1-118	A	6:7	7:22	Some of this history seems a bit irrelevant and long-winded. Can this section be shortened? [Dennis Hartmann (Reviewer's comment ID #: 100-2)]	Rejected. An attempt was made to shorten this section without deleting important details in this history of global temperature time series. This resulted in too little being removed to be able to say accepted. It should be noted that this version is shorter than the zeroth order draft or the first order draft.
1-119	A	6:8	6:8	Change "Kingston" to "Kington" here and in the bibliography. [David Parker (Reviewer's comment ID #: 195-6)]	Accepted.
1-120	A	6:13	6:13	There is no such thing as "modelling" evidence! All the evidence comes from lab or field. The computer guides the experiments but can in and by itself not generate observational evidence. This is probably the point I insist the most in order to protect the public from misunderstandings of what models can or cannot do. [Michel J. ROSSI (Reviewer's comment ID #: 220-3)]	This comment is misplaced as it is not relevant to Chapter 1 page 6.
1-121	A	6:15	6:16	Ament text to "…in 1873. Its successor, the World Meteorological Organization (WMO), formed in 1950, still works…". [David Parker (Reviewer's comment ID #: 195-7)]	Accepted.
1-122	A	6:20	6:20	Amend text to "…areal averaging in the presence of substantial gaps". [David Parker (Reviewer's comment ID #: 195-8)]	Accepted.
1-123	A	6:22	6:22	Replace "most" by "some" No need to exaggerate [VINCENT GRAY (Reviewer's comment ID #: 88-65)]	Rejected. There are four items (data, qc, homogeneity and area averaging) and Koeppen addressed three of them: data, qc and area averaging. Therefore most is more accurate than some.
1-124	A	6:22	6:22	Replace "near global" by "extensive". Here you go again! None of the netwoks of weathert stations past or present comes remotely near to a randomly distributed series. ALL averages are therefore automatically biased until such a network is available, or until a correction procedure can be found.. Satellitrte surveys, by contrast, can derive truly global averrasges [VINCENT GRAY (Reviewer's comment ID #: 88-66)]	Rejected. One of the major reasons for area averaging is to extend the amount of the globe considered. Koeppen's approach allowed him to make an assessment for nearly the whole globe.
1-125	A	6:24	6:24	change "large-scale" to "external" ? [Ileana Blade (Reviewer's comment ID #: 22-7)]	Rejected. The reviewer's suggestion would be accurate but it would loose the sense that non-global scale forcing can be assessed by mean global temperatures. An example of that would be volcanic aerosols which

					primarily directly impact a wide latitude band but in so doing indirectly impact global temperatures.
1-126	A	6:24	6:24	Insert before "global" Apparent" [VINCENT GRAY (Reviewer's comment ID #: 88-67)]	Rejected. Mean global temperature is a well defined, commonly used term. Rather than apparent we could add anomaly instead but that would just unnecessarily make the statement more complex.
1-127	A	6:24	6:25	Amend text to "Later he identified quasi-decadal fluctuations in global temperature (Köppen, 1880, 1881; see also Figure 1.3)" and move the sentence to the end of the paragraph. [David Parker (Reviewer's comment ID #: 195-9)]	Rejected. Initially accepted but then later the whole sentence was removed to make the section shorter.
1-128	A	6:26	8:26	Insert after "climate change" "(defined to include all forms of change)" [VINCENT GRAY (Reviewer's comment ID #: 88-79)]	Rejected. This addition is unnecessary and would make the section longer. (The comment is mislocated as it should be for line 8:26 to 8:26.)
1-129	A	6:28	6:29	Amend text to "…100 stations, Köppen (1873) averaged annual observations from 1820 to 1971 into several major latitude belts and then area-averaged these into near-global time series." [David Parker (Reviewer's comment ID #: 195-10)]	Accepted.
1-130	A	6:31	6:31	"replace "global" with "near-global" [VINCENT GRAY (Reviewer's comment ID #: 88-68)]	Rejected: Callendar (1938) called his time series global (actually "for the earth") due to area averaging.
1-131	A	6:36	6:36	"replace "global" with "near-global" [VINCENT GRAY (Reviewer's comment ID #: 88-69)]	Rejected: Callendar (1938) called his time series global (actually "for the earth") due to area averaging.
1-132	A	6:46	6:46	"replace "global" with "near-global" [VINCENT GRAY (Reviewer's comment ID #: 88-70)]	Rejected. Willett (1950) called his time series global (actually "world") due to area averaging.
1-133	A	6:56	6:56	"replace "global" with "near-global" [VINCENT GRAY (Reviewer's comment ID #: 88-71)]	Accepted.
1-134	A	7:2	7:2	"replace "global" with "near-global" [VINCENT GRAY (Reviewer's comment ID #: 88-72)]	Rejected. Mitchell (1963) called his time series global (actually "world") due to area averaging.
1-135	A	7:8	7:8	Change "that approach" to "whose approach" [Michael MacCracken (Reviewer's comment ID #: 152-236)]	Accepted. However, while "that approach" is no longer in the text, the sentence has been rewritten a different way than the reviewer recommended.

1-136	A	7:14	:22	Would it be useful to spell out in more detail which datasets are homogenized by adjusting the data, and which exclude suspect data but leave the other datasets unaltered? [gabi hegerl (Reviewer's comment ID #: 103-7)]	Rejected. The reviewer's suggestion is too detailed for this overview, which need to be shortened.
1-137	A	7:22	7:22	Add at end "This procedure is only possible where there are large numbers of weather stations for comparison purposes. So far full "homogeneity adjusted" records have only been published for the continental USA, and for China. In both cases the "adjusted" records show little oerall warming for the past century, suggesting that this might be true for the entire near-global set. It might also be mentioned that the wholesale closing down of weather stations worldwide since 1987 has probably biased the average, as they would have been predominantly rural" [VINCENT GRAY (Reviewer's comment ID #: 88-73)]	Rejected. The reviewer is correct that this procedure doesn't work well when there are no stations near by. But example stations like that are few. Saint Helena Island is one of them. So the impact of the first part of the reviewer's comment is minor. For the second part, a bias due to closing of rural stations, analysis has shown that not to be true. Yes there is a decrease in the number of stations in global data sets in recent years, but that is less due to closing than to delay in data exchange. Analysis of global rural and full data set trends indicate the results are quite similar so there is no long-term bias. Analysis of two different approaches to recent data, the anomaly method and the First Difference method, shows little difference in the end result. This would not be the case if the reviewer's comment was correct.
1-138	A	7:27	7:28	The inclusion of this statement contradicts the methodological material on pages 1-3 to 1-4. The claim that there is only negligible nonclimatic contamination of surface temperature data is a hypothesis. It was tested in McKitrick and Michaels (2004) and deLaat and Maurellis (2004) and convincingly rejected in each case, using independent data and methods. The Jones and Peterson papers are quite old and do not provide counterevidence overturning any of the results in M&M of dL&M. [Ross McKitrick (Reviewer's comment ID #: 174-1)]	Rejected. The Jones and Peterson papers are old and that is one of the reasons we cite them. Our mandate is to not present material such as McKitrick and Michaels (2004) which is after the TAR. Those papers would be addressed by later chapters in AR4. Indeed, Chapter 3 addressed this concern explicitly by stating: McKitrick and Michaels (2004) and De Laat and Maurellis (2006) attempted to demonstrate that geographical patterns of warming trends over land are strongly correlated with geographical patterns of industrial and socioeconomic development, implying

					that urbanisation and related land-surface changes have caused much of the observed warming. However, the locations of greatest socioeconomic development are also those which have been most warmed by atmospheric circulation changes (Sections 3.2.2.7 and 3.6.4) which exhibit large-scale coherence. Hence the correlation between warming and industrial and socioeconomic development ceases to be statistically significant. In addition, observed warming and transient greenhouse-induced warming is expected to be greater over land than over the oceans (Chapter 10), owing to the smaller thermal capacity of the land.
1-139	A	7:27	7:28	Jones (1990) should not be cited here, and certainly not in support of the claim that it rules out a global imprint of urbanization effects on temperature data. First, the paper is 15 years old, and refers to data sets that are not the ones used in the AR4. Second, Jones 1990 only examines the US, the western USSR, eastern Australia and eastern China; hardly a global or even hemispheric sample. Third, it proves the opposite of the assertion being made, since the evidence presented in the paper all points to differential urban-rural trends that dominate the regions. In the USSR data they say: "Over the 1930-1987 period, a cooling of ~0.2 C in RUSSR [rural series] is observed. This cooling is about 0.1 C smaller in JUSSR [combined rural-urban], but there are no statistically significant differences between the two series." (p.171). For eastern China they say: "The warming in UCHI [urban series] is 0.39C, considerably higher than that in RCHI [rural series]. For this region, UCHI is the only series for which warming is staistically significnat." (pp. 171-172). For eastern Australia they find similar warming in the rural and urban series, though they define "rural" as up to 33,368 persons. For the US they report earlier findings of a significant (0.15C) urban warming bias. Yet in both the abstract and the conclusion of their paper, they assert that their results provide little or no evidence of urbanization bias, a statement directly contradicted by their own evidence. They suggest that urbanization represents at most 0.05 C of the observed 0.5 C warming over the entire century, with no quantitative basis whatsoever. The 0.05 figure is not calculated anywhere in the paper, it is an off-the-cuff guess about the maximum that might be observed in key areas of the world they did not examine, i.e. Europe and the tropics (p. 172). Despite finding an urban warming bias everywhere but eastern Australia they assert that “In none of the three	Rejected. The question that needs to be addressed is not whether some rural stations are showing less warming (or more cooling) than some urban stations but whether the global temperature time series are biased warm by the presence of urban stations. The reviewer’s comments are not directed towards this question. While the reviewer is accurate in the extraction of a subset of numbers, the numbers not mentioned prove the reviewer’s point is wrong. Take Russia for example. Yes rural Russia had a trend of -0.21 while urban was -0.09. Does this indicate an urban bias in global gridded datasets? No because the number the reviewer did not list was that the Russian area from the global gridded data set which had a trend of -0.20. Take China. Yes the reviewer is correct that urban China was warming faster than rural China

				regions studied here is there any indication of significant urban influence" and "The United States result therefore does seem somewhat atypical compared with other industrialized regions of the world" (p. 172). This latter statement is particularly misleading since their ad hoc sample of eastern China, eastern Australia and the western USSR hardly constitute the "industrialized regions of the world" outside the US. Quoting their "spun" conclusion while ignoring the paper's own evidence is deceptive to IPCC readers. If you want to refer to Jones (1990) then quote it accurately: it provides evidence that urban influences on temperature data do show up in several regions including the US, China and the Russia, and it provides no evidence that these influences are small in the global average. [Ross McKitrick (Reviewer's comment ID #: 174-2)]	but again the reviewer did not mention that the global gridded dataset area for China was warming even less than the rural. The same is true for Australia. Ergo, this comment is rejected. While the reviewer indicates that the paper's own evidence is ignored by the IPCC in the "spun" conclusion, it would be more accurate to state that this reviewer not presenting the numbers from the global gridded data set is actually ignoring evidence and thereby spinning conclusions not represented by the paper.
1-140	A	7:27	7:28	Sources cited: McKitrick, R and P. J. Michaels (2004). "A Test of Corrections for Extraneous Signals in Gridded Surface Temperature Data" Climate Research 26(2) pp. 159-173. "Erratum," Climate Research 27(3) 265—268; de Laat, A. T. J. and A. N. Maurellis. (2004) "Industrial CO2 emissions as a proxy for anthropogenic influence on lower tropospheric temperature trends." Geophysical Research Letters, VOL. 31, L05204, doi:10.1029/2003GL019024, 2004. [Ross McKitrick (Reviewer's comment ID #: 174-3)]	Rejected. Our mandate is to not present material such as McKitrick and Michaels (2004) which is after the TAR. Those papers would be addressed by later chapters in AR4. Indeed, Chapter 3 addressed this concern explicitly by stating: McKitrick and Michaels (2004) and De Laat and Maurellis (2006) attempted to demonstrate that geographical patterns of warming trends over land are strongly correlated with geographical patterns of industrial and socioeconomic development, implying that urbanisation and related land-surface changes have caused much of the observed warming. However, the locations of greatest socioeconomic development are also those which have been most warmed by atmospheric circulation changes (Sections 3.2.2.7 and 3.6.4) which exhibit large-scale coherence. Hence the correlation between warming and industrial and socioeconomic development ceases to be statistically significant. In addition, observed warming and transient

					greenhouse-induced warming is expected to be greater over land than over the oceans (Chapter 10), owing to the smaller thermal capacity of the land.
1-141	A	7:28	7:28	Also Parker (2004). "Climate: Large-scale warming is not urban". Nature 432: 290. [Ileana Blade (Reviewer's comment ID #: 22-8)]	Rejected. Paper cited is post TAR and is addressed in Chapter 3.
1-142	A	7:28	7:28	Add at end. "This conclusion, though, only applies after "homogenieity adjustment" and was found only for the USA… [VINCENT GRAY (Reviewer's comment ID #: 88-74)]	Rejected. After citing two global papers it is inappropriate to say that this applies only to the USA. It is unnecessary to mention homogeneity adjustments as all global datasets now have homogeneity adjustments as stated earlier in the text.
1-143	A	7:28		The discussions in Chapters 1 and 3 are not detailed enough to do the urban heat island effect justice. Needs a more organized discussion about the questions raised and how addressed/resolved. For example, one of the main findings in Chapter 3 is that there have been increases in the extremes of temperatures, which are consistent with global warming. In an analysis of Australian and Argentine temperatures, Camilloni and Barros (1997) showed that interannual variability of temperature is generally lower in urban environments than in rural areas; in other words, urban stations are prone to have lower trends in absolute value than rural ones. Could the trend in temperature extremes globally simply reflect a disproportionate increase of rural stations globally over time? The scientific consensus is that in a global analysis, such biases all tend to come out in the wash. Consider merit of including Camilloni and Barros findings to the discussion. [Govt. of United States of America (Reviewer's comment ID #: 2023-12)]	Rejected. The merit of Camilloni and Barros (1997) was considered but the paper was not added as the focus was on long-term trends rather than differences in variability. As pointed out by an earlier reviewer comment (1-137), the exact opposite of the reviewer's hypothesis is happening: there is a decrease in the fraction of rural stations going into global temperature analyses in recent years. This section describes how potential urban heat islands were addressed in the past. The responsibility for adequately explaining how they are currently addressed lies with Chapter 3.
1-144	A	7:37	7:37	Delete "significant". In science this is usually associated with statistical methods [VINCENT GRAY (Reviewer's comment ID #: 88-75)]	Accepted. Significant replaced by major.
1-145	A	7:37	7:45	For reasons relating to how we believe and interpret the 20th century climate record, I think this paragraph needs to have a sentence or two about the problems that arose during WWII, when not only the method of measuring SST was changing (to intake temperature) but when there were all sorts of other problems. For example, it is my understanding that nighttime marine air temperatures had to undergo quite significant adjustments (like 2 C) due to the change in how measurements were made (near wheelhouse instead of bow of ship). In addition, the spatial coverage of observations changed (particularly over the oceans), observers changed, etc. It is likely very hard to go back and reconstruct what	Rejected. Much of the point made by the reviewer is correct. The problem is that (a) this section is already too long and addressing the reviewer's comment adequately would be quite lengthy, (b) addressing them in passing would be confusing but in detail would overweight this concern, and most

				happened for each observations, but what seems clear is that the uncertainty about the measurements should generally be higher than for periods since (and before). This issue of uncertainty during the WWII period is important because that is said to be when there was a warming peak--and it is really strange that right after the end of the war there was a sudden return to a different situation. In my view we should be very suspicious of this change--and it is likely due to problems during WWII. I find it very interesting--and disturbing--that if one were to simply drop out the observations from during the war years, one would have a quite different impression of the 20th century temperature record, something that is true only for that period. It seems to me that this chapter having one example of where science is a bit stymied with past observations, rather than them always improving over time, might be useful. [Michael MacCracken (Reviewer's comment ID #: 152-237)]	importantly (c) some of the solutions to this problem have been applied since the TAR. Therefore it should be covered by later chapters.
1-146	A	7:45	7:45	Add at end "US workers have never accepted that this method is sufficiently reliable to incorporate such measurements in a global average.. Christy et al 2001 Geophysical Research Letters Vol 28 pages 183-186 have shown that the transition from measurements in buckets drawn from the sea to measurement in the engine intake introduces an upwards bias which, so far, has not been corrected"" [VINCENT GRAY (Reviewer's comment ID #: 88-76)]	Rejected. First off, the Christy MSU air temperature, particularly in the tropics, has been corrected since the cited paper came out (they had an error in their adjustment for diurnal drift). Secondly, the relevant US SST work ("US workers have never accepted that this method is sufficiently reliable …") is too recent for this section and should be addressed by AR4 Chapter 3. It should be noted, though, that the US statistical approach produces results quite similar to the method questioned by the reviewer. And lastly, there may well be a bias in the data in recent years but it is most likely a cold bias as very recent work has indicated that buoys are reading colder than ships.
1-147	A	7:51	7:51	Insert after " 1998)". "Christy et al [VINCENT GRAY (Reviewer's comment ID #: 88-77)]	Rejected. That reference is not necessary.
1-148	A	7:51		Should also include Argo floats, which provide SST observations with over 2000 now deployed. Change "several hundred" to "several thousand". [Govt. of United States of America (Reviewer's comment ID #: 2023-13)]	Rejected and accepted. Argo floats can not be included because, despite plans, they do not take SST measurements (the sensor is turned off near the surface to prevent biological contamination). In March of 2004, the number of buoys had increased to 909 drifters and in 2005 reached its target of 1250 drifting buoys. There are also 82

					moored buoys. So it is partly accepted in that the old several hundred term is clearly out of date and has been replaced with “over a thousand”.
1-149	A	7:52	7:52	insituinsitu [Stefan Brönnimann (Reviewer’s comment ID #: 30-1)]	Accepted.
1-150	A	7:52	7:52	Amend typo where "in situ" is repeated. [David Parker (Reviewer’s comment ID #: 195-11)]	Accepted.
1-151	A	7:52	7:52	"in situin situ" -> "in situ" [Pedro Ribera (Reviewer’s comment ID #: 213-2)]	Accepted.
1-152	A	7:52		repeat of "in situ" [Richard Allan (Reviewer’s comment ID #: 3-7)]	Accepted.
1-153	A	7:52		Typo: In situin situ should just be in situ [Govt. of United States of America (Reviewer’s comment ID #: 2023-14)]	Accepted.
1-154	A	8:3		Fig. 1.3. As in comment 1 for Fig. 1.1 the text is not clear for the same reason. [Govt. of Spain (Reviewer’s comment ID #: 2019-3)]	Accepted. The text describing the figure and the figure caption have been clarified and harmonized.
1-155	A	8:10	8:10	Add at end "But does not alter the fact that the average is greatly influenced by proximity to human habitation (see McKitrick and Michaels 2004 Climate Research Vol 26 pages 159-173) [VINCENT GRAY (Reviewer’s comment ID #: 88-78)]	Rejected. Our mandate is to not present material such as McKitrick and Michaels (2004) which is after the TAR. Those papers would be addressed by later chapters in AR4. Indeed, Chapter 3 addressed this concern explicitly by stating: McKitrick and Michaels (2004) and De Laat and Maurellis (2006) attempted to demonstrate that geographical patterns of warming trends over land are strongly correlated with geographical patterns of industrial and socioeconomic development, implying that urbanisation and related land-surface changes have caused much of the observed warming. However, the locations of greatest socioeconomic development are also those which have been most warmed by atmospheric circulation changes (Sections 3.2.2.7 and 3.6.4) which exhibit large-scale coherence. Hence the correlation

					between warming and industrial and socioeconomic development ceases to be statistically significant. In addition, observed warming and transient greenhouse-induced warming is expected to be greater over land than over the oceans (Chapter 10), owing to the smaller thermal capacity of the land.
1-156	A	8:10	8:10	Amend text to "…the changes they are indicating since 1900 are real". [David Parker (Reviewer's comment ID #: 195-12)]	Accepted.