16th Australasian Wind Engineering Society Workshop
Brisbane, Australia
18-19 July, 2013

Best Practice in Tropical Cyclone Wind Hazard Modelling: In Search of Data and Emptying the Skeleton Cupboard

B. A. Harper

GHD Pty Ltd, Brisbane, QLD 4000, Australia.
Systems Engineering Australia Pty Ltd, Brisbane, QLD 4006, Australia

Abstract

The title of this paper might suggest that numerical and/or statistical modelling is the focus of discussion – however it is *data* that underpins all *models* and the quality of data used to calibrate and verify models is what makes models useful. This paper therefore relates progress in recognising inconsistencies in the historical datasets used for numerical and statistical modelling of tropical cyclone (TC) wind hazard, storm surge, waves and their associated damage to the natural and built environment.

The developments described were driven by Australian-led enquiry and questioning of what had become accepted practices internationally for the past 30 years, yet rarely tested as to their reliability and objectivity by either meteorologist or engineer. The intersection of this enquiry and increased pressure to obtain insight into the potential impacts of anthropogenic climate change have resulted in an increased focus on improving the precision of TC-related data for the future and to correct past data biases. This paper outlines that journey and our progress to date.

Introduction

As noted in Harper (2001) there is a rich history of development of methods for modelling the wind, wave and storm surge impacts of TCs that involve a wide range of deterministic, probabilistic, dynamical and parametric approaches. These very rare and severe weather events, whose region of peak intensity is at a scale of order 50 km, can nonetheless create very significant impacts of order 1000 km. Peak winds are associated with the smallest of these scales, while peak waves can be associated with the largest, and the scale of storm surge impacts are in-between.

It has long been recognised that in order to obtain reliable estimates of the probability of extreme winds from such rare small scale events it is necessary to perform long-term stochastic simulation (e.g. documented by Walker 2013). This method superseded the purely statistical modelling of fixed-point wind measurements from the 1970s onwards to meet the demand for continuous spatial and temporal windfield modelling.

However this development relied on other sources of data that characterise the TC event itself, and models of the structure and behaviour of such storms, that would allow a reliable estimation of surface winds given a set of defining parameters. Once such a model was built it could be tested and compared to available point measurements both deterministically and probabilistically, and sensitivity testing of its parameters could be carried out.

Notwithstanding the patent advantages of simulation techniques, with several degrees of freedom in their parameter space, competing models have been able to coexist in harmony without the need to agree on some of the most fundamental assumptions of TC behaviour. This has limited the predictive precision of these models out to the low limits of risk now commonly demanded (e.g. 10,000 year Return Periods or less). This can only be improved with increasing attention to data accuracy.

Historical TC Data Quality – The Elephant in the Cupboard

The essential data required for TC wind hazard simulation, in basic terms, is:

- Track – determines the spatial and temporal encounter space and the forward speed component
- Size and Structure – determines the spatial distribution of the hazard
- Intensity – determines the peak magnitude of the storm-relative wind hazard and modulates structure.

Track: Holland (1981) was the first to systematically consider the accuracy of the Australian TC data set in general and concluded that prior to the earliest Infra-Red (IR) satellite images circa 1960, the detection rate of TCs and hence estimates of their track is much diminished, being highly dependent on shipping and aviation encounters. Limited range coastal radar only became available in the late 1960s. Similar caveats apply to many other TC regions.

Size and Structure: Storm spatial scale information is essential for modelling the radial wind profile and a vital contributor to the point wind risk, but there is little or no information available for the vast majority of historical TCs worldwide. Although the Bureau of Meteorology (BoM) has acted to populate the dataset with eye diameter and wind speed radii estimates over the past decade, the yield is small and these metrics are not easily adapted into the modelling framework (for reasons discussed later). Internationally, the situation is similar or worse.

Intensity: Fewer than 2% of Australia's TCs in the historical record (say circa 1950+) have even a single point-in-time intensity based on a reliable physical measurement of either a central pressure or a peak wind speed (Harper et al. 2008). This should not be a surprising statistic given the obvious low likelihood of encountering an eyewall passage (of order 50 km width) using fixed instrumentation across sparsely settled northern Australia where Automatic Weather Station (AWS) separation has at best been of order 200 km only since the mid-1990s. The possibility that such a measurement would also be the peak intensity of such a storm having a typical lifetime of several days and now being at or near the coast is also very unlikely. In addition, having an eyewall passage measurement, depending on the asymmetry of the storm and the reliability of the instrumentation, does not guarantee precision in the peak wind estimate.

So what is our database of historical TC events and how reliable is it for simulation modelling? Up until the late-1990s, most wind-risk professionals using historical TC data at least limited their sampling to the post-satellite circa 1960 era and were critically aware of the lack of spatial scale information (e.g. Harper et al. 1989, 1993; Harper 1996ab, 1999) and applied rudimentary scale assumptions linked to sparse data, often latitude-based. However most users of the historical TC data were still largely unaware of the significant uncertainty in intensity estimates, the influence of remote sensing improvements and changes in agency policy and methods over time that create bias. These biases were to later figure significantly in the heated debate over inferred climate change trends in 2005 (refer later).

It would be an initiative of Woodside Energy Ltd in 2001-2002, in support of their ambitious "10^{-4} Waves Study"[1], that opened a *Pandora's Box* of issues surrounding the estimation of TC intensity worldwide. I was fortunate to be involved in this project and to lead the investigation into past and present estimation methods and agency practices worldwide. The resulting project report (Harper 2002), although unpublished, remains one of the more widely cited of its type in the literature and has been influential in encouraging a number of atmospheric science researchers and operational meteorologists to re-examine a wide range of historical practices, hopefully leading to significant improvement in forecasting accuracy and, over time, the quality of historical records.

An influential precursor to the Woodside development was Harper (1998); an opportunity facilitated by long term colleague Greg Holland[2] to improve interaction between professional engineers and meteorologists through the quadrennial WMO IWTC[3] conference. This interaction with the operational and research meteorological community had a pivotal impact on my appreciation of the difficulties faced by meteorologists when estimating TC metrics in general and intensity in particular. It established strong and lasting relationships with many of the leading figures both internationally and in Australia. It also strengthened my existing relationship with the BoM and especially Jeff Callaghan[4], with whom I have variously published. These developments were also timely for the renewed interest in storm tide hazards across Queensland that led to the so-called "Blue Book" best practice approaches (Harper 2001).

Historical TC Intensity Estimation Techniques

In Australia, the Mean Sea Level Pressure (MSLP) of the TC centre has long been the preferred BoM metric for intensity and because of this it typically formed the basis of statistical analyses of storm intensity (e.g. Harper 1980). One can argue that due to the inherent natural variability of wind sampling alone, pressure estimates are the more reliable, but this has not been the practice worldwide. In any case, Harper (2002) noted the importance of knowing how TC intensity was generally estimated so that the application of analytical models to construct spatially varying surface pressure and wind speeds could be made consistent with that process. One of the fundamental physical assumptions in this regard is the reliance on gradient wind balance linked to the MSLP of the storm centre.

Much of the 2002 Woodside investigation then necessarily focused on the operational use of the satellite image pattern recognition system due to Dvorak (1975, 1984), which has underpinned almost all TC intensity estimates since its earliest introduction circa 1972. This method provided an empirical estimate of the peak surface wind speed based on a sequence of visible spectrum (VIS) cloud image pattern types and later enhanced this more quantitatively by Infra-Red (IR) spectrum temperature potentials. Implicit in this approach was the adoption of a "regional wind-pressure relationship" to convert the wind speed estimate to an MSLP. Hence, the Dvorak method already assumes a wind-pressure link that should, for closure, be reflected in any application of a gradient-wind based analytical model (e.g. Holland (1980) and variants).

Prior to the Dvorak method becoming fully established, the method of pressure profile plotting and extrapolation from surface observations was, outside of aerial reconnaissance in US territories, the accepted operational practice. This typically followed the US NWS National Hurricane Research Project recommendations from the 1950s onwards (e.g. Schloemer (1954) to Schwerdt (1972)), and was the precursor to the present NOAA Hurricane Research Division activities in Miami. However the new Dvorak technique, undoubtedly due to its appeal in association with the evolving remote sensing technology and geostationary satellites, quickly supplanted the more laborious direct data-based methods.

As noted in Harper (2004a), at an American Meteorological Society Special Session in honour of the 30th anniversary of this remarkable tool, it also ironically stifled the routine collection of TC scale information from that date forward. The method did not specifically require such information and it led instead to focus attention on a single metric – the estimated peak surface wind speed (*Vmax*) – as the sole intensity measure, without specific regard to concepts of wind field scale or structure. Meanwhile, engineers continued to rely upon the use of physically-consistent, albeit simplified, analytical models of the wind structure (e.g. Harper 1980, Holland 1980, Harper and Holland 1999) or simplified boundary layer "slab" models (e.g. Vickery et al. 2000) so that full surface wind and pressure fields could be estimated for simulation purposes.

Building on the evidence collected from Harper (2002, 2004a), Harper and Callaghan (2006) vividly demonstrated the critical role of "increasing technology, methodology and skill" over the past 100 years in our ability to detect and describe TCs in Australia, and by implication, worldwide. This is summarised below in Figure 1 and is characterised by (top panel) the role of satellites in stabilising the detection of TCs and (bottom panel) the role of the Dvorak method in association with various satellite sensors (as well as ground-based data), and increasing analyst skill, in increasing the mean intensity of TC intensity estimates over time. The SOI trend is also indicated to show that this increasing trend in estimated intensity mainly occurred during a period of El Niño conditions unfavourable to TC development.

The Dvorak Method – The Good, the Bad and the Ugly

As chronicled by Harper (2002), the empirical Dvorak technique emerged during the late 1960s from a number of separate efforts to utilise the increasing amount of regular and good quality visible satellite imagery. The technique was developed by Vernon F. Dvorak of the Analysis Branch, US National Environmental Satellite Service of NOAA, based in Washington D.C. As a non-meteorologist, I was privileged to be a major author to the American Meteorological Society 30th anniversary

---

[1] The study targeted the reliable estimation of 10^{-4} p.a. exceedance levels for extreme wind and wave design criteria for offshore structures on Australia's North West Shelf.

[2] Holland (1980) is arguably the most widely-used parametric TC pressure and wind field model by coastal, ocean and wind engineers.

[3] The World Meteorological Organization International Workshop on Tropical Cyclones, held every four years since 1985 by invitation only. I have been fortunate to have attended the 1998, 2002, 2006 and 2010 IWTC as a significant contributor or rapporteur.

[4] Jeff Callaghan was the Head of the Severe Weather Section at the BoM Queensland Regional Office in Brisbane (retired in 2008 but still research-active).

Dvorak tribute paper, especially since Dvorak himself was not a trained meteorologist. Quoting from Velden et al. (2006):

> *"The Dvorak TC intensity estimation technique has been the primary method for monitoring tropical systems for more than three decades. The technique has likely saved tens of thousands of lives in regions where over one billion people are directly affected by TCs. The Dvorak technique's practical appeal and demonstrated skill in the face of tremendous complexity place it amongst the great meteorological innovations of our time. It is difficult to think of any other meteorological technique that has withstood the test of time and had the same life-saving impact."*

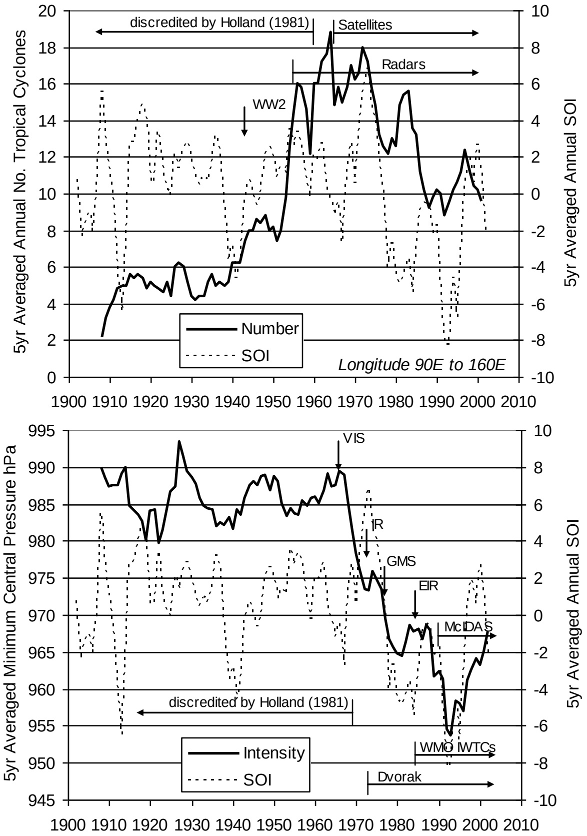


Figure 1 Potential influences on the Australian BoM TC database (from Harper and Callaghan 2006)

The Dvorak method was based on a conceptual model of tropical cyclone development and decay that initially used VIS satellite imagery to identify steps in the storm development. Figure 2 provides a useful schematic overview of the satellite image pattern recognition technique, albeit as it was further refined and described in Dvorak (1984) for IR and Enhanced IR imagery. The application of the method is designed to estimate the so-called T number (*tropical number*) that represents the increasing storm intensity on a scale of 1 through 8, with a resolution of 0.5. The T number is then further adjusted to a CI (*current intensity*) number to allow for inertial lags in the decaying phase of a storm, which is argued to hold the intensity higher while the cloud structure is breaking down (although more recent experience questions the role of this, e.g. Brown and Franklin 2002). The CI number is then converted into an estimate of *Vmax* via an empirical relationship, which is largely linear for storms of interest, and has not changed since its minor refinement in 1975.

The Dvorak method undoubtedly provides a substantial basis for forecasting the intensity of a TC in the open ocean environment and at the time of its introduction had an enormous beneficial effect. The later modification utilising EIR imagery further bolstered the method by using a more physically robust and objective temperature proxy. However, since then the method has remained unrefined and largely unquestioned, except for the work by Velden (1998) on the Objective Dvorak Method. The ODT promised to automate the manual processes and remove the scatter from subjective image interpretation and also reduce the operator skill and experience factor[5]. This attempt to automate the method has not been universally adopted nor deemed to have outperformed the manual approach but work has continued on its further refinement (e.g. Olander and Velden 2007).

To appreciate why an automated approach has not proven as "reliable" as might be expected, it is important to re-examine the basic assumptions of the Dvorak method. Critically the method ignores, explicitly at least, storm "structure" and forward speed. This refers to the links between size, wind and pressure gradients, asymmetry and "intensity". We now appreciate that TC intensification and decay can occur on much shorter timescales than was ever anticipated when only daily or even 6-hourly satellite images were available, and this is related to the complex dynamical evolution of the storm and its environment (e.g. ocean heat content, vertical shear, dry air intrusion, divergence aloft; boundary layer dynamics etc). Some of these phenomena are implicitly represented by the various patterns that are identified by the method (refer Figure 2 bottom) but the original tenet of "one T number" being the modal expected intensity change over 24 h is routinely shown to be unreliable and the constraint of "2.5 T" intensity increase over a day is often exceeded.

In the late 1990s the advent of polar-orbiting satellite microwave sensors (passive and active), altimeters and scatterometers increasingly documented the highly non-linear interactions that can occur in the vertical cloud structures and surface winds and waves to inform spatial structure changes (e.g. Appendix A in Harper et al. 2008). In recent years the role of "eyewall replacement cycles" in modulating storm size and intensity has also been recognised and, linked to the complex boundary layer physics, is a significant avenue for research. This is but one aspect unknown to the earliest Dvorak method development.

The Dvorak method enjoyed overwhelming support from all forecast agencies worldwide because of its simplicity. Ironically, one of its staunchest advocates has been the agency that, having regular aircraft reconnaissance, arguably least relies upon it – the US National Hurricane Center (NHC). Suspicious of the rigour of contemporary comparisons between Dvorak and more objective TC intensity estimates (e.g. Brown and Franklin 2002 – refer later), Harper (2002) contended that a closer examination of US "best track" intensities suggested likely cross-contamination of Dvorak estimates with other more objective data based on aircraft deployed dropwindsondes (e.g. Franklin et al. 2003). Outside of the near-continental US reconnaissance areas, this included the usage of statistical-persistence models that had been largely trained on Dvorak-conditioned datasets. There also appeared to be a tendency for NHC *Vmax* estimates to adopt the highest of

---

[5] The individual operator experience in encountering extreme (rare) weather events has long been a factor in forecast performance and lead to the formation of the BoM Severe Weather Sections in the 1990s to help concentrate and fast-track forecaster skills. With the increasing sensing and communication technology over the past 20 y the access to real time training opportunities has also massively increased, whereby armchair analyses can now be conducted globally with ease. In fact this access has increasingly allowed challenges of official regional forecast agency performance and now fuels the quest for improved understanding.

the several competing values[6], assisted by a procedure of always rounding up to the nearest 5 kt. Interestingly, Dvorak (1984) noted rather candidly that the best track datasets even at that time had already become biased by the application of the technique itself.

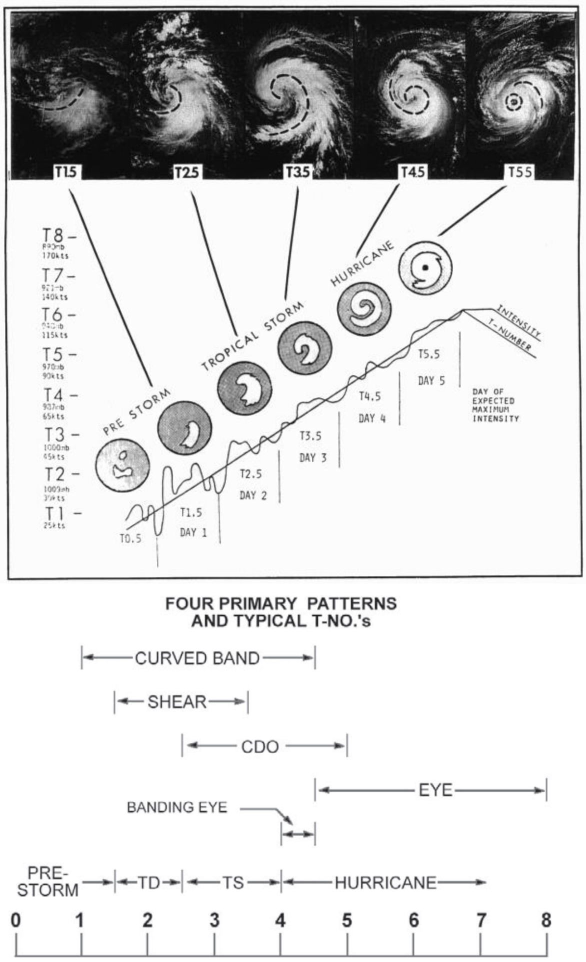


Figure 2 (top) Schematic of the Dvorak (1984) technique for the Atlantic basin (Dvorak and Smigielski 1990); (bottom) Primary Dvorak cloud patterns and the T values typically assigned (Velden et al. 2006)

Wind – Pressure Relationships (WPR)

The Dvorak method delivers an estimate of the “maximum surface wind” anywhere in the storm. Implicitly this means an earth-relative wind associated with the radius of maximum winds near the centre. To obtain an estimate of the MSLP the method provides a wind pressure relationship (WPR) and provided tabulations of this relationship to allow forecasters to make that estimate and hence make comparison with surface observations. By 1984, when the method was formalised with EIR imagery, two WPR were provided to suit the main US-centric needs: an “Atlantic” version and a “Western North Pacific” version (the latter WNP method serving a military context). The former was deemed suited more to “smaller systems” and the latter to “larger systems”, thus being a nod to the potential role of storm structure that was left unaddressed.

Much of the impetus for the Harper (2002) investigation for Woodside centred on the realisation that, over time the BoM had used a variety of WPR and the practices across the three Tropical Cyclone Warning Centres (TCWC are at Brisbane, Darwin and Perth) typically differed. This manifested most strongly in the region of crossover between the TCWC’s whereby a storm’s MSLP might often undergo a stepped change in the historical record. This inhomogeneity was most evident in the vicinity of the Timor Sea and clearly played havoc with statistical hazard analysis for Woodside and others. It was the exploration of this issue that lead further to a challenging of the popular practices of all TC forecasting agencies, a critique of the Dvorak method itself and a search for its essential calibration and verification datasets upon which to reassess the overall accuracy.

This investigation led to some surprising conclusions and uncovered dogma that had long remained untested, *viz*

- There was no recoverable dataset that fully documented the development of the Dvorak method;
- The original 1972 method was based entirely on WNP storms and evolved over a period of time;
- Initially the CI concept had correlated better with MSLP than *Vmax* estimates but *Vmax* was preferred operationally for warnings and the WPR was relegated as the secondary step;
- As the satellite data increased, differences between WNP and Atlantic storms were recognised;
- Contemporaneously from 1975, an alternative WPR was developed for the WNP region, published by Atkinson and Holliday (1977), hereafter A&H;
- The final 1984 Dvorak method then adopted the A&H WPR for the WNP region in preference to the original WPR (which may have been influenced in any case by Kraft 1961);
- The original 1972 Dvorak WPR then became the “Atlantic”, which was contrary to popular knowledge of US forecasters staunchly defending its regional applicability.

None of this detracts from the ingenuity of the original Dvorak method, nor should it unduly criticise the efforts of those involved in any of these developments. However, support for developing more rigorous and transparent methods from both agencies and academics languished for more than 20 y, leading to the gradual accumulation of highly inhomogeneous global historical datasets. This simple fact would then play a central role in the pivotal 2005 debate over the influence of climate change on TCs (refer later).

Unlike the original Dvorak (1972) “Atlantic” WPR, A&H offered a more recoverable dataset that enabled critique and even reanalysis at least of their recommended WPR relationship. Ostensibly A&H was a very comprehensive assessment of groundtruth, assembling the best available MSLP and *Vmax* close-encounter datasets from Hong Kong, Taiwan, Guam and scattered island sites in-between, from 1947 to 1974. Adjustments were made for observation height (1/6 power law) and stratified for exposure to some extent, resulting in a base dataset of 76 TC events. After a quadratic best fit, the resulting WPR gave a 12% reduction in *Vmax* relative to the original Dvorak WPR relationship for a given MSLP (refer Figure 3, with winds adjusted to 10 min standard and the MSLP adjusted to a pressure deficit as per Harper 2002).

Notwithstanding the care taken in its development, A&H stopped short of considering the differing anemometer responses and

---

[6] This claim was naturally contested by my NHC colleagues, fresh from countering Sparks (2003), but it has been admitted that cross-contamination of objective and subjective data is always a possibility. Additionally, the dilution of otherwise useful data by the NHC practice of providing only 6 hourly summary track fixes prompted me to provocatively substitute the term “worst tracks” each time I had the opportunity to question such procedures with colleagues.

ignored (likely significant) topographic site effects. Critically it also relied on “peak gusts” from chart-recording anemometers but adjusted these to the so-called “1 min” sustained wind speed which was favoured by the US military at this time. Dvorak (1984), in recommending use of A&H for the WNP region, also seemingly associated the “Atlantic” WPR with a “1 min” sustained wind speed. Henceforward this perpetuated the application of wind-averaging adjustments to *Vmax* values obtained by the Dvorak method for non-US regions that had adopted the agreed WMO “10 min” standard (refer later).

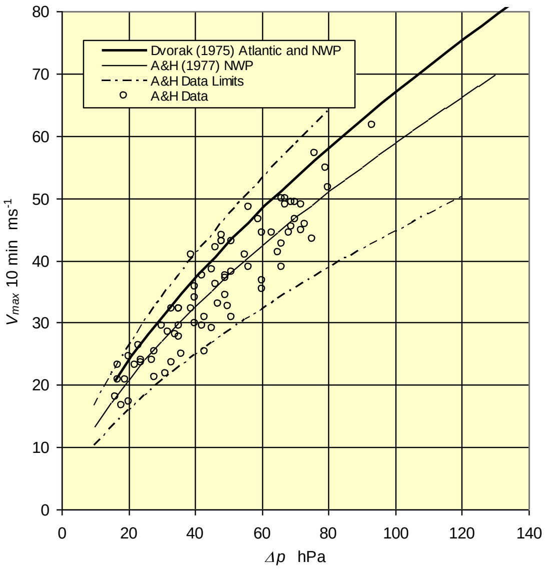


Figure 3 A comparison of the Dvorak (1975) and A&H (1977) data and relationships (from Harper 2002)

As a footnote to A&H, prompted by Harper (2002), Knaff and Zehr (2007) re-examined some 15 y of contemporary wind-pressure data from a variety of sources and also reconsidered the A&H fitting procedure. After arguing that the A&H dataset should have been binned before fitting to reduce bias, they devised a revised formula that tended to increase *Vmax* for the more intense storms. Also, they noted similar work (in Japanese) from Koba et al. (1990) that developed a similar relationship in regular use by the Japanese Meteorological Agency (JMA).

In the Australian context, the WPR issue was exacerbated by the development of a third option in 1985 that was conceived and implemented locally by the Darwin TCWC, predicated on the real experience from a number of small yet intense storms, with the devastating experience of TC *Tracy* (1974) being a principal player. This storm (e.g. Harper 2010) presented with an accurately measured and modest MSLP of 950 hPa yet had a peak *Vmax* gust recorded (by a Dines anemometer) of the order of 60 ms^{-1}, making it an outlier for either of the Dvorak WPRs. This was the first instance of what has become classified as “midget” TC behaviour in wind-pressure space. However, in hindsight the Darwin TCWC method lacked substantive justification, yet remained in BoM operational usage until circa 2010 (Knaff and Harper 2010).

Subsequently a new BoM-supported WPR (Courtney and Knaff, 2009) has been adopted to provide standardisation across TCWCs and to incorporate the range of observed behaviours worldwide. This is a modified form of the Knaff and Zehr (2007) relationship that better suits forecaster needs and provides a more generalised framework than the original, which was US-centric and relied somewhat on prognostic numerical model fields. Prior to this, historical TC MSLP estimates in the Australian region could have been based on either A&H by Perth, the “midget” Darwin TCWC method, or a combination of both from Brisbane.

Harper et al. (2008) describes efforts circa 2002 to reconstruct a more homogeneous TC dataset of the original Dvorak T and CI estimates that had not been retained over time for the North West Shelf region. This permits exploration of WPR sensitivities in a wind hazard context. Collectively, this work has led to the BoM and other agencies now routinely recording all forecast metrics in a more transparent manner to facilitate such analyses.

Wind Averaging - What Wind Averaging?

The documentation surrounding the development of the Dvorak technique at no stage discussed whether the estimated surface wind speed was a mean wind speed or a peak gust wind speed – it was simply the “maximum surface wind” or MSW. As noted earlier, the advent of A&H’s preferred use of a “1 min sustained” wind speed was then simply adopted as the metric. With the possible dynamic range of wind gusts versus mean winds of the order of 20-40%, this represented a potentially large source of variance in the science of TC intensity estimation that was worthy of further investigation.

After Dvorak (1984), the conventional approach for non-US regions was to apply the WMO-recommended conversion from a Dvorak “1 min” wind to a “10 min” wind, being a factor of 0.88 (WMO 1993). Notwithstanding this accepted practice, the difference between various wind-averaging standards (aka *gust factors*) was a source of confusion amongst agencies and prevented the ready exchange and interpretation of data[7]. In 2002, a WMO committee recommended that this issue be investigated for resolution and in 2003, due to the emerging impact of Harper (2002), Systems Engineering Australia Pty Ltd was asked to address this task. This seemingly straightforward investigation would ultimately take 7 years to complete the round of inter-agency comment and external peer review, with the final WMO report and recommendations delivered as Harper et al. (2010). Why did it take so long and what has been its import and impact to date?

The review considered a wide range of issues, data and publications in the meteorological and wind engineering literature, including whether there was any fundamental difference between TC and non-TC wind regimes. It concluded that the latter was reasonably unlikely, but the existing WMO Regional Association (RA) plan definitions of “mean” winds, “sustained” winds and wind “gusts” lead to uncertainty and ambiguity with regard to how specific metrics of the wind can be inter-compared. This reflected a lack of rigour generally in describing near-surface winds within the forecasting environment that can lead to misinterpretation and result in unintentional biases (high and low) of forecast winds (and by implication “best track” winds used for wind hazard studies). In particular, there was a tendency to misuse the term “mean” wind amongst the tropical cyclone community where the “maximum 1-min sustained” wind is involved. Also, it is noted that only one RA plan defined the “surface” wind height as being 10 m and no plans explicitly stated the wind exposure[8], although the typical

---

[7] As documented in the various WMO Regional Association reports and procedures, both 2-min and 3-min winds were in use in addition to the US 1-min and the WMO 10-min “standard”.

[8] Notwithstanding this, it should be recognised that the work by Powell et al. (1996) after Hurricane *Andrew* in 1992 has been influential in introducing forecasters to the need to generally adjust for exposure.

context is “at sea”.

The wind averaging review specifically highlighted the need to distinguish clearly between randomly sampled estimates of the mean wind speed based on any chosen averaging period and the peak gust wind speed of a given duration within a particular observation period. It particularly noted that *mean* wind speed estimates could not be converted between different averaging periods using gust factors – only *gust* wind speed estimates could be converted. In particular, the principal widespread misuse of the ambiguous US-centric term “maximum 1-min sustained wind” failed to note the need to specify an observation period and that “sustained” actually meant a “peak 1-min gust”.

After consideration of a range of potential theoretical frameworks ESDU (2002) was adopted in a modified and simplified form, driven by *apriori* turbulence intensity applicable to typical TC forecasting exposure classes (Table 1).

Exposure Class	Turbulence Intensity I_u	Roughness Length z_o (m)
“in-land”	0.250	0.18
“off-land”	0.200	0.07
“off-sea”	0.150	0.013
“at-sea”	0.100	0.0005

Table 1 Recommended turbulence intensities and associated roughness lengths for tropical cyclone forecasting purposes. (after Harper et al. 2010).

Associated with this, a new nomenclature was introduced to forecasters to hopefully avoid misinterpretation in the future, such that an estimate of the *true mean wind V* should be explicitly identified by its averaging period *To* in seconds, described as V_{To} , e.g.

V_{600}	is a 10-min averaged mean wind estimate;
V_{60}	is a 1-min averaged mean wind estimate;
V_{3}	is a 3-sec averaged mean wind estimate.

Likewise, a gust wind is additionally prefixed by the gust averaging period τ and described as $V_{\tau,To}$, e.g.

$V_{60,600}$	is the highest 1-min mean (gust) within a 10-min observation period;
$V_{3,60}$	is the highest 3-sec mean (gust) within a 1-min observation period.

The *gust factor* $G_{\tau,To}$ then relates to the mean and the gust as $V_{\tau,To} = G_{\tau,To}\ V$.; where the true mean wind *V* is estimated on the basis of a suitable sample, e.g. V_{600} or V_{3600}. Questions as to what represents a *suitable sample* unavoidably must consider the issue of stationarity of the wind field and the study specifically examined wind records from the eye passage of TC *Orson* in 1989 provided by Woodside, to illustrate these very issues. Figure 4 below shows the matching obtained between the adopted ESDU formulation and the available wind data sets known to be from TC conditions. Differences between the newly recommended wind averaging conversion factors and those previously used were significant in a number of ways. Firstly, the new analysis considered a wider range of averaging periods and exposures, focusing on cases of specific concern for TC forecasting. Secondly, the magnitudes of the recommended conversion factors are different from those used previously. Also, converting between agency estimates of storm-wide maximum wind speed (*Vmax*) was seen to require special considerations because it represents both a time and space context and is also a function of the exposure. Accordingly, the review recommended an “at-sea” exposure[9] conversion of 0.93 between the US “maximum 1-min sustained” estimate of peak storm intensity *Vmax* and the WMO standard 10-min average wind speed estimate, rather than the previous value of 0.88, which was shown to be associated more with a rougher “off-land” exposure. This implies that current non-US practice has underestimated the “at-sea” 10-min average *Vmax* obtained by Dvorak (1984) by about 5%, relative to an equivalent 1-min value. However, the review noted that the Dvorak-related intensity estimation techniques should be re-calibrated based on a more rigorous and consistent treatment of wind-averaging issues. For example, A&H ostensibly converted chart recorder peak gusts (of specifically unknown averaging period) into “1-min sustained” wind speeds using a method developed at the time for military use. Further inspection of this revealed that the method actually aimed to deliver an estimate of the peak 1-min gust over a 5-min observation period.

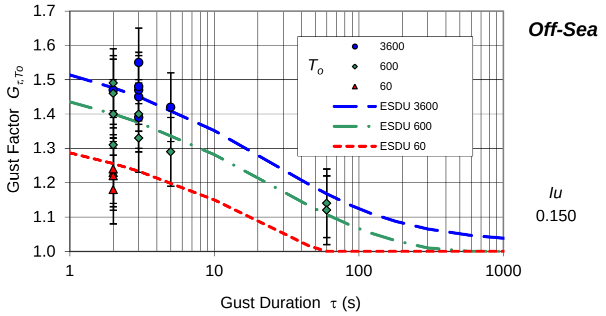


Figure 4 Calibration of the modified ESDU method for tropical cyclone forecasting purposes for “Off Sea” exposure (from Harper et al. 2010).

As a result of widespread dissemination of the recommendations from Harper et al. (2010), presentations to WMO regional committees over many years and a special keynote at the IWTC-VII (Knaff and Harper 2010), three of the five WMO RA plans now contain a prominent summary of the recommendations but none have made significant changes to their wind terminology, and this will likely not assist in improving understanding. While BoM has incorporated the new advice into its operational procedures through adoption of the Courtney and Knaff (2009) WPR, the applicable 2012 WMO RA plan remains unchanged and does not include the report’s summary.

The Saffir-Simpson Scale – The Plot Thickens

In the mid-1970s the Director of the NHC Bob Simpson (Simpson 1974), a meteorologist, and Sam Saffir, a civil engineer, were influential in proposing a “hurricane disaster-potential scale” for the USA that extended the Beaufort scale ranges and included storm surge potential useful for emergency management purposes. The operational reliance on and deemed significance of this scale in the US context belies the fact that its origins are shrouded in mystery.

While Saffir (1975) clearly labelled the adopted index wind speeds as “2 or 3 s gusts”, there is no similar confirmation by Simpson (1974) as to the applicable averaging period. Subsequently, it appears that an arbitrary decision was later made to associate the Saffir-Simpson Scale wind speed ranges with the “maximum 1-min sustained” wind, as popularised at the time by A&H. However the description of the level of damage for the stated wind speed remained unchanged (i.e. more applicable to a peak gust). Potential consequences of this sequence of assumptions were raised by Sparks (2003), frustrated by what

---

[9] This acknowledges the new appreciation of open sea boundary layer structure revealed by dropwindsondes as, for example, reported by Powell et al. (2003) and found to be consistent with Woodside wind data.

appeared to be consistent overestimation of forecast and best track wind speeds by the NHC.

Harper (2002) suggested that Simpson may have based the scale around the much earlier Kraft (1961) WPR. Evidence for the potential biasing effect of this shows in the Brown and Franklin (2002) analysis (Figure 5) that summarises how exceedingly well the circa-1961 Kraft WPR seems to fit the mean value of contemporary "objective data" ostensibly much influenced by aircraft reconnaissance. Likewise the Dvorak "Atlantic" WPR, conceived in 1972 using scant WNP data, likewise is a very good fit to the modern "data" mean, and A&H is clearly not a good fit (as it applies to the WNP?).

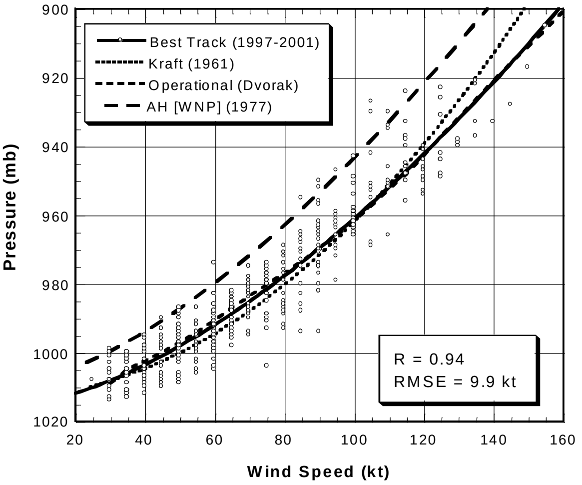


Figure 5 Pressure – wind relationships based on best track data in the Atlantic from Brown and Franklin (2002); 1 min winds shown.

Ideally, an arbitrary wind scale should not be allowed to contaminate objective data. However it seems that, like the historical tendency for accumulated data to follow the Dvorak relationship, the operational pressure of the Saffir-Simpson scale in the US has also possibly exerted influence on best track outcomes.

In closing this topic, it can be mentioned that the Saffir-Simpson Scale was modified in 2009 to separate wind damage potential from storm surge damage potential (the latter being finally recognised as not well correlated to the simple concept of a *Vmax*). In doing so, the NHC sought advice from the wind engineering community (Masters et al. 2009) and implemented various changes in the wind damage texts that better aligned the description of damage using the "1-min sustained" wind rather than the original Saffir (1975) peak gust wind scale.

Climate Change Confusion and Contusion

Contrary to the IPCC SAR projections at the time, a 1998 WMO-led consensus amongst the TC research community agreed that future $2\times CO_2$ climates were unlikely to dramatically change the distribution of TC occurrence by 2100 but might conceivably lead to a 10 to 20% increase in peak intensity. This WMO-endorsed statement (Henderson-Sellers et al. 1998) countered a simplistic argument at the time for the extension of the 26° isotherm sea surface temperature "trigger" in a 2° warmed world. However, there was significant uncertainty as to exactly what the future might bring, given the lack of knowledge at the time regarding TC behaviour in general and the significant limitations of global models, but set against the unequivocal increase in exposure of vulnerable coastal assets.

This consensus at least brought together the competing Maximum Potential Intensity (MPI) theorists Emanuel (1988) and Holland (1997); the former retreating from earlier assertions of possible future climate "hypercanes" and the latter urging caution on the basis of likely strong atmospheric feedbacks. The 1998 statement was also supported by the more observationally-oriented William Gray[10] (CSU) and Christopher Landsea (NHC). In the face of continuing TC alarmism from the IPCC TAR that followed, this statement was an important reference for those of us providing advice for long term planning and engineering design (e.g. Harper (2004b) for Engineers Australia).

However, in 2005, a series of remarkable events acted to split the earlier consensus and the TC community as a whole. Firstly, in January of that year, Landsea resigned from his role of TC-contributing author on the IPCC AR4 project, citing in a public letter that he could not "*continue to contribute to a process that I view as both being motivated by pre-conceived agendas and being scientifically unsound*". This referred to the allegation that an IPCC Lead Author was actively promulgating unsubstantiated TC-alarmist commentary to influential committees, including the US Senate. Next, in August 2005, just before Hurricane *Katrina* devastated New Orleans and the Mississippi coast, Emanuel (2005) presented an impressive correlation between an analysis of WNP "best track" TC intensities over the past 30 y and increasing sea surface temperature data, noting a significant and unexpected doubling of a derived intensity (power dissipation) index. Next, immediately following *Katrina*, Webster et al. (2005) was published , citing a similar assessment of "best track" data globally, showing significant increases over the past 30 y in the number of "Cat 4 and 5" TC events. The 2005 hurricane season then continued unabated, reaching a record number[11] of 28 named systems, 15 of which were hurricanes and 3 Cat 5 crossed the US coastline. This set the scene, fuelled by media and the blogosphere, for significant public concern and a vigorous debate amongst the TC research community – noting that Emanuel, Holland and Webster had all been party to the 1998 statement and the ostensibly identified trends were well above those supported by any rigorous scientific assessment of climate change impacts.

The American Meteorological Society's 27th Hurricanes and Tropical Meteorology Conference at Monterey in April 2006 was destined to be a contentious affair as a result of media interest and the clear division between some in the TC research community and the operational communities. A panel discussion was held between the opposing camps, with Webster and Emanuel on the one side and Landsea and Chan on the other (Gray was contentiously excluded) but did not lead to any resolution[12]. Harper and Callaghan (2006) delivered at that conference was one of many responses from that part of the community who were well aware of the inaccuracy of historical TC data, with some attendees even willing to claim personal responsibility for that over their forecasting career. As later noted in the contemporary non-fiction work *Storm World* (Mooney 2008) I was credited with coining the term "primate change" to explain the likely reasons why the historical data might show an increasing trend in TC intensity over time. It was at that meeting that colleagues Kossin and Knapp discussed with me the

---

[10] Prof Bill Gray at Colorado State University has been an influential figure in TC science and in developing seasonal forecasting techniques in the US. He has also long been a strident opponent to simplistic climate change projections. Although I do not share nor understand all of Bill's arguments, we formed a common collegiate bond and friendship in recent years on the subject of data reliability. Many of Bill's former students have shared his essential position (e.g. Landsea and Chan) but others have not (Holland) and another (McBride) has even acted as a go-between.

[11] Thankfully following seasons returned to more average conditions.

possibility of helping settle the argument through the use of what was a recently assembled homogenised and objective global satellite IR image dataset. Meanwhile I was privileged to be a supporting author to Landsea et al. (2006) that was published in *Science* in July, arguing the case against reliable TC intensity trend detection using historical data.

The postscript to this event was the WMO 2006 IWTC VI, held in Costa Rica in November, which I attended. Preliminary results from Kossin et al. (2007) were available that, although limited to a 22 y period, showed no globally increasing trend in intensity when using fully objective data[13]. Notwithstanding some heated debate between the various protagonists, this helped produce a consensus statement (WMO 2006) that was framed by all 125 delegates.

This tumultuous year at least ended with a WMO-sponsored plan to reunite the opposing parties, leading to a formal published expert consensus being prepared as Knutson et al. (2010) to update the original 1998 statement. Interestingly, the advice was was not too different to that in 1998, except for the growing global model consensus that there may be a considerable reduction of global TC occurrence by 2100! This latest advice was able to be incorporated into Harper (2012) for Engineers Australia.

The postscript to this period of unrest for me was the publishing of the outcomes of the earlier Woodside TC database review (Harper et al. 2008). This formalised the examination of the disparate agency practices that had been noted in Harper (2002), the significant changes in remote sensing, knowledge and skill and as an example of likely global implications, reported on the consistent review of some 183 TCs in the North West Shelf region from 1969/70 to 2000/01. The results were summarised in terms of equivalent Saffir-Simpson category scales to enable direct comparison with Webster et al. (2005). Figure 6 condenses the outcome into a simple histogram of category changes that occurred as a result of the review.

The left-most column of Figure 6 summarises the overall percentages of storms having an increase or a decrease in their original BoM intensity category. The next four columns document the shifts in categories by one Saffir-Simpson number, two, three or four respectively. The right-most four columns document the changes between specific categories, where “0” indicates “tropical storm” on the S-S scale, or essentially a Category 1 Australian TC[14]. It can be seen that, as a result of the review, there was a significant increase in estimated TC intensity in this region across a wide range of events (net of 21.3% of storms increased), mostly by one Category (net of 19.7%). Many of these are at the higher end of the range, whereby Category 3+ changes represent almost 11% of those storms having been assigned increased intensities.

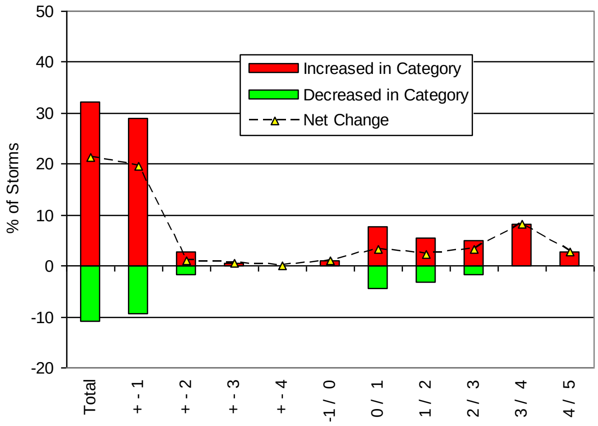
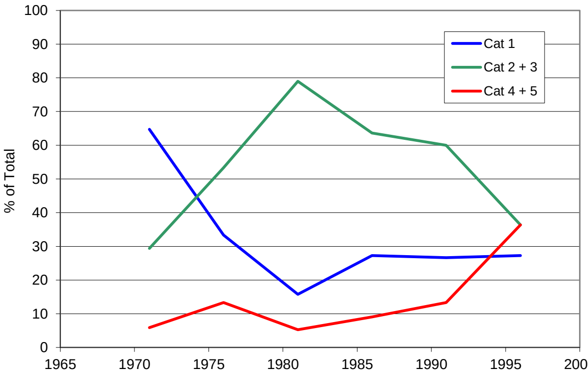
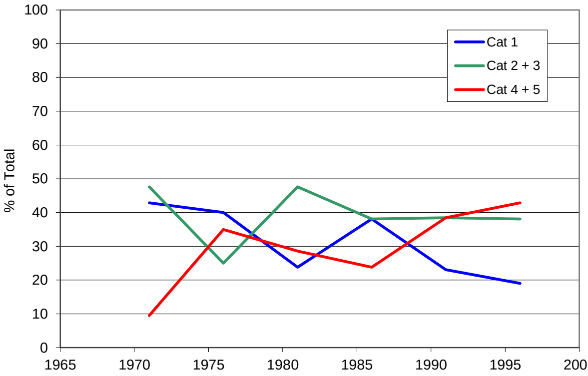


Figure 6 Summary changes in US Saffir-Simpson category between the original and reviewed North West Shelf TC datasets (from Harper et al. 2008).

The results were also presented in a pentadal Category-grouped manner similar to Webster et al. (2005), which had highlighted the appearance of significant trends in intensity over time. Figure 7 illustrates the effect of the review in the North West Shelf region on inferred trends using this classification and grouping of intensity estimates. Whereas the original data suggested a reduction in weak and moderate storms over time and an increase in the more severe classes, identified in Webster et al,, the reviewed dataset essentially removed these trends except for the earliest pentad that suffers significant satellite data quality issues related to frequency and nadir.



(a) Trends derived from the original BoM dataset



(b) Trends derived from the reviewed dataset

Figure 7 A comparison of trends in grouped intensity classes inferred from the original and reviewed North West Shelf datasets (after Harper et al. 2008).

---

[13] I thank Jim Kossin for including me on this landmark paper in recognition I think for my efforts in opening up the issue and trying to broker consensus within the TC community. Not to misrepresent Jim’s further work, a later analysis suggested that some intensity trends were detectable using a more sensitive statistical method. Notwithstanding this, Jim was a party to Knutson et al. (2010).

[14] It should be noted that the Australian TC Category scale does not match the Saffir-Simpson Scale but was originally based around Dvorak T value divisions. Notwithstanding this, the scales differ at the hurricane force wind transition in any case because of the US practice of using the “1-min sustained wind” (a gust) rather than the WMO standard “10-min average”.

Conclusions

There is much more that can be said on this subject that has eluded the present space limitations. However the foregoing will hopefully inform those working in this field to be wary of the intrinsic value of much historical TC data and remain circumspect when developing modelling constructs that rely on ostensibly objective data. In particular I trust it will dissuade those intent on adopting "best track" data to support simplistic wind risk assessments (e.g. as critiqued by Harper et al. 2012).

Acknowledgements

The work described here was achieved with significant input and encouragement from a number of BoM personnel and also international colleagues from WMO, NOAA and university researchers worldwide. Many of these persons have already been mentioned here but there are many others acknowledged in the publications listed. I also wish to recognise the valuable contribution by the late Mike McCormack in steering the North West Shelf data review and the support of Woodside Energy Ltd over the past 20 years.

References

Atkinson G.D. and Holliday C.R. (1977) Tropical cyclone minimum sea level pressure/maximum sustained wind relationship for the Western North Pacific. Mon. Wea. Rev., 105, 421-427.

Brown D.P. and Franklin J.L. (2002) Accuracy of pressure-wind relationships and Dvorak satellite intensity estimates for tropical cyclones determined from recent reconnaissance-based "best track" data. Proc. 25th Conf Hurricanes and Tropical Meteorology, AMS, San Diego, 458-459.

Courtney J. and Knaff J.A. (2009) Adapting the Knaff and Zehr wind-pressure relationship for operational use in Tropical Cyclone Warning Centres. Australian Meteor. and Oceanographic J., 58, 167-179.

Dvorak V.F. (1975) Tropical cyclone intensity analysis and forecasting from satellite imagery. Mon. Wea. Rev., 103, 420-430.

Dvorak V.F. (1984) Tropical cyclone intensity analysis using satellite data. NOAA Technical Report NESDIS 11, 45pp.

Dvorak V.F. and Smigielski F. (1990) Tropical clouds and cloud systems observed in satellite imagery. NOAA Technical Report NESDIS. (updated to 1995).

Emanuel K.A. (1988) The maximum intensity of hurricanes. Journal of Atmospheric Science, 45, 1143-1155.

Emanuel K.A. (2005) Increasing destructiveness of tropical cyclones over the past 30 years. Nature, 436, 686-688.

ESDU (2002): Strong winds in the atmospheric boundary layer. II: Discrete gust speeds. Engineering Science Data Unit, Item No. 83045, London.

Franklin J.L., Black M.L. and Valde K. (2003) GPS dropwindsonde wind profiles in hurricanes and their operational implications. Weather & Forecasting, 18, 1, 32-44.

Harper B.A. (1980) Numerical simulation of tropical cyclone storm surge, PhD Thesis, James Cook University, Queensland, Australia, September 1980. (also as Sobey et al., 1977, Dept Civil and Systems Engin, Research Bulletin CS14, May.)

Harper B.A. (1996a) Risk modelling of cyclone losses, Proc. IEAust Annual Engin Conf., Institution of Engineers Australia, Darwin, April, 53-88.

Harper B.A. (1996b) The application of numerical modelling in natural disaster risk management, Proc. Conf. on Natural Disaster Reduction NDR'96, Institution of Engineers Australia, Gold Coast, Sept, 107-114.

Harper B.A. (1998) Parametric wind fields. In: Topic Chairman and Rapporteur Reports of the Fourth International Workshop on Tropical Cyclones, World Meteorological Organisation, Tropical Meteorology Research Program (TMRP) and Commission for Atmospheric Sciences (CAS), Report No. 59, Haikou, China, April 21-30, WMO/TD No. 875.

Harper B.A (1999) Numerical modelling of extreme tropical cyclone winds APSWE Special Edition, Journal of Wind Engineering and Industrial Aerodynamics, 83, 35 - 47.

Harper B.A. (ed.) (2001) Queensland climate change and community vulnerability to tropical cyclones - ocean hazards assessment - stage 1, Report prep by Systems Engineering Australia Pty Ltd in association with James Cook University Marine Modelling Unit, Queensland Government, March, 375pp.

Harper B.A. (2002) Tropical cyclone parameter estimation in the Australian region: Wind-pressure relationships and related issues for engineering planning and design. Systems Engineering Australia Pty Ltd for Woodside Energy Ltd, Perth, May, 83pp. http://www.systemsengineeringaustralia.com.au/download/Wind-Pressure%20Discussion%20Paper%20Rev%20E.pdf]

Harper B.A. (2004a) Globalisation, calibration and opportunities for enhancement of the Dvorak technique, 30 yr of the Dvorak Technique Special Session, American Meteorological Society, 26th Conference on Hurricanes and Tropical Meteorology, Miami, May, 8A.1, 218-219.

Harper B.A. (2004b) Guidelines for responding to the effects of climate change in coastal and ocean engineering – 2004 Update. Engineers Australia, National Committee on Coastal and Ocean Engineering, EA Books, 76pp.

Harper B.A. (2010) Modelling the *Tracy* Storm Surge - Implications for Storm Structure and Intensity Estimation. Proc. Cyclone *Tracy* Special Session, AMOS Conference 2008, Australian Meteorological and Oceanographic Journal, 60, 187-197, Sept. http://www.bom.gov.au/amoj/docs/2010/harper.pdf

Harper B.A. (2012) Guidelines for responding to the effects of climate change in coastal and ocean engineering – 3rd Edition 2012. Engineers Australia, National Committee on Coastal and Ocean Engineering, EA Books, May, 74pp. http://www.engineersaustralia.org.au/sites/default/files/shado/Learned%20Groups/National%20Committees%20and%20Panels/Coastal%20and%20Ocean%20Engineering/vol_1_web.pdf

Harper B.A. and Callaghan J. (2006) On the importance of reviewing historical tropical cyclone intensities. American Meteorological Society, 27th Conference on Hurricanes and Tropical Meteorology, 2C.1, Monterey, Apr.

Harper B.A. and Holland G.J. (1999) An updated parametric model of the tropical cyclone. Proc. 23rd Conf. Hurricanes and Tropical Meteorology, American Meteorological Society, Dallas, Texas, 10-15 Jan.

Harper B.A., Holmes J.D., Kepert J.D., Mason L.M. and Vickery P.J. (2012) Comments on "Estimation of Tropical Cyclone Wind Hazard for Darwin: Comparison with Two Other Locations and the Australian Wind-Loading Code". J. Applied Meteorology and Climatology, 51, 161-171, Jan.

Harper B.A., Kepert J.D. and Ginger J.D. (2010) Guidelines for converting between various wind averaging periods in tropical cyclone conditions. World Meteorological Organization, TCP Sub-Project, WMO/TD-No. 1555, August, 62pp. http://www.wmo.int/pages/prog/www/tcp/documents/WMO_TD_1555_en.pdf

Harper, B.A., Lovell, K.F., Chandler, B.D. and Todd D.J. (1989) The derivation of environmental design criteria for Goodwyn 'A' platform, Proc. 9th Aust Conf. Coastal and Ocean Engin., Institution of Engineers Australia, Adelaide, Dec.

Harper B.A., Mason L.B. and Bode L. (1993) Tropical cyclone *Orson* - a severe test for modelling, Proc. 11th Australian Conf. on Coastal and Ocean Engin., Institution of Engineers Australia, Townsville, Aug, 59-64.

Harper B.A., Stroud S.A., McCormack M. and West S. (2008) A review of historical tropical cyclone intensity in north-western Australia and implications for climate change trend analysis. Australian Meteorological Magazine, Vol. 57, No. 2, June, 121-141. http://www.bom.gov.au/amoj/docs/2008/harper.pdf

Henderson-Sellers A., Zhang H., Bertz G., Emanuel K., Gray W., Landsea C., Holland G., Lighthill J., Shieh S-L., Webster P. and McGuffie K. (1998) Tropical cyclones and global climate change: a post-IPCC assessment. Bull. of the Amer Met. Soc., 79, 1, Jan, 19-38.

Holland G.J. (1980) An analytic model of the wind and pressure profiles in hurricanes. Mon. Wea. Rev., 108, 1212-1218.

Holland G.J. (1981) On the quality of the Australian tropical cyclone data base. Aust. Met. Mag., 29, 169 181.

Holland G.J. (1997) The maximum potential intensity of tropical cyclones. J. Atmos. Sci., 54, Nov, 2519-2541.

Knaff J.A. and M. Zehr (2007) Reexamination of tropical cyclone wind-pressure relationships. Weather and Forecasting, 22, 71-88, Feb.

Knaff J.A. and Harper B.A. (2010) Tropical cyclone surface wind structure and wind-pressure relationships. In: 7th International Workshop on Tropical Cyclones, World Meteorological Organisation, World Weather Research Program, WWRP 2011-1, Saint-Gilles-Les-Bains, La Réunion, France, 15 - 20 November 2010, WMO/TD No. 1561. http://www.wmo.int/pages/prog/arep/wwrp/new/documents/final_WWRP_2011_1_TD_No_1561.pdf

Knutson T.R., McBride J.L., Chan J., Emanuel K., Holland G., Landsea C., Held I., Kossin J.P., Srivastava A.K. and Sugi M., (2010) Tropical cyclones and climate change. Nature Geoscience, 3, 15 –163.

Koba H., Hagiwara T., Asano S. and Akashi S. (1990) Relationships between CI number from Dvorak’s technique and minimum sea level pressure or maximum wind speed of tropical cyclone (in Japanese). J. Meteor. Res., 42, 59–67.

Kraft R.H. (1961) The hurricane’s central pressure and highest wind. Mariners Weather Log, 5, 155.

Kossin J.P., Knapp K.R., Vimont D.J., Murnane R.J. and Harper B.A. (2007) A globally consistent reanalysis of hurricane variability and trends. Geophys. Res. Lett., 34, Feb 28, L04815, doi:10.1029/2006GL028836.

Landsea C.W., Harper B.A., Hoarau K. and Knaff J. (2006) Can we detect trends in extreme tropical cyclones? Science, 313, 28 July, (10.1126/science.1128448).

Masters F., Vickery P., Harper B., Powell M., and Reinhold T. (2009) Engineering Guidance Regarding Wind-Caused Damage Descriptors. Provided to the NOAA Saffir-Simpson Hurricane Wind Scale Review Committee. Dec, 10pp.

Mooney C. (2008) Storm world - hurricanes, politics, and the battle over global warming. Harcourt Books.

Olander L. and Velden C.S. (2007) The advanced Dvorak technique: continued development of an objective scheme to estimate tropical cyclone intensity using geostationary infrared satellite imagery. Weather and Forecasting, 22, April, 287-298.

Powell M.D., Houston S.H. and Reinhold T.A. (1996) Hurricane Andrew’s landfall in South Florida. Part I: Standardizing measurements for determination of surface wind fields. Weather and Forecasting, 11, 3, Sept, 304-328.

Powell M.D., Vickery P.J. and Reinhold T.A. (2003) Reduced drag coefficient for high wind speeds in tropical cyclones. Nature, 422, March, 279-283.

Saffir H. (1975) Low cost construction resistant to earthquakes and hurricanes. Part Two. Hurricanes, ST/ESA/23, Dept. of Economic and Social Affairs, United Nations NY, 149-170.

Schloemer R.W. (1954) Analysis and synthesis of hurricane wind patterns over Lake Okeechobee, Florida. Hydromet. Rep., 31, Dept of Commerce, Washington D.C., March, 1954, 49pp.

Schwerdt R.W. (1972) Revised Standard Project Hurricane criteria for the Atlantic and Gulf coasts of the United States. Natl. Wea. Serv., Silver Spring, Md., Memo. HUR 7-120.

Simpson R.H. (attributed) (1974) The hurricane disaster potential scale. Weatherwise, 27, pp. 169 and 186.

Sparks P.R. (2003) Exaggeration of wind conditions in hurricane Floyd and other tropical cyclones. Natural Hazards Review, 4 (1), ASCE, Feb, 27-35.

Velden C., Harper B., Wells F., Beven J.L., Zehr R., Olander T., Mayfield M., Guard C., Lander M., Edson R., Avila L., Burton A., Turk M., Kikuchi A., Christian A., Caroff P., McCrone P. (2006) The Dvorak tropical cyclone intensity estimation technique: a satellite-based method that has endured for over 30 years. Bulletin American Meteorological Society, Vol 87, Sept, 1195-1210.

Vickery P.J., Skerlj P.F., Steckley A.C and Twisdale L.A. (2000) Hurricane wind field model for use in hurricane simulations. J. Structural Engineering, ASCE, 126,10, Oct, 1203-1221.

Walker G.R. (2013) A proposal for the review of design wind speeds in tropical cyclone prone areas of Australia. Proc. 16th Australasian Wind Engineering Society Workshop, Brisbane, July.

Webster P.J., Holland G.J., Curry J.A. and Chang H-R (2005): Changes in tropical cyclone number, duration, and intensity in a warming environment. Science, 309, 1844-1846.

WMO (1993) Global guide to tropical cyclone forecasting. Tropical Cyclone Programme Report No. TCP-31, World Meteorological Organization, WMO/TD – No. 560, Geneva.

WMO (2006) Statement on tropical cyclones and climate change. Proceedings WMO 6th International Workshop on Tropical Cyclones, IWTC-VI, San Jose, Costa Rica, November, World Meteorological Organization, Tropical Meteorology Research Programme (TMRP), 13pp. [http://www.wmo.ch/pages/prog/arep/tmrp/documents/iwtc_statement.pdf