Response to Second Rebuttal Proof of Evidence of
Dr John Underhill-Day (RSPB/4/F)

The Predicted Impacts of Aircraft Noise and Visual Disturbance on Bird
Species of Conservation Importance near to London Ashford Airport (Lydd)

In respect of:

Planning Application Reference: Y06/1647/SH (New Terminal Building)

Planning Application Reference: Y06/1648/SH (Runway Extension), relating to land
at London Ashford Airport, Lydd, Romney Marsh, Kent, TN29 9QL
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Appendix 1: Email to Dr John Underhill-Day, 21 April 2011.

Appendix 2: The locations, site information and bird counts for wildlife sites to the West of Heathrow Airport.

Appendix 3: LAMax Noise contour plans for airports discussed in the supplementary submissions.

Appendix 4: References.
1. INTRODUCTION

1.1 I have read Dr Underhill-Day’s Second Rebuttal Proof of Evidence in response to my “Supplementary Information to the Proof of Evidence of Dr Roy Armstrong” (SI). This is a summary response in writing to some of the contents of that Second Rebuttal Proof. It is submitted with the intention of saving time at the inquiry itself for the hearing of further oral evidence relating to ornithology. I can elaborate at the inquiry as required. For the avoidance of doubt, it is not intended to be exhaustive and I reserve the right to respond further as necessary at the inquiry itself.

1.2 My SI set out a review of a number of species. The SI came about because RSPB (through Dr Underhill-Day) was not willing to identify or specify individual species about which they had particular concerns. This avoidance of discussion on individual species has proved unhelpful and I do not regard it as justified. I note that in Dr Underhill-Day’s Second Rebuttal Proof, this same general attitude is maintained - save that he now includes a specific section on “reedbed birds” (which I return to below), Dr Underhill-Day does not refute any of the evidence already presented for individual species in my SI.

1.3 Following the direction and encouragement from the Inspector, I have attempted to discuss and clarify with Dr Underhill-Day which species (if any) the RSPB consider are of real concern so that the discussions could be focussed on actual species. I have no doubt that this attempt to narrow the issues is appropriate. Dr Underhill-Day's citing of species that are widely known by all professionals with any practical knowledge in this area to be highly tolerant of aviation noise e.g. Lapwing, is patently not credible or helpful. Unfortunately Dr Underhill-Day does not appear willing to engage in a process of species focused assessment – see my attempts to discuss at Appendix 1: Email dated 21 April 2011.
1.4 Dr Underhill-Day’s approach is instead to generally seek to rely on a created illusion that there is insufficient available literature to allow the impact of the proposed development to be assessed. In doing so, he conspicuously fails to accept that the impacts of disturbance and their context within Behavioural Ecology for birds have been reviewed in my Proof of Evidence and SI by reference to the available literature. In so far as it is necessary, building on my own professional and practical experience and that of Nigel Deacon, and in the absence of any evidence of any likely effects on any of the species from what is proposed, my Proof of Evidence and SI therefore provide this type of scientific assessment.

1.5 My Proof of Evidence looked at causes of disturbance and presented the well-known and accepted “risk-disturbance hypothesis”. This establishes that the general principles in terms of impacts from sources such as aviation are the same as disturbance caused by predators. The evidence I presented is therefore directly relevant. This principle is, at least, partially acknowledged and accepted by Dr Underhill-Day in section 4.1 of his Second Rebuttal Proof when considering reedbed birds when he refers to them “living in dense vegetation where they are unable to see potential predators (which from their perspective may include aircraft)”. Unfortunately, however, he does not apply these principles to his evidence.

1.6 I can also find no citation in Dr Underhill-Day’s Proof of Evidence, Rebuttal Proof of Evidence or Second Rebuttal Proof of Evidence that materially changes the understanding of aviation disturbance on birds since 1997 in the context of the previous planning permission for a runway development. At that time English Nature (now Natural England) and RSPB did not feel that a proposed runway development would have an impact on birds and therefore suggested they would not object on “these grounds” (CD12.39). In the absence of any material new information to demonstrate that the previous scientific understanding was wrong, I cannot see any evidence presented by Dr
Underhill-Day that raises any legitimate or justified concerns, especially
in view of his own accepted lack of knowledge on aviation ornithology
and his self-admitted dependence and reliance upon scientific literature
alone.

1.7 When it comes to an assessment of the scientific literature, in section
1.4 of his Second Rebuttal Proof, Dr Underhill-Day attempts to dismiss
the relevance of several evidence-based examples to which I referred.
He claims generally that a “lack of quantitative data on the birds noted
in the SP as being found on or close to airports makes it impossible to
comment in detail on the conclusions given. In many cases no
numbers, locations or other detail is given”. This claim is simply wrong.
I set out below why the examples are appropriate. Indeed, a number of
the points he seeks to make in this respect actually provide further
support for the points I make, and I illustrate this below where
appropriate.

2. IMPACTS OF ROAD TRAFFIC NOISE ON BREEDING BIRDS

2.1 It is concerning that Dr Underhill-Day continues to rely upon studies of
road traffic noise (e.g. 2.2, 3.5, 4.2 of his Second Rebuttal Proof) in
circumstances where it is clear and obvious that they are not materially
relevant to assessment of potential impacts of aviation noise as I have
explained already. As already stated in my Rebuttal Proof of Evidence
(3.4.2), the impact of road traffic noise that is identified in such studies
is through the masking effect on vocal communication, including song,
by the continuous state of road traffic noise. This is clearly not the
case with aviation noise which is very intermittent, allowing vocal
communication to take place between species for the vast majority of
daylight hours. In fact, most territorial singing takes place early in the
morning (the “dawn chorus”) around dawn and shortly after. During the
main bird breeding period from March to early July, the absence of
flying before 07.00hrs under the proposal would in fact allow long
periods of completely undisturbed vocal communication in any event.
Examples of the timing of the dawn chorus for this time of year (times given are for London and are from www.sunrisesunset.com):

**Time of Dawn**

<table>
<thead>
<tr>
<th>Date</th>
<th>Time</th>
</tr>
</thead>
<tbody>
<tr>
<td>1\textsuperscript{st} April</td>
<td>06.37</td>
</tr>
<tr>
<td>1\textsuperscript{st} May</td>
<td>05.34</td>
</tr>
<tr>
<td>1\textsuperscript{st} June</td>
<td>04.50</td>
</tr>
<tr>
<td>1\textsuperscript{st} July</td>
<td>04.47</td>
</tr>
</tbody>
</table>

2.2 In addition, civilian dawn (the time when, although the sun may not be above the horizon, it is light enough to for people to work outside without additional light), is around 30 minutes before the “true” dawn. In my experience, most species commence singing before “civilian” dawn, giving a completely undisturbed period of between an hour to almost 3 hours should the development proceed, as compared with the unconstrained position if the development does not proceed. However, it is simply wrong to infer any material effect anyway, let alone a disturbance impact, from aviation activity by reference to what was being assessed in the road noise studies.

2.3 Dr Underhill-Day’s conclusions in this respect, therefore, are simply wrong and inappropriate and involve a clear misreading of the literature, as well as its application to these proposals.

3 **RESPONSE TO PAPERS CITED BY DR UNDERHILL-DAY**

3.1 Dr Underhill-Day attempts to devalue the relevance or significance of the references I have cited. For example, in paragraph 3.3 he claims that Nisbet (2000) is a “single scientific paper”. However this paper is
in fact a commentary and review paper by the world’s foremost tern biologist, giving an assessment of the evidence regarding terns and other colonially nesting waterbirds in the way that I summarised. The paper itself is based on a review of 130 scientific papers all (except for a few chapters from scientific books) drawn from peer-reviewed scientific journals. Dr Underhill-Day’s attempts to discredit the paper are not justified.

3.2 Dr Underhill-Day also criticises the use of the research of Frid and Dill (2002), claiming “in no sense is this paper a review of aircraft disturbance.” However, this misrepresents the points I have made regarding the value of this research and its application - it is a review of responses to disturbance which, in line with well-established and accepted wisdom, analyses responses to perceived predation risk. This “risk-disturbance hypothesis” is thoroughly tested by analysing each of the predictions of this model against the available scientific literature. The literature reviewed strongly supports the hypothesis, firmly establishing that our knowledge of anti-predator behaviour is highly pertinent to studies of disturbance, as explained in section 3.15 of my Proof of Evidence. The authors themselves, while attempting a general review of disturbance studies, highlight the relevance to aviation disturbance where this is apparent.

3.3 Dr Underhill-Day also refers to Burger (1981) in stating that differences in disturbance levels can produce different responses by Herring Gulls. This is obviously correct; however this paper plainly does not support his general position and only serves to confirm my conclusions for two main reasons:

- First, the response to now-extinct commercial “supersonic” aircraft (Concorde, in particular) is manifestly not relevant to what is proposed by these applications. In Burger’s study, the noise levels produced by Concorde flying directly over the breeding colony were in the range of 101-116dB with a mean of 108.2dB. This is over
100 times louder than noise levels predicted for the SPA boundary at LAA and over 1,000 times louder than expected for any areas suitable for gull and tern nesting on the RSPB reserve.

- Secondly, the Burger paper also looked at the impact of noise associated with large “subsonic” aircraft, including Boeing 747s, overflying the gull colony. These produced no difference in the behaviour of the gulls. Again, even then the sound produced by these aircraft was significantly louder (Mean = 91.8dB with a range of 88-101dB) than the levels predicted for any area of the SPA and were very significantly louder than levels predicted for suitable gull and tern nesting areas (<60dB).

3.4 It will be recalled that I was asked generally in cross-examination by RSPB about what were said to be the results of the Burger (1981) paper, in particular that in response to certain aircraft noise, gulls “panicked” and fought resulting in eggs being smashed. I noted at the time that effects of this kind were in fact only said to be as a result of Concorde departures, although I was not taken to the paper itself. The paper confirms this. I stated at the time that, as a gull expert who has monitored thousands of nests and studied the behaviour of this species for my PhD, I thought this result to be highly unlikely in respect of aviation noise generally. Again, this is borne out by the Burger paper. In any event, it is clear to me that Professor Burger’s paper has an inherent fundamental methodological flaw that would make it impossible to be used legitimately to suggest that there is a negative impact on hatching success in relation to aircraft noise. She referred to the difference between egg loss in the disturbed area (as a result of damage during fights with close neighbours) and isolated pairs in an undisturbed area. Clearly if the mechanism for egg loss was fights between near neighbours, using isolated nests as a control would be wholly unacceptable and unscientific. In addition, based on my own extensive experience of monitoring nests at Herring Gull colonies, the
egg losses she reports are not remarkable and are far lower than some where no significant disturbance occurs at all.

3.5 Dr Underhill-Day also refers to a paper by Brown (1990) to make the claim that Crested Terns seemed tolerant of aircraft noise but that Bridled Terns did not (section 3.6). In fact, this is only a reference to a single sentence at the end of Brown’s discussion, the author himself does not present any data/methods etc. for this, and, as the “experiment” would appear to have been performed at the same time as the Crested Tern study, it is questionable why the data for this species were not included in the paper if the data demonstrated this. Indeed, the data do not appear to have been published elsewhere at a later date, suggesting that this study was not to the standard required for publication. There is also, in any event, a basic methodological problem in seeking to apply the results of this study to any assessment of aircraft in the real world. Brown presented the terns with aircraft noise through speakers placed in a colony. The birds would not be able to identify the source of noise (e.g. a visual stimulus) and therefore inevitably responded in a different way than might be expected towards a known source as “most of the colony attempted to locate or evaluate the stimulus”. Given that the response to disturbance is analogous to responses to predators (as I have explained and has been clearly demonstrated and I believe Dr Underhill-Day has accepted as I have cited), then it is obvious that not being able to locate the predator or potential predator or to recognise or confirm with visual evidence that it is not a predator, but still believing it to be present, is likely to produce a far stronger and different response than visually locating a source of noise and being able to identify it as a distant object, even if it is still perceived as a potential predator. Indeed, this is one of the reasons birds and other animals issue alarm calls.

3.6 It should be noted anyway that in spite of the “invisible” nature of the potential predator that would have made a response more likely (for the
reasons I have explained), it can be seen that the noise level at which Crested Terns responded with startle or escape responses was, in fact, between 85-90dB. This is far louder than any levels expected for the potential tern nesting areas at RSPB Dungeness (<60dB), which in turn are actually quieter than the ambient noise levels recorded in the Brown’s study (60-75dB). Brown’s study also makes it clear that the study was to investigate the responses of birds that had not become habituated to aircraft noise. I have already demonstrated elsewhere that habituation would be expected with this group (Nisbet 2000) anyway.

3.7 Dr Underhill-Day also seeks to challenge the use of a paper cited in Ruddock and Whitfield (2007) regarding the sensitivity of Marsh Harriers. He refers to a finding that disturbed pairs produced “more malnourished” chicks than did undisturbed pairs (although the paper itself does not make clear whether the undisturbed chicks were malnourished). This again misrepresents the purpose and relevance of my citation of the paper and the point that it demonstrates. The reason for making reference to it was to rebut Dr Underhill-Day’s clearly incorrect assertion that Marsh Harriers are “particularly intolerant of human disturbance” (RSPB/4/A, 10.54). That assertion is clearly incorrect in light of the study from Ruddock and Whitfield because if this had been the case, then the Marsh Harriers would be expected to leave this area of highly intrusive disturbance. However, it is also important to note that the disturbance stimulus in respect of the disturbed pairs producing more malnourished chicks was very different and far more significant than any effect from aviation noise of the type in issue here. The disturbance examined was direct human disturbance at the nest for a species that is in fact persecuted by humans. Such disturbance is patently far more significant than that associated with aircraft noise of the type in issue, as reflected in Dr Underhill-Day’s references to the impact of aviation disturbance on birds of prey. I have dealt with this issue already in more detail in my written and oral evidence.
4. COMPARISONS BETWEEN LAA AND OTHER UK AIRPORTS

4.1 In section 3.8 of his Second Rebuttal Proof, Dr Underhill-Day states that none of the noise levels at cited airports have been measured. However, I believe he accepts that the SI refers to examples of situations where the noise levels will clearly be far higher and more frequent than those identified as likely to occur as a result of this development (I do not believe that can be controversial). Nonetheless, to further illustrate the point, I have included additional noise level diagrams prepared by Mr Perkins both in consequence of the proposed development (Figures 1-3) and for some of the other sites referred to in the SI (Figures 4-7). The noise level plots for LAA have been redrawn to simplify the information by showing the 79dB and 85dB L_Amax contours, as 80dB and 85dB were previously used as threshold levels above which disturbance effects might occur (see e.g. CD1.23i).

4.2 In fact, I believe that the 80dB threshold is too low and conservative. It was only used because of a reference to potential disturbance effects in Harlequin Ducks, an arctic species that occurs no closer than Iceland (Goudie & Jones, 2004). However, this threshold is inappropriately low as the authors acknowledge that in response to “noise generated by low-level passes (30-100m)” “direct behavioural responses to military jet overflights were of short duration and were unlikely to affect critical behaviours such as feeding and resting in the overall time-activity budgets of breeding pairs.” The paper describes a disturbance “effect”, but not a disturbance “impact”. The additional noise level diagrams for the proposed development clearly show that the vast majority of the RSPB reserve and SPA at LAA lie outside of the 85dB noise contour. The three flightpaths plotted result in an exposure to noise levels >85dB in only two very small areas at the North-West extremity of the RSPB reserve. These areas lie well away from the main reedbeds which Dr Underhill-Day (Second Rebuttal Proof of Evidence par 4.1) states: “currently extend to the northern
edge of Hookers Pits (see Appendix 1 to RSPB/1/C Mr Gomes’ Proof of Evidence, Map 7)”. Figures 1-3 show that these areas also fall outside of the 82dB contour and mostly outside of the 79dB contour.

4.3 I note that of the 29 UK airfields referred to by me in my SI, Dr Underhill-Day in fact only challenges the use of four of them on the grounds of level of use (see Second Rebuttal Proof paragraph 3.12) and five on the basis of location of areas relative to an airfield (see RSPB/4/F paragraph 3.9). I will deal with these apparent criticisms individually, as the criticisms vary from irrelevant (in view of other information available for the species involved) to fundamentally incorrect (e.g. paragraph 3.9.3 of his Second Rebuttal Proof where he gives information on the wrong balancing ponds, demonstrating what is already clear, namely that he has little or no knowledge of the individual sites under discussion).

4.4 The “levels of use” criticism he advances is confined to:

- Sumburgh Airport, Shetland
- Stornaway Airport, Isle of Lewis
- Carlisle Airport
- Fairford USAF airfield.

4.5 I have been to and/or worked at all of these airports, except Fairford USAF (which is obviously a military base).

4.6 Sumburgh Airport was only identified in the SI as an example of an airfield where Redshank breed. As this species is acknowledged as breeding on “many Scottish airfields in loose colonies” (CAP680: CD 16.18), and it is known to use areas directly under flightpaths such as is the case at Belfast City Airport, the further reference to this site is clearly not fundamental. But the point that this species is highly tolerant to aviation noise is clearly correct by reference to this Airport as well as others.
4.7 Stornaway Airport was referred to in the SI as an example of a breeding site for Redshank and also for Little Tern, Arctic Tern and Short-eared Owl. The tolerance of terns to aviation noise has already been firmly established both through my own experience, but also through reference to Nisbet (2000) and from the presence of breeding terns on other airfields in the UK. Further, it should be noted that terns have largely deserted RSPB Dungeness for Rye Harbour. The potential nesting areas at RSPB Dungeness are in areas where the predicted noise levels will not only be well below the levels shown to be tolerated by nesting terns (Brown 1990), but also well below the ambient noise levels recorded in that study. The absence of any material impacts on Short-eared Owl is fully covered in the SI. In fact both Stornaway and Sumburgh Airports offer scheduled flights (and at Stornaway this did include jets between 2002 and 2006, see http://tinyurl.com/6y3wjdg) and they ought to have been employing bird control measures similar to those required at LAA. The presence of these species at those airports therefore reflects my own experience of their tolerance not only to aviation disturbance, but also to the bird control measures required at a fully-functioning commercial airport that offers scheduled flights.

4.8 Carlisle Airport was referred to in the SI as it also has a breeding population of Redshank but, additionally, it also has local wintering populations of Pink-footed Goose and Whooper Swan (both associated with the Upper Solway Flats and Marshes SPA). Following several years of monitoring this site, I included these as additional examples as I have repeatedly observed both species feeding, undisturbed, very close to the airfield. For Pink-footed Goose, I have observed several thousand individuals feeding within 300m of the end of the runway. I have also observed Whooper Swans feeding, apparently undisturbed, around 500m from the end of the runway - and they used the same fields for many days in a row.
4.9 Fairford USAF base was referred to in the SI because the airfield lies close to Cotswolds Water Park, a site that regularly hosts wintering Smew. As to Dr Underhill-Day’s comments about Heathrow, I am aware that the reservoirs lying below the flightpaths for Heathrow itself support populations of this species and, in view of the size and frequency of aircraft using this site, this is, in fact, another clear example of Smew’s tolerance to aviation noise (see Figure 4a for the noise contours in the vicinity of the waterbodies). The reservoirs also support important populations of Shoveler, a species that features in the SPA designation. Further information about the reservoirs is presented in Appendix 3 to this response. Although the waterbodies do not fall within the higher level noise contours, a significant area of Wraysbury reservoir falls within the 79dB LAmax contour for Boeing 747 departures on runway 27L, and a small area falls within the 82dB contour. These conditions are very similar to, or louder than, those predicted for Dungeness RSPB reserve (see Appendix 3, Figures 1-3).

4.10 In paragraph 3.9 of his Second Rebuttal Proof, Dr Underhill-Day states that the examples chosen are “off to the side of the airport” and that “it would be expected that noise would be attenuated and lines of vision obstructed”. I note, first of all, in so far as this point has any relevance, it is clear that it is also the situation for Dungeness RSPB. Moreover, even if reedbeds did not attenuate noise levels to the same extent as other features (and leaving aside the actual noise levels anyway), it is clear that the “lines of vision” are likely to be even more obstructed in such cases, especially for species associated with dense reedbeds. This appears to be accepted in paragraph 4.1 of the Second Rebuttal Proof where Dr Underhill-Day states: “They are all species living in dense vegetation where they are unable to see potential predators (which from their perspective may include aircraft)”. In fact, it should be noted that, at ground level, the line of sight between LAA and RSPB Dungeness is almost entirely screened by scrub and hedgerows.
Turning in particular to the 5 sites criticised for being “off to the side”, I deal with them in turn:

4.11 Belfast Lough RSPB Reserve (Second Rebuttal Proof, paragraph 3.9.1) - this site was identified in the SI for its large tern colonies. The absence of impacts on terns has already been comprehensively dealt with, and it does not rely or depend upon the use of this site. But it should be noted anyway that the nesting area at Belfast Lough is far closer to the runways than the potentially suitable nesting areas at RSPB Dungeness – the comparison is therefore still a valid and proper one and Dr Underhill-Day’s criticism is not justified. Noise contour plots for Belfast Airport are presented in Figures 5a and 5b. This clearly supports the claims in my SI for the tolerance of a wide range of species known to occur in Victoria Park, the “lagoon” and the “wader roost” (Figures 2 and 3 in my SI).

4.12 Stansted (Second Rebuttal Proof paragraph 3.9.2) - the balancing ponds are around 1.5km from the main runway (although considerably closer to many busy parts of the airports and the A120 dual carriageway). The same distances or more are found for most of the ponds at RSPB Dungeness. This airport was referred to in the context of demonstrating the position of Gadwall and Coot (the latter of which has already been mentioned as nesting close to the runways at Manchester Airport but to which Dr Underhill-Day makes no reference). Lapwings at Stansted (which I have, in any event, already established are very tolerant to aviation noise), nest at the location marked with a red diamond in the image below:
4.13 Heathrow (Second Rebuttal Proof – paragraph 3.9.3). Dr Underhill-Day here makes the mistake of referring to reservoirs that exist around 1.5km to the east of the runways. He fails to refer correctly to the balancing pond near the northern runway (now netted, and marked on Figure 4b) to which I was making reference in the SI nor the large SSSI waterbodies and SPA areas to the west of the runway lying directly under the flightpath. It is clear that his criticism is therefore based upon a basic and critical error in his understanding of the location of the relevant waterbodies to this airport.

4.14 Coventry (Second Rebuttal Proof para 3.9.4) - this airport was only referred to in the context of Shoveler, a species which is known to occur in any event at Wraysbury Reservoir close to Heathrow Airport (Appendix 3, figure 4a).

4.15 Shannon (Second Rebuttal Proof, para 3.9.5) - this airport was included as yet another example of a site that supports Shoveler (see above), as well as Little Grebe. Little Grebe also nest and winter under
the flightpath from Belfast City Airport in Victoria Park (see Appendix 3, Figures 5a and 5b for noise contours), and are therefore clearly very highly tolerant of aviation noise. Again, the lagoon at Shannon Airport lies closer to the runway than do most of the waterbodies at RSPB Dungeness. The screening “woodland” suggested by Dr Underhill-Day, is, in fact, open plantings of deciduous trees (hence leafless in winter) on a golf course.

4.16 Newcastle (Second Rebuttal Proof, para 3.11) - in Table 1 of his Second Rebuttal Proof, Dr Underhill-Day presents selected counts for Big Waters Nature Reserve to suggest that there is a decline in four species at that site. However, he does not present counts for the other four species referred to in my SI or indeed any other species. In any event, Dr Underhill-Day is clearly entirely speculating as to the causes for these particular species’ decline. For example, since the designation of Big Water SSSI, there has been re-flooding of nearby Prestwick Carr. This has produced improved conditions for wildfowl which may well have resulted in birds that previously used Big Water relocating to the re-flooded areas in the same way that breeding terns and gulls have forsaken the Dungeness RSPB reserve in favour of the habitats created at Rye Bay. The Prestwick Carr floods do appear to offer ideal conditions for several species of wildfowl, some of which are known to already occur (for example see http://wwwpcfblogcom.blogspot.com/2010/11/carr-acter.html). Dr Underhill-Day’s assertions are not supported.

5. STATISTICAL CONSIDERATIONS

5.1 Dr Underhill-Day maintains (at paragraph 3.17 of his Second Rebuttal Proof) that some species (though without identifying which) that are listed in the SPA designation are relevant, saying they have been identified as being of importance by “experts”. I do not agree with this approach and have previously dealt with it (see SI sections 3.14 and 3.17 for examples). This is a similar naïve approach to statistics to the
one that led to the misuse of WeBS data previously by the BTO and, in turn, by Dr Underhill-Day. For clarification, the BTO have now changed the way in which they calculate the WeBS totals for the Dungeness to Rye Bay SPA: “Since last year, individual sites on Dungeness peninsula (including, for example, Dungeness RSPB Reserve, Scotney Pits and Rye Harbour) have been combined. Data from this recording area are now presented as ‘Dungeness and Rye Bay’. 
(http://www.bto.org/sites/default/files/u18/downloads/publications/WITU K%20METHODS_2.pdf). This clearly proves the point I made repeatedly about the SPA counts being inflated and incorrectly assessed.

6. CLIMATE CHANGE

6.1 At 3.18 of the Second Rebuttal Proof, Dr Underhill-Day refers to a claimed inconsistency in my evidence over the impact of climate change, stating that the SI “claims that some populations will increase due to climate change (the reverse argument was used in Dr Armstrong’s rebuttal section 2.2).” Dr Underhill-Day is misrepresenting what was written in my Rebuttal Proof of Evidence which, in fact, clearly states: “Climate change is not all negative and many species are forecast to colonise the UK or increase in range and/or population size. This includes some species that are currently restricted/rare and of special importance around the airport such as Marsh Harrier and Purple Heron” (2.2.6). There is no inconsistency in my evidence in this respect.

6.2 The key point about climate change remains that some species will be lost from the SPA for reasons beyond control of site managers. The suite of species that are likely to replace those species that will probably be lost are not of the same level of conservation significance. Of those species listed as species expected to colonise (CD12.34), the habitat within the SPA and pSPA is only likely to be significant for the three heron species listed and possibly Great Reed Warbler. This does
not even approach the conservation “value” of the suite of species expected to be lost.

7. **THE IMPACT OF BIRD CONTROL MEASURES ON SPA INTEREST FEATURES**

7.1 In section 3.20 of his Second Rebuttal Proof, Dr Underhill-Day refers to bird control measures and states that in response to “birds which have become habituated to aircraft disturbance”, “one would expect measures to be taken by airport authorities to deliberately disturb them or to change the habitat to make it less attractive in order to move them elsewhere”. Whilst clearly accepting here that habituation is likely to occur, again, no specific species of actual concern are mentioned by him. I assume that this point is aimed at those species which could be potentially hazardous in terms of bird strike risk, and not meant to infer all birds “which have become habituated,” in which case the species he might be suggesting could include the following (which I deal with in turn):

- **White-fronted Goose** – Mr. Gomes has already accepted (correctly) in cross-examination that this species is not affected by aircraft at LAA as it normally occurs around Cheyne Court, commuting to fields West of Lydd and does not usually cross the airport area.
- **Bewick’s swan** – again, Mr. Gomes has also already accepted (in Cross Examination) this species will not be affected by the development (see my SI section 3.24).
- **Greylag Goose** – these are from a feral population of no conservation significance and likely to have a negative impact on species of conservation significance (as highlighted in my Proof of Evidence).
- **Golden Plover and Lapwing** – I have already explained that there are many other areas are available for such species, and in any event the habit of feeding mostly at night means that day-time disturbance would have no impact (as discussed in my SI).
7.2 As Dr Underhill-Day should be aware, any of these species presenting an unacceptable birdstrike risk currently, or in the future regardless of the development proposals, would already require remedial action under existing regulatory conditions. As with his other attempted criticisms, I believe it is clear, from evidence already in front of the Inquiry, that Dr Underhill-Day has no legitimate or credible point to make on any of these species, or on this issue in general.

8. REEDBED BIRDS

8.1 I am surprised at the reported suggestion that neither the current nor the previous warden at Leighton Moss RSPB reserve have ever seen light aircraft flying over the reserve (para 4.3.1, Dr Underhill-Day’s Second Rebuttal Proof) as the area (including Morecambe Bay) often attracts light aircraft (pers.obs.). To clarify the circumstances, during my fortunate observation, I was not, in fact, alone as Dr Underhill-Day appears to suggest, but in fact had a group of around 25 undergraduates studying for their Animal Conservation Science degrees. Most of these had suitable optical equipment and several of them are accomplished birdwatchers. All of them had been guided on what to look for when trying to observe Bitterns. Several other birdwatchers were also in the hide at the time and Bitterns had been observed in the 15 minutes previously. I therefore clearly disagree with Dr Underhill-Day’s assertion that “If a Bittern did fly up it is more likely than not that Dr Armstrong would not have seen it” (para 4.3.2, Dr Underhill-Day’s Second Rebuttal Proof).

8.2 In addition, the point that I made was that if Bitterns were known to fly in response to aircraft disturbance, this would be a phenomenon that was not a site-specific one. Bitterns winter at many sites throughout the UK including breeding areas in extensive reedbeds, but also in smaller reedbeds and other areas of wetland edge. If Bitterns became more obvious by e.g. flying in response to aircraft, I am sure that
birdwatchers would have noticed this and would use it to enhance their chances of finding this secretive species.

8.3 Dr Underhill-Day states that Bitterns do not nest in close proximity to airports anywhere in the UK (para 4.4, Dr Underhill-Day’s Second Rebuttal Proof). This is not surprising as its exclusive habitat (suitable extensive reedbeds) also does not occur in close proximity to UK airports. I take it from Dr Underhill-Day’s comment that the Bitterns nesting at RSPB Dungeness are not in close proximity to LAA. He also states (para 4.7, Dr Underhill Day’s Second Rebuttal Proof) that the staff at RSPB Valley Lakes reserve believe that Bitterns did not “summer or breed” there in 2010. However, at least one of the RSPB’s “community” found them there, see: (http://www.rspb.org.uk/community/forums/p/13660/95794.aspx) in late April 2010, well into the breeding season (http://www.rspb.org.uk/wildlife/birdguide/name/b/bittern/nesting.aspx), The observer notes “the RSPB Valley Wetlands Reserve in Anglesey, which is directly opposite the RAF base from where a different species of bird is frequently seen and heard on low-flying practice”. The fact that this species exists and has bred at a reedbed that borders a very busy and very noisy military airport clearly supports the view that this species is tolerant to aircraft noise.

8.4 In section 4.5 of his Second Rebuttal Proof, Dr. Underhill-Day accepts that Nisbet’s discussion focuses on colonially nesting herons. He claims that this does not apply to Purple Heron as this is a “generally solitary species”. However, apart from the fact that breeding birds are rarely solitary, this species is in fact highly colonial e.g. Moser (1986) who states: “Purple Herons occur mainly in these freshwater areas, where they breed in large colonies (max. 380 pairs) in reedbeds, often in association with the Grey Heron Ardea cinerea.” Dr Underhill-Day seems to have, in fact, little knowledge of this species, whilst the comments of Dr Nisbet are clearly highly pertinent for discussion of this species.
8.5 Whilst there are no examples of Purple Heron nesting close to airports in the UK (as the Dungeness nesting in 2010 was the first, highly welcome, occurrence in the UK), this species is clearly tolerant to disturbance. Bob Swann, a highly-respected birdwatcher, has observed that this species occurs close to the very busy airport at Dalaman, Turkey. In his “birdblog” available at http://www.birdtours.co.uk/tripreports/turkey/turkey12/turk-apr-03.htm, he records:

13th April, 2003.

“On the flight in I had noted a large area of wetland by the airport at Dalaman, so we decided to have a look at it. Just before the airport a road branches off to the left and follows the airport perimeter fence towards the coast. Between the fence and runways were marshy areas with 35+ Squacco Heron, 55+ Little Egret, 11 Grey Heron, 2 Purple Heron, 5 Great White Egret, 40+ Glossy Ibis and at the edge of a wee pool superb views of a feeding Great Bittern. Waders included 4 Greenshank, 35+ Wood Sandpiper, 2 Ruff and a few Little Stint.”

8.6 This confirms my earlier point, and the view of Dr Nisbet, that Purple Heron and other members of the heron family (including Bittern – referred to here as “Great Bittern”) are highly tolerant of the predictable and non-harmful disturbance associated with aviation. Further, it provides further demonstration that “waders” (including Ruff and Little Stint) as well as the herons are clearly comfortable feeding within the airfield’s boundaries. The presence of these species actually on an airport that handled 3.8 million passengers in 2010 with a variety of aircraft (including Airbus 319, 320, 321 and Boeing 737, 757 and 767) demonstrates that these species are clearly highly tolerant to aircraft noise. As can be clearly seen from the noise contour maps for departures on runway 19 (Figure 8a) and runway 01 (Figure 8b), the noise levels within the perimeter fence of the airfield are relatively high
(>88dB). From the description, it would appear that the fence mentioned is the Eastern perimeter fence running alongside a small road (which can be clearly seen as a pale line on both of the figures). This area all falls within the 85dB area and as the birds were observed between the fence and the runway, the noise levels they experienced must have been far higher.

8.7 In para 4.8 of his Second Rebuttal Proof, Dr Underhill-Day asserts that the tolerance of reedbed species to aviation noise is unsupported by any evidence. He also states that the species being discussed are highly secretive and are rarely seen. However, as I would hope he is fully aware, these species are highly vocal and while they may be difficult to see, they are very easy to detect as, especially, Cetti’s Warblers are very vocal and hard to miss from their calls. The fact that both Water Rail and Cetti’s Warbler both breed on the airfield (along with Reed Warbler, Sedge Warbler and many other species) in an area that already experiences noise levels in excess of 88dB (see below diagram marked “Pond A and reed” in the legend), and Water Rail have been proven to breed on Warton airfield, is in fact strong evidence of their tolerance to aviation.

Redrawn from: FIGURE NV22 in LAA/5/C – Appendices to the Proof of Evidence of Mr Richard Perkins. LAMax Noise Contours, Gulfstream V Series Departure Runway 21.
8.8 The LAA noise contour maps clearly show that the SPA will not be affected by noise levels above 85dB and it cannot be reasonably expected that any of the SPA species will be impacted by the predicted noise levels.

9. THE IMPACT OF BIRDSTRIKE MORTALITY ON HEN HARRIERS

9.1 An issue appears to have been raised by Ms Dear in cross examination as to the potential effects of birdstrike on Hen Harrier if it were to occur. I do not consider the potential for birdstrikes with this, or any other rare species, to be a legitimate cause for concern in terms of their conservation, and this view is supported by the birdstrike statistics for the UK nationally (see below). Ms Dear expressed concern over Hen Harriers in particular, as these winter on the RSPB reserve and surrounding areas. Although this species occurs at a number of airports, particularly in in the North, along the East coast and on the Isle of Man, there are only two birdstrike records in the last thirty years involving this species and these were both at Isle of Man Airport, Ronaldsway – on an island that is a stronghold of this species’ breeding population and home to the largest Hen Harrier winter roost in Europe (http://tinyurl.com/5wer7zj).

9.2 The reason that Hen Harriers remain an uncommon breeding species in the UK is clearly persecution by man, in particular where gamebirds are reared for shooting. This is highlighted in “A future for the Hen Harrier in England?” (Natural England 2008) in which they summarise: “Of all birds of prey, the Hen Harrier is the most heavily persecuted in relation to population size in the UK. The significance of persecution for Hen Harrier populations is well-established: populations in Scotland have been proven to be limited by persecution (Redpath & Thirgood 1997); and models suggest that in the absence of persecution, numbers in Scotland would rapidly recover (Etheridge et al. 1997). Potts (1998) estimated that in the absence of persecution the English uplands would support 232 territorial females”. There are currently
around 20 nesting attempts per year in England, many of which are unsuccessful as a result of persecution.

9.3 As there are no reasons for any other rare species associated with Dungeness to commute across the airfield, I do not believe that any other species are under threat from birdstrike mortality and I do not believe there to be any material risk to Hen Harrier from birdstrike.

10. CONCLUSION

10.1 Nothing in Dr Underhill-Day’s Second Rebuttal Proof provides any credible or justifiable challenge to my evidence on the absence of effects from the proposed development on bird populations associated with the SPA, pSPA and SSSIs. The key conclusions are maintained for the three main groups using the area:

**Wintering birds**

10.2 There would be no material effects on these birds from the proposals. In any event, there are considerable areas of suitable habitat for all species to relocate within the site even if any effect was to occur and sufficient time will be available for all niches to be exploited fully. Further, there are no key blocks of habitat within the 85dB noise contour that would, even if any disturbance from aviation were to occur which would not otherwise have occurred, would be made unavailable to bird populations.

**Passage birds**

10.3 I repeat the same general points as for wintering birds set out above. In addition, quite apart from my disagreement that some of the species are in fact significant during passage, birds are even less dependent on individual areas and, as already stated in my Proof of Evidence (3.11), “populations with weak density-dependence can experience extensive redistributions with minimal impacts on population size” (Gill 2007).
Thus even if any disturbance effect were to occur, there would be no adverse impact on any species of any of the designated or proposed designated sites

**Breeding birds**

10.4 Again, I repeat the same key points in respect of wintering and passage birds. In addition, I note that no key breeding areas (particularly reedbeds) occur within the 85dB noise contours and therefore even if any disturbance effects were to occur from noise at this level, it would have no material impact on breeding birds anyway. The breeding species listed in the SPA citation have largely vacated the RSPB Dungeness reserve and will therefore not be affected in any way.

10.5 The review presented in my SI and the earlier evidence as to the absence of impacts regarding disturbance to birds clearly demonstrate that no species of conservation significance is likely be materially affected as a result of the proposed development and even if any effects were to occur, there would be no adverse effects on the integrity of any of the proposed sites. I consider that strong and compelling evidence has been provided, both from my own practical experience and knowledge, and also through the literature, to support my conclusions for each species and group, and to enable the likely effects of the development to be fully assessed and to demonstrate no negative impact on the SPA, pSPA and SSSI can reasonably be expected.
11. REFERENCES


