Conformity to observations:
A banner and bane in the development of weather prediction.

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1. INTRODUCTION

The development of weather prediction has taken place against the backcloth of three strikingly diverse driving factors :- the scientific challenge to forecast the temporal evolution of a complex physical system; the socio-economic motivation to provide timely and reliable forecasts to guide human activity and thereby avert disruption; and the socio-managerial exercise of pursuing science under conditions of public scrutiny and interest.

In combination these factors have exerted a profound influence upon the development of weather prediction. Concomitantly the history of weather prediction provides an early example of the scientific enterprise's response to these distinctive set of factors. An example that is not without interest because it parallels many of major challenges currently confronting the scientific community.

Here the focus is on the strikingly creative nature of the community's response in the early twentieth century. Three prototypical approaches were promulgated for weather prediction. Brief consideration is given sequentially to the then-prevailing general scientific scene and the specific meteorological and societal setting; the nature of the three approaches including the rationale espoused by the respective proponents, and their reception by the scientific community. Comments are made on influence of the forementioned driving factors, and the role of observations in promoting, regulating and validating the various approaches.

2. THE SETTING

At the end of nineteenth meteorology retained a predominantly descriptive character and there remained an inadequate recognition of the underlying physical principles. Nevertheless it was slowly being accepted as a legitimate scientific activity worthy of research funding. Leading physicists, whilst exponents of reductionism-like approaches, were also careful and enthusiastic observers of atmospheric flow phenomena and were eager to provide scientific understanding of the accompanying natural processes.

However the status of science posed a dilemma for both mainstream scientists and practitioners of weather prediction. On the one hand the prevailing ethos placed emphasis on the solid foundations of science and the reliability of its results, but meteorology could offer neither. On the other hand the increasing cost of conducting scientific research prompted the need to better explain and justify its value to the wider community, and to harness its power to provide useful predictions. In the latter context there was a demonstrable and acknowledged need for reliable weather forecasts.

It was in this climate Robert Fitzroy, the first director of the weather service in the UK, launched a forecasting programme, and thereby provided an early example of the importance of 'conformity to observations'. He recognised the need for a network of surface observations to monitor and forecast in-coming weather, obtained funding to support his efforts from the British Association, and proceeded to issue forecasts of storm occurrence. His attempts met with a mixed reception. He was reviled and ridiculed by the press, respected and appreciated by the maritime-user community and informed meteorologists. (Note that Buchan records that he achieved an 80 % success rate). The science community was unable to stomach this lack of reliability and the programme was terminated. Responses to a questionnaire addressed to the international meteorological community indicated its desire to distance themselves somewhat from "forecasting" and to limit themselves to issuing guarded advice on the likelihood of severe weather.

3. THREE APPROACHES

In the first two decades of the twentieth century three novel but radically different approaches were proposed to predict the weather. Each encapsulated a mode of conducting scientific research that have become fashionable and stylised, and moreover the modes epitomise different conceptions of the scientific method that became a battleground for philosophers of science.

(i) The Empirical-Inductive Approach

In the years around 1920 the Bergen School of meteorologists led by Vilhelm Bjerknes honed an approach to weather prediction that was avowedly Baconian in conception. It was based upon the careful and systematic synoptic analysis of the limited available meteorological observations with a view to extracting the salient aspects of the evolving weather pattern. The future evolution of the prevailing weather pattern was then inductively inferred by historical analogy.

In approach, and at least partially in content, it built upon the slowly accrued knowledge of the structure of synoptic weather patterns. However a reinforced and special emphasis was placed upon two quasi-discontinuities in the surface patterns that were diagnostically related to well defined weather signatures. These discontinuities were eventually assigned the terms of cold and warm fronts. The key novel and delightfully perceptive assertions were that these frontal structures underwent a characteristic space-time evolution in the form of frontal-wave cyclogenesis, and that the future atmospheric state could be inferred by diagnosing the current phase of the evolution.

The Bergen School were adamant that their approach held the key to successful forecasting and that better observational data would lead to improved forecasts. From the onset they were also reluctant to introduce modifications to their schematic of frontal cyclogenesis.

(ii) The Hypothesis-Deductive Approach

In a series of papers from 1904 to 1912 Felix Exner, a member of the renowned Viennese School of meteorology and climatology, pursued a weather prediction programme that both in its form and function was a forerunner of the hypothesis-deductive perception of scientific research. The programme sought to identify and isolate one, conceivably major, aspect of the evolving weather pattern, and to evaluate its impact upon the subsequent development.

The underlying rationale was linked to a significant feature of the synoptic flow. It was recognised that the outbreak of a cold air at a specific location occurred as a surge from a more polar region and was accompanied by a sharp increase in the surface pressure. Exner studied this effect by constructing a simplified mathematical model that sought to isolate the underlying mechanism - surface pressure change associated with the low-level adiabatic advection of air. Moreover to evaluate the effect Exner introduced potential temperature (sic. entropy) as a dependent variable, made observationally-based insightful heuristic assumptions (- geostrophic flow), resorted to finite-difference numerical techniques and/or quasi-analytical simplifications, and refined the model to allow consideration of some diabatic effects.

His idealised model can be viewed as a major achievement and the forecast itself was relatively successful. However Exner did not persist with this approach. He and his colleagues in the Vienna became convinced that tropopause-level effects played a significant role in day-to-day weather variations, and he came to view his model as merely providing an estimate of the decidedly limited contribution of low-level advective effects. Nevertheless his approach captures the essence and bears the hallmark of current day studies in the realm of geophysical fluid dynamics.

(iii) The Complex System Model

A stunningly novel approach to numerical weather prediction was set out by Lewis Fry Richardson in his book on the subject. His approach epitomises that adopted in many present-day studies of complex systems.

At root his reasoning was based upon the juxtaposition of two disparate considerations. On the one hand the atmosphere and its interaction with the underlying surface was a complex system governed by a known set of equations that included a myriad of physical processes. On the other hand the system's temporal evolution could in principle be evaluated by solving the governing equations numerically in a forward-marching process subject to an adequate representation of the flow and the initial atmospheric state.

In effect his study was an exploration of this hypothesis. He achieved several notable goals:- he identified the key governing equations and introduced a masterful and necessary major simplification in the form of a diagnostic equation for the vertical velocity; he inferred the spatial scale of weather systems and thereby set criteria for an adequate spatial numerical representation; he developed simplified representations for land-surface and radiative processes; and he carefully compiled an observational data set based upon the limited amount of available data (- secured in part from earlier studies of upper-level flow by Bjerknes). He failed notably in producing a reasonable forecast.

Richardson himself subsequently attributed the failure to inadequate observational data particularly for the upper-air, and set to develop techniques to acquire such measurements.

4. REACTION

The response to the three approaches diverged widely. This was true amongst the respective proponents of the approaches, their contemporaries, and in the subsequent level of their acceptance by the scientific community.

It is noteworthy, but not always recognised, that the proponents were aware of each others efforts. In particular during the two-part conference in Bergen in 1920 when the Bergen School unveiled the latest version of their approach, all three were present at various stages. Bjerknes and Exner made presentations and Richardson participated in post-presentation discussions.

In his presentation Bjerknes alluded to the inadequacy of the numerical approaches and in effect attributed Richardson's failure, at least in part, to non-consideration of frontal discontinuities. It was an assertion that Richardson subtly and urbanely sought to discount in his book. In turn Richardson alerted the Bergen School to a crucial missing ingredient of their conception, viz. the waves growing on the frontal discontinuity might be a manifestation of a physical instability.

In his presentation Exner reviewed the extant knowledge on "frontal" dynamics and thereby formally recorded the known historical pedigree of the Bergen School concepts that stretched back on the observational side to Napier Shaw and on the theoretical side to Helmholtz and Margules. In addition he synthesised his own contribution to frontal studies including his explicit evaluation of an idealised frontal surface's movement. His own weather prediction studies are not mentioned in his manuscript of the presentation, but an allusion is made to the possible influence of upper-level effects. Later he and his colleagues in Vienna were to draw attention to various mismatches between the Bergen School concepts and the observed features of fronts (e.g. their finite width and length).

Richardson was aware of Exner's pseudo-prediction studies, and indeed in his own book he referred explicitly to the pertinent portions of Exner's book that record this work. It is also noteworthy that Exner's book clearly sets out Margules' derivation, and caveat regarding the use, of the surface pressure tendency equation - a caveat that is now claimed to account for Richardson's forecast failure.

Wider reaction to the three approaches hinged in part upon the perception of their success as forecasting tools. In this context empirically-based approaches by their very nature should appear to conform to observations. This was instrumental in the propagation and in the ultimate acceptance of the Bergen School's methods. Moreover subsidiary refinements could be incorporated as further, possibly discomforting, observations became available. In short it was difficult to disprove a method that conformed reasonably with observations, was sufficiently elastic to allow for refinement, and for the most part provided tolerable forecasts. However it suffered from a lack of theoretical underpinning of the core frontal-instability concept, and was later undermined by the burgeoning observational and theoretical evidence for baroclinic development involving the entire troposphere. To a measure the Bergen School concepts provide an illustration of both the nature, power, and shortcomings of the empirical-inductive approach

Exner's approach was overlooked for decades, but was subsequently accorded some attention. This approval has some merit since the approach hinges upon the exploitation of a Lagrangian invariant (- potential temperature) and the systematic use of geostrophy. Indeed his approach was reinvented decades later by Kibel. It can also be compared with the approach adopted by Charney and his colleagues in the late 1950s who used a Lagrangian invariant (- first the absolute vorticity of a barotropic encironment, and later the potential vorticity of a baroclinic environment) and the systematic use of quasi-geostrophy. More strikingly it provides an early and succinct recipe for the hypothesis-deductive approach, and moreover the desert is Exner's willingness to relinquish a promising line of research in the face of persuasive counter evidence.

Richardson's dream of numerical weather prediction was first classified as a glorious failure and then with the advent of high-speed computers paraded as a prime example of system modelling. The original forecast failure pointed to the need for better observations, discouraged others from pursuing the same track, and highlighted the need to better understand the nature of the system. Today's system modelling community can benefit from recognising these caveats and repercussions.

In summary the formulation and development of the three approaches illustrate delightfully the complex relationship between weather prediction and conformity to observations. The latter serving both as a banner and bane in the development of the subject.