 June. By this time the date of the predicted earthquake, which had shifted several times, had shifted again to 10 July. Word of this update appears to have not made its way to Peru, where anxiety mounted in advance of the 28 June window. Even having been well-steeped in the debate for several years, Filson's visit was a revelation. "I had no idea," he wrote in a report, "of the level of anxiety and concern these predictions had caused in Lima. During my stay, every newspaper contained at least one front page story about Brady; property values have fallen dras-

1 The transcript of the NEPEC hearing is riddled with transcription errors. Scientific terminology is mangled: "defamation" instead of "deformation," "Ace of H" instead of "A_H," etc. The transcript also misattributes the "Book of the Dead" line to Bob Engdahl, rather than Rob Wesson, the committee member responsible for the line.

tically; many who could afford it left town for the weekend, and the people at the hotel where I stayed said their bookings were down to about one-third normal" (Filson 1981). Filson recalls the eerie quiet that he met, walking around what he knew should be a vibrant urban community. At the American ambassador's house where he had been invited one evening, the ambassador's wife served tuna fish sandwiches for dinner. The household staff, including the cook, had gone home to be with their families (Filson, personal communication, 2009).

The date 28 June passed quietly, at least in geological terms. Filson's four-day visit, during which he emphasized the formal NEPEC rejection of the prediction, was front-page news. The Peruvian newspaper *Expreso* (1991) ran a full page, front-page headline that day: "NO PASO NADA" ("Nothing happens.") The subtitle quoted the reaffirmation by noted seismologist "John Philson" (sic) that the prediction had not been scientifically credible. Likewise, the statements of Peruvian authorities, which had formerly been somewhat ambivalent, now expressed unequivocal rejection of the prediction.

In a 9 July report on his trip, Filson expressed concern that the date of the predicted earthquake had shifted at least three times since May, most recently to 10 July. "If he is allowed to continue to play this game... he will eventually get a hit and his theories will be considered valid by many," Filson wrote. His fears were soon put to rest. Brady reportedly began a draft of a formal retraction on 9 July, although it was not sent out until 20 July. Thus was the prediction formally and finally put to bed.

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U.S. Geological Survey
525 South Wilson Avenue
Pasadena, California 91106 U.S.A.
hough@usgs.gov