Seasonal climate outlooks: from revolutionary science to orthodoxy

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Seasonal climate outlooks are prepared and disseminated monthly by the Bureau of Meteorology and other organisations. Such forecasts were not considered feasible when I started in meteorology in 1971. This paper describes my personal view on how scientific understanding about El Niño, or at least its relationship to Australia, has evolved (Nicholls, 2004).

Much of the early work on climate prediction was initiated as a result of the disastrous drought-related Indian famines of the late 19th century. Henry Blanford, the first Imperial Meteorological Reporter to the Government of India, observed that the atmospheric pressure over the countries surrounding the Indian Ocean had been abnormally high during the Indian monsoon failure of 1877 and 1878. Blanford requested information on atmospheric pressure conditions at this time, from other meteorological observers around the world, including Charles Todd in South Australia. Todd was intrigued by Blanford's report, and noted that Australia was also in drought in 1877. In the next major Australian drought, in 1888, Todd concluded that "there can be little or no doubt that severe droughts occur as a rule simultaneously over the two countries". This tendency for Indian and Australian droughts to occur at the same time we now know is just one of the many "teleconnections" that are an essential feature of the El Niño - Southern Oscillation phenomenon.

Over the next few decades several meteorologists, most notably Gilbert Walker, one of Blanford's successors in India, documented many teleconnections, named them the Southern Oscillation, and demonstrated the phenomenon's potential use in seasonal prediction. In Australia, E. T. Quayle showed that atmospheric pressure in Darwin could be used for seasonal prediction in southeast Australia (Quayle, 1929). Quayle simply related the observed atmospheric pressures at Darwin with later rainfall over southeast Australia. Darwin pressure is half of the Southern Oscillation Index or SOI, an index now commonly used in Australian seasonal prediction. Quayle was, in part, inspired by Walker's work on the Southern Oscillation to look at Darwin pressures as a predictor, although he had much earlier (1910) suggested that the number of "monsoonal" depressions over summer could be used to forecast winter Victorian and New South Wales rainfall. Treloar (1934), another Bureau scientist, also built on Walker's work and presented empirical evidence that total September-May rainfall in far north Australia could be predicted using Darwin pressures. There seems to have been little, if any, effort to use Quayle's (or Treloar's) work in operational climate prediction in Australia. I suspect this was because:

1. The work underlying the forecast relationships was purely empirical, with doubts about whether these relationships would continue to hold in the future. Quayle only had a few decades of data to examine, so there was a good chance that the relationship was a fluke.
2. No one had a theory to explain the teleconnections or the lag relationships. Grant (1956) was speaking for most atmospheric scientists when she said noted the "dangers of purely empirical studies of relationships between meteorological variables. Such relationships can only be established by an increased knowledge of the underlying physical processes".
3. The empirical relationships did not work very well through the 1920s and 1930s, which was when Quayle, perhaps, was testing them.
4. By the late 1930s the focus of meteorologists had shifted from climate to daily weather forecasting, especially for aviation.
5. From the late 1940s, as computers were developed, attention shifted even further to weather prediction, including the development of computer models.

6. It became clear that we needed massive amounts of data to forecast even a few days ahead, so the possibility that simple statistical methods might be useful in predicting a season ahead seemed naïve.

For these reasons, seasonal climate prediction slipped of the agenda of meteorological services. Two public comments, made about 60 years apart, on the feasibility of climate prediction, illustrate the change in emphasis and ideas. The first was published by Ellsworth Huntington, the geographer and environmental determinist, who visited Australia in the early 1920s and asked:

> "Why should not Australia find out how to predict the general types of weather six months or a year in advance?…That this can be done and that the general character of the weather for six months or a year in advance can some day be predicted with considerable accuracy seems certain. It is merely a question of securing able men, giving them every possible facility for investigation at home and abroad, and permitting them to study their problems for years without being forced to show immediate practical results" (Huntington, 1925, pp 415-416).

Sixty years later we no longer had Huntington's confidence. At a 1983 meeting on El Niño's impact on Australia, a senior Bureau scientist was asked why the Bureau did not issue forecasts based on the statistical relationships found by Quayle and others. In reply he pointed out that we needed data from all over the world to make forecasts for just five days ahead, so the idea that a single number (the SOI) could be used to forecast a season ahead was just not credible. This was the orthodox view about climate prediction amongst meteorologists in the 1970s and early 1980s.

In Australia, the Bureau of Meteorology experimented with thirty-day forecasts in the 1960s. These forecasts extrapolated the observed broad-scale circulation fields around the hemisphere (ie, they did not use any information about the El Niño - Southern Oscillation), and used these to estimate temperature and precipitation for the ensuing month. The forecasts showed little if any skill and were discontinued at the start of the 1970s (Anon., 1972).

Despite the widespread belief that climate was fundamentally unpredictable, some researchers continued to study the (now unfashionable) Southern Oscillation (Allan et al., 1996, provides a history of the research into the phenomenon; also see Glantz and Nicholls, 2001, for a chronology of interest in El Niño). My first job in the Bureau was as the most junior member of a team studying a severe drought in Papua New Guinea. Not until the following year did it became apparent that the El Niño of 1972 had been a major influence on the Papua New Guinea drought (Nicholls, 1973). The main reason for this delay in attributing the 1972 drought to El Niño was the slowness of data collection, especially from the oceans – we simply did not know that an El Niño was under way in 1972. But neither had we been looking for an El Niño – the El Niño was not on anybody's radar.

After my experience with the New Guinea drought and El Niño I was convinced that we could use the Southern Oscillation for seasonal prediction. When I stumbled across the early work of Quayle, I realised we now had 60 years of independent data, not available to Quayle, to confirm that Darwin pressures (ie, the SOI) could be used to forecast east Australian rainfall from about the middle of the year (Nicholls and Woodcock, 1981). The new, independent data also allowed confirmation that Indonesian early wet season rainfall could be predicted, as had been suggested by Braak (1919) over 60 years earlier (Nicholls, 1981). It also showed that the date of onset of the Australian wet season and seasonal tropical cyclone activity around Australia could be predicted using the SOI (Nicholls, 1979; Nicholls et al., 1982).

Tentative attempts to forecast an early onset to the North Australian wet season were made in late 1981, based on the relationship between onset date and the El Niño - Southern Oscillation. A forecast was released to the media in Darwin, but the Bureau of Meteorology decided not to implement an SOI-based forecast system at that time because it only worked for some seasons and regions and because the relationship and why it could lead to climate predictions was not understood. The work was purely empirical, with no theoretical understanding of the causes of these relationships. By the time the major El Niño of 1982 appeared there had been some improvements in data collection, and it had been realised that the El Niño was the product of tropical Pacific air-sea interaction. Some scientists had even found what appeared to be useful clues to the onset of an El Niño. Satellites now provided global sea surface temperatures.

However, the scepticism of many atmospheric scientists about the use of El Niño in climate prediction was still strong, partly because there was still no fully-developed theory of the phenomenon, and no computer model had simulated the phenomenon. In 1986 the success of the first computer model forecast of an El Niño changed the situation. The forecast, by Mark Cane and Steve Zebiak, was received suspiciously by many (including me) but it proved to be correct. This successful forecast made El Niño - Southern Oscillation forecasting respectable in the eyes of most meteorologists. By this time, some of the details of how the ocean and atmosphere interacted to link the (atmospheric) Southern Oscillation and the (oceanic) El Niño had been worked out. This also helped legitimise the phenomenon, in the eyes of physical scientists, leading to enhanced interest in its use in prediction.

Media and political interest in the phenomenon also increased. After an article about El Niño and its impact on Australian crops appeared in the Adelaide Advertiser in early 1988, Senator Robert Ray, representing the Minister for Administrative Services, answered a question without notice about seasonal forecasting. The question alluded to an article in the Adelaide *Advertiser* claiming that Bureau of Meteorology funding was insufficient to allow El Niño monitoring and prediction. Ray stated that "The Bureau's ability to monitor these climatic fluctuations and its scientific understanding of phenomena such as El Niño and the so-called southern oscillation have reached the stage where it expects soon to be able to begin issuing regular public advice through the media on the general state of the climate system over Australia and the likely seasonal outlooks" (Hansard, Senate, 20 March, 1988). Mike Coughlan, from the Bureau's National Climate Centre, and I had been preparing and revising a proposal for the forecasts for over a year at that stage. Through 1988 we prepared trial seasonal outlooks, but these were not released to the media (they were stamped "Confidential"). Then a *New Scientist* article on 25 March 1989 criticised the Bureau for not releasing warnings of the likelihood of widespread wet conditions. Severe flooding had followed these heavy rains. The article observed that the Bureau had noticed the similarity of the current climate situation to previous widespread flooding events "but feared that they might be wrong and so made no public warnings". Soon afterwards the seasonal outlooks started to be distributed to the media. These forecasts were still based on the statistical relationships dating back to Quayle's early work (Nicholls, 1990).

The very strong 1982 El Niño had led to a major improvement in observations. The equatorial Pacific Ocean was now instrumented with buoys that reported sea surface and sub-surface temperatures, via satellite, almost instantaneously. By the 1991 El Niño we were monitoring the east Pacific in "real time". In 1994, when the next El Niño developed, we issued a press release in early June stating that an El Niño was on the way and predicting an increased probability of drier than normal conditions. This forecast was issued just as a major rain-bearing system drifted over southeast Australia. Even as we made the forecast we knew we would be lambasted by colleagues, friends and the media for talking about a drought while the rain pelted down. But the rain lasted less than a month, and the remainder of the year was relatively dry, as expected.

By the time of the 1997 El Niño event we were changing the presentation of our forecasts. Because the climate has a strong "chaotic" element forecasts should not be expressed in deterministic terms. Instead, an El Niño shifts the probability distribution, making drier than normal conditions more likely than in other years. Our forecasts, therefore, were now expressed as probabilities.

During the 1997/98 and 2002 El Niño events there were El Niño cartoons, El Niño jokes, El Niño posters. Michael Hall from the NOAA Office of Global Programs has pointed out that the use of the El Niño - Southern Oscillation in seasonal climate prediction is the most important contribution of climate science to agriculture since the annual cycle was recognised more than 10,000 years ago. This change in attitude to the El Niño – Southern Oscillation and its use in prediction occurred in a very short time, a few years around the 1997/98 event, even though scientists had been preparing the foundations for this change for more than 100 years. The development of models such as POAMA (http://www.bom.gov.au/bmrc/ocean/JAFOOS/POAMA/) capable of simulating and predicting the El Niño has been key in causing this "sea change" in the credibility of seasonal prediction based on the El Niño – Southern Oscillation. Now other phenomena (Indian Ocean sea surface temperatures, the Antarctic Circumpolar Wave, the Pacific Decadal Oscillation) are regularly proposed as ways to improve our climate predictions, with much less evidence than Quayle had 70 years ago for the use of the Southern Oscillation (and little evidence that these phenomena can be simulated or predicted). Those who for decades proposed the use of the El Niño – Southern Oscillation in climate prediction are now characterised as "wet blankets", unable to see the extra skill obtainable through the use of these other phenomena. The El Niño – Southern Oscillation has progressed from radical science to orthodoxy.

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