

**IN THE HIGH COURT OF NEW ZEALAND
AUCKLAND REGISTRY**

CIV-2010-404-005092

UNDER

the Judicature Amendment Act 1972 and Part 30 of the High Court Rules

BETWEEN NEW ZEALAND CLIMATE SCIENCE EDUCATION TRUST

Applicant

A N D

**NATIONAL INSTITUTE OF WATER AND ATMOSPHERIC RESEARCH
LIMITED**

Respondent

**SECOND AFFIDAVIT OF MANFRED OTTO DEDEKIND IN SUPPORT OF
APPLICATION FOR DECLARATORY JUDGMENT**

Sworn this 11th day of July 2012

Next event date: Hearing – 16 July 2012

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I, **MANFRED OTTO DEDEKIND** of Auckland, physicist, swear:

1. I am the deponent of a previous affidavit sworn and filed herein. At the time that document was being prepared, I was not asked to refer to a Code of Conduct and was not then aware of the requirements of the High Court Rules.
2. I confirm that I have now read the Code of Conduct for Expert Witnesses in the High Court (Consolidated Practice Note 2006), and I agree to comply with it in all respects. This affidavit has been prepared in accord with the principles and practices specified in the Code.
3. I am an IT professional with a background in computer modelling of high temperature non-linear damage effects in superalloys, such as creep and thermal fatigue. I completed a B.Sc (Hons) in Physics at the University of Natal, South Africa in 1986, and worked for the CSIR in Pretoria, South Africa for ten years before emigrating to New Zealand.
4. In my work as a modeller, I was constantly engaged with statistical analyses. While claiming no particular aptitude in advanced statistical theory, I have a sound grounding in the practical application of established statistical techniques.
5. The issues which I address in this affidavit relate mainly to the application of a statistical comparison between certain datasets, in accord with the techniques described in a scientific journal paper, Rhoades & Salinger (1993)¹ ("RS93").
6. In addressing those issues, I have assumed that those comparison techniques were fully and accurately described in RS93, and that the reasons ascribed for selecting the particular techniques were valid.
7. I have read the 2010 booklet ("the Review")(Exhibit A64) describing NIWA's reconstruction of the Seven-station Series ("7SS") as well as the NZ

¹ Rhoades, D. A., and Salinger, M. J., 1993: Adjustment of temperature and rainfall records for site changes. *International Journal of Climatology*, **13**, 899

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Meteorological Service ("MetService") technical reports known as Fouhy et al (1992)² and Salinger et al (1992)³.

8. I have assumed the correctness of the general methodology of neighbour - station comparisons as described in the Review, and that the comparison stations selected by NIWA were appropriate for that purpose. I utilised raw data downloaded from NIWA's online climate database which I understand was also used in the Review.
9. The calculations required to apply the RS93 comparison techniques to the downloaded data are well within my area of expertise, as is my assessment of the similar techniques used by NIWA.
10. I have also read and understood the scientific journal papers Hessel (1980)⁴ and Hansen et al⁵ (2001) which deal with the detection and effects of contamination of statistical data.
11. Issues of data contamination and the detection of sudden or gradual changes within datasets are practical and familiar statistical issues which are within my area of expertise.
12. The opinions expressed in my affidavit are based on standard statistical methodology. Although I sometimes rely upon climate-related matters described in journal papers, I offer no personal views on issues requiring expertise in meteorology or climate change research.
13. I joined the NZ Climate Science Coalition ("NZCSC") in November 2008. I was a co-author of the 2009 NZCSC paper "Are We Feeling Warmer Yet?"

² Fouhy, E.; Coutts, L.; McGann, R.P.; Collen, B.; Salinger, M.J. (1992). South Pacific Historic Climatological Network Climate Station Histories. Part 2: *New Zealand and Offshore Islands*. NZ Meteorological Service, Wellington, ISBN 0-477-01583-2, p 216

³ "South Pacific Historical Climate Network. *Temperature Trends in New Zealand and Outlying Islands*"

⁴ Hessel, J.W.D. 1980. Apparent trends of mean temperature in New Zealand since 1930. *New Zealand Journal of Science*, **23**, 1-9.

⁵ Hansen, J.; Ruedy, R.; Sato, M.; Imhoff, M.; Lawrence, W.; Easterling, D.; Peterson, T.; Karl, T., 2001: *A closer look at United States and global surface temperature change*. *J. Geophys. Res.*, 106, 23 947-23 963

(Exhibit A13) which examined the adjustments made to the raw temperature data to produce the 7SS.

14. I participated in the analysis work undertaken by members of the Coalition which was collated into the papers "Statistical Audit of the NIWA 7-Station Review" and "Supplementary Information" (collectively, "the Audit") appearing on the Coalition's website (Exhibit A67).
15. I took final responsibility for the calculations within the Audit, and the drafting of the documents. I believe the calculations and other statements set out in the Audit are true and correct.
16. I forwarded a penultimate draft of the documents to three separate consulting statisticians requesting that they "peer-review" the methodology and calculations. All three responded that they believed the documents to be correct. The minor improvements they suggested were incorporated into the text.
17. The main purpose of the Audit is to correctly apply the statistical comparison techniques described and used in RS93, whilst the remainder of the Review methodology is left unchanged. Although this is achieved, it also became necessary to correct certain flaws in the Review, which are described below.
18. The 7SS adjustments described in the Review appear to be faulty in several respects:
 - the comparison techniques depart from the RS93 precedent in four ways, which markedly affect the results;
 - the departures from RS93 are not mentioned or explained in the Review;
 - confidence levels of the adjustments are not disclosed. Because confidence levels were unknown, several statistically-insignificant adjustments have been wrongly accepted;
 - a substantial instrument change at Hokitika was not recognised;



- no attempt was made to test and correct for known or suspected data contamination at Auckland, Wellington, Nelson and Lincoln sites (with one exception);
- potentially contaminated data has been spliced with data from a subsequent site, a practice which Hansen et al (2001) reports as producing a false upwards trend;
- potentially contaminated data from Auckland (and others) has been used to drive adjustment calculations at other stations.

Rhoades & Salinger (1993)

19. NIWA contends that its adjustments are based on internationally-accepted methodology, and its website states that "the methodology for adjusting for site changes in the NZ temperature record was published in the peer-reviewed [RS93]⁶. RS93 is the only authority dealing with comparison techniques mentioned in any of the eight bibliographies contained within the Review.
20. The NIWA bibliographies for all of the seven station reports mention an appendix to a thesis submitted by James Salinger at the Victoria University of Wellington in 1981 ("the Thesis").
21. RS93 deals with homogenising historical temperature records for periods that include known site changes, where unbroken monthly data from neighbouring stations is available for at least 12 months (and preferably 24 months) before and after the site change. The paper describes and illustrates a set of statistical comparison techniques to identify the value of any adjustments which could be expected to improve the reliability of the series.
22. Part of the RS93 methodology is described on page 900:

"The method proposed here... is to use a symmetric interval before and after the site change and select only those neighbouring stations that have no site changes over the period of comparison. The standard error is

⁶ www.niwa.co.nz/our-science/climate/information-and-resources/nz-temp-record/review/changes/seven-station-series-temperature-data



based on the variation of a set of differences (between the target station and its neighbours) of monthly differences (before and after the site change)."

23. The Review departs from this methodology in using annual differences before and after the year (rather than month) of the site change. The Review also uses asymmetric intervals, on occasion.

24. RS93 discusses the length of the comparison period at page 900 (near the foot of the page):

"The use of monthly differences means that the t-statistic has relatively high degrees of freedom, even when computed from a short time interval of only 1 or 2 years before and after the site change. The period of comparison is kept relatively short in order to avoid contamination by gradual effects, or sudden but unrecognized effects, at one or more of the neighbouring stations. If no such effects are present, it is optimal to use as long a period as possible. However, in this case, the usual concern to maximize the power of the test is balanced by an opposing concern that the modelling assumptions are likely to be more seriously invalidated as the period of comparison is lengthened."

25. The authors' concerns regarding possible contamination are well founded. The Review finds contamination by shelter at Auckland and Lincoln and Fuohy et al (1992) mention shelter effects at other sites in the 7SS. UHI is an ever-present threat in cities or towns, as it is not disclosed in the metadata.

26. In a 1-year comparison period there are 11 degrees of freedom each side of a change point. The Review uses 10-year comparison periods with annual data which allows 9 degrees of freedom each side. If the comparison datasets were guaranteed to be perfectly contamination-free, a longer period would be better. But RS93 points out that the opposite is true when a real contamination risk exists. On page 899, the authors suggest that contamination risk is almost always present in meteorological series, and that is certainly the case in respect of the 7SS stations. The actual worked examples in RS93 use a 2-year ($k=2$) period.

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27. DW347 notes the different outcomes as between using a ± 2 year and a ± 4 year period. Those differences are magnified considerably if a ± 10 year period is used, as in the Review.
28. A weighted average is taken of the 'set of differences', the weighting being the fourth power of the correlation between the target station and each comparison station. RS93 (page 906) discusses weighting strategies and suggests the options are worthy of future study.
29. The Review does not discuss the merits of weighting options, and uses simple unweighted averages. Weighting does not have much effect when all the comparison stations have similar correlations, but it can make a big difference if one station has a high correlation with the target while others do not.
30. In RS93, the adjustment is adopted if it is significant at the standard 5% level (page 904-5) – ie if there is 95% confidence it is a real improvement, and not merely "noise". The 'significance' or 'confidence' level is said (page 905) to have:
- ".. a profound effect on the statistical properties of the adjusted series. A rational choice of [level] would involve minimizing a loss function, which balances the risks of Type I and Type II errors (ie adjusting when no shift occurred and failing to adjust for a real shift)."*
31. The Review uses no significance limits (which are based on confidence levels) and makes no attempt to balance between the two types of statistical errors. Some texts use the metaphor that Type I errors convict an innocent party and Type II errors allow a guilty person to go free. On this basis, the Review techniques would convict every accused person. Every adjustment considered was accepted.
32. At paragraph 104 of the affidavit of Dr David Wratt ("DW104") it is said that RS93 adjustments can sometimes fail to reach the 95% confidence level unless the comparison period is extended. That is like changing the rules to avoid Type I errors being identified and excluded..



33. The prevalence of potential Type I errors is underlined by the worked example in RS93. Table ii (page 907) identifies 13 data shifts that are candidates for adjustments. After significance testing, 11 of these are excluded.
34. I have seen the NZCSC table at Exhibit B2 which compares the statistical comparison techniques used by the Thesis, the 7SS, the Review and the Audit. In my opinion, the table is correct and shows that the first three documents used the same set of techniques.
35. DW186 says the "length of period" used by [RS93] was not 1-2 years, it was substantially longer. This point and others are elaborated in Dr Mullan's papers at Exhibits DSW4 and DSW 12. I have responded to those papers in my paper at Exhibit B3 and I believe the statements in my response are correct.
36. DW327 says that the NZCSC 'invented' the preference for only ± 2 years and applied RS93 "rigidly and narrowly" to sometimes obtain insignificant results. "Had the Coalition extended their comparison period to ± 4 years as Salinger et al (1992) did, their result would have been very different and closely parallel to those in the NIWA Review."
37. I agree with Dr Wratt's analysis that the differences in comparison period/confidence level largely account for the results gulf as between the Audit and the Review. The "rigid" application of RS93 excludes some Type I errors.
38. The "results gulf" is shown in the following table:

TABLE 1: Results of Different Statistical Comparison Techniques (linear trends in °C/century: 1909-2009)

	7SS	NZT7	RS93 (Audit)
Dunedin	0.58	0.58	0.24
Lincoln	0.99	0.83	0.21
Hokitika	1.07	1.11	0.21

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Nelson	0.81	0.76	0.27
Masterton	0.80	0.88	0.36
Wellington	0.79	0.86	0.48
Auckland	1.34	1.53	0.27
4SS* Average	0.80	0.76	0.27

*This is the average of Dunedin, Lincoln, Nelson and Masterton – where the statistical techniques were the only material difference between the Review and the Audit.

Salinger et al (1992)

39. Each station report in the Review contends that the Schedule's adjustments were calculated in Salinger et al (1992). This statement seems to be incorrect. I have read the paper and it does not contain those calculations or suggest adjustments for any of the 7SS stations.
40. Work carried out by the MetService in 1992 apparently led to some form of homogenized temperature series for 1920-90 comprising 21 mainland New Zealand stations plus three from islands ("MetService24"). (DW24, 79). This series does not seem to have been published anywhere and I have been unable to obtain a copy.
41. Salinger et al (1992) discusses the homogenisation methods used in the course of deriving MetService24, and refers to a procedure described in "Rhoades and Salinger (1992)". No copy of that paper is available, but it seems likely to have been an early draft of RS93, which was published the following year. I note that Salinger92 also refers to the Thesis techniques, using annual data, being applied to MetService24.
42. The 1992 paper mentions "comparison periods of monthly data ± 2 or ± 4 years before and after" a site change, whilst the 1993 paper uses just ± 1 year and ± 2 years. The longer period seems to have been excised during the peer-review

process that would have occurred before RS93 was accepted by the journal. At the end of RS93, the authors acknowledge that two anonymous referees "made several valuable suggestions".

43. RS93 describes a detailed statistical technique involving correlation-based weighting and 95% confidence levels which receive no mention in Salinger92.
44. Salinger92 indicates that MetService24 applied only to the period 1920-1990, so it could not have directly supplied the 7SS Adjustments for the 7SS graph which appeared on NIWA's website in 1999. That graph extended from 1853 to the present. In contrast to MetService24, the Thesis also provided adjustments from 1853.
45. DW36 claims MetService24 was used by Zheng et al (1997) but that paper refers to the Thesis and does not mention either MetService24 or Salinger92. Its trend studies date back to 1896. Similarly, Basher & Thompson (1996) compares its station data with the Thesis and fails to mention either MetService24 or Salinger92. Both papers were part-funded by FRST and all of the authors were on the staff of NIWA .
46. DW97 states that the 7SS came from the time series reported in Folland & Salinger (1995). Page 97 of Folland & Salinger (1995) makes it clear that the 7SS (referred to as "NZT") was actually drawn from the Thesis, and not from MetService24. The NZT period ran from 1853, as did the Thesis.

UHI/Shelter

47. The Review acknowledges the need to correct for any contamination of raw data by non-climatic factors. Because warming caused by UHI or shelter is typically progressive, it proposes a correction method (in Fn 1, page 8) of "successively reducing annual temperatures by 0.01°C more than the previous year".
48. It is important to affirmatively establish that a site is free of UHI/shelter impacts before its data is spliced with that of any neighbouring site. The splicing

process involves the earlier site's data being adjusted up or down by the estimated climatic difference between the two sites. If progressive non-climatic effects are not first excluded, they will be conveyed to the earlier site at peak value and maintained at that level throughout the lifetime of the site.

49. The effects of splicing contaminated data are shown graphically in Hansen et al (2001) which are reproduced in the Audit (Supplement at page 58). For example, the result of including Albert Park's 1976 non-climatic warmth is to subtract that same warmth from the site's data 60-70 years earlier. In those early years, the trees might have been saplings and the high-rise buildings unimagined.
50. The graph reproduced in the Audit shows an exaggerated linear warming trend caused by UHI/shelter. The adjustment moves that trend to a lower level, but significantly increases the slope of the warming curve. This produces a false warming trend, which is linear over the lifetime of the earlier site.
51. I believe this linear false warming effect could explain the shape of the NZT7 graph as highlighted in Exhibit A51.
52. RS93 points out that it is very difficult to distinguish genuine climate change from gradual change caused by non-meteorological factors (page 899). It recommends avoiding stations "likely to be affected" by shelter or urbanization. Towns with populations as small as 1,000 experience urban heating⁷. Station metadata recorded in Fouhy et al (1992) show serious instances of sheltering by trees at Auckland, Wellington, Nelson (pre-1920) and Lincoln (pre-1944).
53. The Review indicates that NIWA has not yet tested for contamination by the usual methods of assessing day-time and night-time temperatures for progressive change (although further research is promised) and applying analyses such as wind-testing and other methods used in Hessell (1980).
54. In the cases of Lincoln College and Kelburn, NIWA has applied a Wang test at selected times. The Wang test is designed to locate sudden discontinuities or step changes, and has no useful application in identifying long-term progressive

⁷ Torok, S.J., Morris, C.J.G., Skinner, C. and Plummer, N. 2001. "Urban heat island features of southeast Australian Towns" *Australian Meteorological Magazine* 50:1-13.



changes. At Lincoln, it probably identified sudden jumps when the shelter belts were cut back.

Auckland

55. Hessell (1980) found that Albert Park in Auckland was seriously affected by both shelter and UHI during 1930-80 and Kelburn in Wellington was affected by shelter. This paper has been in the international scientific literature for 30 years and has never been seriously challenged. In fact, I have been unable to find any other journal paper describing shelter/UHI effects at weather stations in New Zealand. However, on sheltering in general, McAneney et al (1990)⁸ found that shelter belts on their own increased maximum temperatures by up to 1°C over a relatively short 6-year period.
56. The Review cites the Hessell (1980) paper in its bibliographies for both the Auckland and Wellington stations, and quotes some material from it in the Auckland section.
57. DW197 criticizes Hessell (1980) because two sites the paper believed to be free of shelter/UHI were later discovered to be affected by shelter (Ophir) and unsatisfactory (Lake Coleridge B). This merely reinforces the paper's point that most sites were unreliable. The affidavit does not criticize the testing undertaken by Hessell at Auckland and Wellington or suggest that those results were unreliable in any way.
58. NIWA's consideration of gradual change caused by UHI and shelter is limited to Albert Park during 1928-60. The impacts on that site during 1909-28 are not tested against Te Aroha because that station's records are unreliable pre-1928. I believe it would be logical to assume that contamination was the same pre-1928 as it was post-1928, as the trees were growing throughout the whole period, and Hessell (1980) p. 3 shows a significant reduction in wind run measured from 1916. On page 40, the Review calculates the impact of warming by multiplying 0.09°C/decade by 32 years, so the impact over 1909-60 would be

(0.09×50) 0.45°C . That is 50% greater than the Review' 1928-60 estimate which produced a calculated trend change (page 8) of 0.38° . The Audit found contaminations at Albert Park and Auckland Aero of 0.53°C and 0.46°C respectively. A contamination-caused trend change of 0.99°C is a very significant part of the total 1.53°C attributed to Auckland.

59. There is a strong likelihood that the shelter contamination was even greater pre-1928 than during 1928-60. The Thesis is quoted (page 38 of the Review) as stating that the Albert Park trees "reached their maximum height in 1930, and it is not expected that they will further affect the exposure". The wind-graph data appearing at page 3 of Hessel (1980) suggests that the sheltering effects were growing rapidly before 1930.
60. Although the Review also compares the Albert Park trend with Waipoua Forest, footnote 29 on page 39 notes that forested sites are noticeably warmer than others, no doubt because they have shelter problems.
61. The period 1960-76 was disregarded with a comment that "Albert Park shows no differential warming relative to other sites in the northern North Island after the mid-to-late 1950s". No comparisons are shown but this statement does not accord with the graph of the Albert Park/ Te Aroha comparison at page 57 of the Audit (Exhibit 67.2). It is also inconsistent with the comment in the Schedule of Adjustments (footnote 3 page 4 Exhibit A25) which suggests that the station was moved to Mangere in 1976 because of urban warming. If this evidence is accepted, the $0.09^{\circ}\text{C}/\text{decade}$ would have continued for 68 years, with a consequent trend more than double the $0.38^{\circ}\text{C}/\text{century}$ calculated by NIWA.
62. The impact on the overlap with Mangere – described in the Audit (page 58, Supplementary Papers) is also disregarded. In my view, the UHI effects at Auckland Aero and Mangere also made them quite unsuitable for climate records, for the reasons described in the Audit.

⁸ K.J McAneney, M.J Salinger, A.S Porteous, and R.F Barber 1990 "Modification of an orchard climate with increasing shelter-belt height" *Agricultural and Forest Meteorology*, 49: 177-189



63. DW317 says "while there may have been an excess warming of about 0.3°C this is by no means certain". This statement indicates a view that a site should be included unless it can be positively proved, to a high standard, that its data was contaminated. This is very difficult to prove, especially in the absence of testing methods described in the Hessel (1980) paper. RS93 (p. 899) states that "Gradual changes can seldom be assigned with any certainty to non-meteorological causes." According to RS93, any likelihood of contamination is enough to exclude a site: "Where long-term homogeneous series are required, for example, for studies of climate change, it is best to choose stations that are *unlikely to have been affected* by gradual changes in shading or urbanization." (Emphasis added).
64. The obvious response is to exclude Auckland from the study. The Review discussed the impact of this course, but failed to implement it. It noted that removal would leave six stations showing an average warming trend of 0.81°C/century. DW317 (7th bullet) says this lower figure "would not agree so closely with the independent 11SST or be consistent with the published trends in sea surface and night-time air marine air temperature around New Zealand last century." However, Folland & Salinger (1995) notes the sea-surface trend at 0.6°C/century and the night-time air trend at 0.7°C/century, while a glacier study (DW110) suggests 0.6°C/century and the IPCC global average was 0.74°C/century (DW110). So the decision to retain Auckland seems to depend heavily on the 11SS being more reliable than the other "lines of evidence".

Wellington

65. Hessel (1980) found (page 4) that the Kelburn site was affected by UHI and shelter during 1945-70 to about half the extent of Albert Park. Fouhy et al records that trees were cropped in 1949, 1959 and 1969, but it appears the problems have been ongoing in 1986 and later (see A34 and A35).
66. In both 1912 and 1928 Wellington data was adjusted to an extent which was influenced by data from Albert Park (A49). The latter site was being directly




contaminated by UHI/shelter, and the long comparison periods used by NIWA allow such gradual changes to have an appreciable effect.

Hokitika

67. Hessel (1980) declared Hokitika "unworthy of investigation due to periods of unsatisfactory records"...
68. The 2010 Hokitika document published with the Schedule as a sample station record [A26] ("the Sample") discovered a site which had operated for 20 years from 1943 but was not recorded in the electronic database. The Sample had used the same agent number (3709) from 1867 until 1964. During 2010, the rediscovered site was named Southside and numbered 37939.
69. As there was a 1943-45 overlap between Town and Southside sites, the single dataset was divided between the two sites as shown in footnote 6 on page 117 of the Review. One month prior to the commencement of the Southside station, the minimum and maximum thermometers (and largely the screen) were replaced, possibly due to flooding. This site change was not examined by NIWA and is not mentioned in the Review.
70. The Audit found that the new instruments/screen at the Town site were recording sharply higher temperatures, so that a further adjustment is necessary.
71. In 1928 Hokitika data was adjusted to an extent which was influenced by data from Albert Park (A49). If the latter site was being directly contaminated by UHI/shelter, the long comparison periods used by NIWA might allow such gradual changes to have an appreciable effect.

Lincoln

72. The Thesis declared that Lincoln records should be used with caution before 1927, but Fouhy et al (page 141) details serious shelter problems persisting from 1915 to 1943. Contrary to the direction in RS93, NIWA attempts to treat the problems as sudden step changes occurring in 1915 (-0.52°), 1923 (+0.57°) and 1925 (-0.61°C) rather than gradual progressive changes, possibly punctuated by shelter cut-backs.
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Conclusion

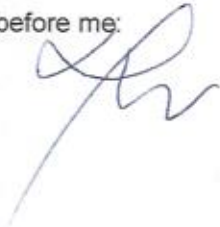
In my view, NIWA's methodological errors account for most of the warming trend shown in the 7SS during the 20th century.

Sworn at Auckland)



11^m day of July 2012)

before me:)



MARIAN KOHLER
SOLICITOR
AUCKLAND

A Solicitor of the High Cou^{rt} of New Zealand