

2-Way Memo

Subject:

N ihoa Trip- May- Mahi

DATE OF MESSAGE
DATE OF REPLY
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To:



John Sincock

5027-101

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USE BRIEF, INFORMAL LANGUAGE

Roger Clapp is reporting on Nihoa and he is interested in whether or not there were any seals using Nihoa during your stay. Seems like we did see some but I didn't record anything. What was the max. number you saw and did it appear like the same seals were there every day????

DLO

From:

Wildlife Administrator
 Box 698
 Kailua, Hawaii 96734



UNITED STATES
DEPARTMENT OF THE INTERIOR
FISH AND WILDLIFE SERVICE
BUREAU OF SPORT FISHERIES AND WILDLIFE
Post Office Box 698
Kailua, Hawaii 96734
January 5, 1969

Mr. Roger Clapp
Pacific Project
Smithsonian Institution
Washington, D.C. 20560

Dear Roger:

Enclosed are some belts with answers to most of the questions which you asked in your letter of November 19. I was not quite sure in several instances as to what you were asking, but in the event that I misinterpreted the question, ask again.

Also enclosed are copies of the trip reports for February, 1969 to Pearl and Hermes Reef and the August-September, 1969 trip. We had to content ourselves with only a day at Laysan because that is all the Coast Guard would give us. So we practically galloped around that island, once around the outside beach then around the lagoon.

We saw sharks every time we dove at French Frigate Shoals, Laysan ($\frac{1}{2}$ hour), and Pearl and Hermes Reef. The big tiger we saw at Trig Island changed our plans to dive near there that day.

Jane's little note about getting space to receive gear was very intriguing. We would like to hear more about that.

Hauoli Makahiki Hou (Happy New Year) to all youse people there.

Best regards,

Eugene Kridler
Wildlife Administrator

Enclosures

To: Roger B. Clapp
From: Eugene Kridler

January 1969

Well, hello there Roger. Merry Christmas and a Happy New Year to you and all the rest of the people there. I've decided to try and get these comments to you via recorded methods. It seems there is one continual interruption after another and I figured maybe this is the best way to get it to you.

Starting with the Black-footed Albatross - under status - present and breeding from at least December through June, probably absent from August through October. From observations at Midway and everywhere else there in the Leeward Islands Albatross beginning to appear at sometime around late October to early November so I would say that they probably like the rest of the birds in the Chain, this species in the Chain begin nesting at least in early November, so I don't see the basis for saying probably absent from August through October. On the basis of our observations I would say absent from July through at least the middle of October.

Getting into populations - I don't go along with your statement "that recent estimates suggest that the breeding population has decreased since 1915." Going all the way through this whole thing on various species this chap Munter seems to be extremely high in his estimates on just about everything and I am not familiar with his paper and I very strongly suspect the accuracy of his estimates. Then you go on to say that the 60 young seen by Wetmore in June 1923 were more than were present on any subsequent visit. The only figure for breeding birds which approaches that estimate was in the 1968 observation of 59 young. That's only a difference of one bird and to me this is really splitting hairs. The 60 young seen by Wetmore in June 1923 was almost the same as the 1968 observations so then how could we come up with the statement that estimates suggest that the breeding population has decreased since 1915. Then you say however that this observation was made much earlier in the year than Wetmore's and it seems likely that by June chick mortality would have decreased the number present. Well on the basis of all my trips to Nihoa, and that number is about 10 now as I recall, I think that there's very little chick mortality at all on these. We get up there in July and then again in August, you find no bones, you find no old bands, and we've been banding birds up there. Now this is not like Laysan and Lisianski and even Pearl and Hermes Reef where you have quite a bit of mortality. I think there's very very little mortality and until someone can come along and show me that there is considerable mortality I don't think that a statement like this belongs in this particular paper. There's bound to be a few that may be deserted but we haven't found it so. I was there in July of 1964 then again in July and early August of 1966, and in late August of 1968 and found no evidence of mortality. This isn't saying of course that a few birds might not have died, but I don't think that the mortality there amounts to a hill of beans or is significant at all and I feel that your statement that

It seems likely that by June chick mortality would have decreased the number present is not dealing with facts.

Then another thing I'd also like to point out now. Wetmore's figures in June of 1923, then we have this Munter in 1915. On the basis of the work which Chan Robbins was doing on Midway for a number of years and my association with Chan for several winters there particularly on the Albatross program certainly shows that breeding populations vary considerably from year to year. A one shot observation may be average, it could be above average, and it could be way below average, so unless you have a number of observations you cannot draw valid conclusions as to whether a population is going up or down. I suspect very strongly on the basis of my observations that the population is relatively stable and if there is any variation up or down it's purely the natural phenomenon and has nothing to do with the steady decrease or steady increase. Then the estimates since 1957 do not indicate any recent overall change in population size and suggest that about 100 birds breed on the island yearly. Here again, you are basing statements on a few years and drawing some rather valid conclusions on some rather flimsy evidence.

Just reading the paper it would be my impression that what you are saying is that there has been a large decrease in young or in the number of breeding birds there say in the last 40 years. The population is apparently stabilized since about 1957 to about 100 birds. I'm also curious about how Munter estimates? Did he actually count the chicks like we have? Did he visit the area, or just what is he basing his estimate on? Now, we've been there since every March except March of 1966; we've been there in 1964, 65, 67, and 1968; certainly more times than anybody else at that time of the year and we haven't found any Black-footed Albatross anywhere but right on the top of North Peak. In some Marchs we've covered the island rather thoroughly and if any were present, undoubtedly we would have seen them. It is very likely, of course, that we might have missed one or two, but this I think is not the case. (3) (5)

Getting down to this last paragraph there about the young present in March can be quite variable - this could indicate that the number of breeding birds varies from year to year or that the mortality early in the nesting season may be considerably greater in some years. I don't think mortality figures in here at all. I think it's pretty much a case of a variation in the number of breeding birds.

Now in your last sentence on ecology - breeding habitat - all observers who noted the location of the Black-footed Albatross nests stated or indicated that the species are confined to areas of little vegetation on the lower ^{Miller} plateaux. It wasn't a case of indication, this was the fact, so I would strike out the word or indicated. (8)

(9) Now on banding - correct me if this is strictly going to be a Pacific Project paper or is it supposed to be co-authored as was the impression that I was given. You say that no Black-footed Albatross were banded on Nihoa by Pacific Project. This is probably correct, now if we're going to include banding by anybody over here, and I do think it belongs in here on the basis of our work, the following information should be inserted.

~~On~~ March 6, 1964 there were 50 chicks banded; March 13, 1965 there were 35 chicks banded; March 8, 1968 we banded 60 chicks. Of course on the basis of the work done on Midway it is a little too early to state whether we're going to have returns or not but I see no reason why we shouldn't have any returns. The '64 chicks should be back there, a few of them should be back in '69 and certainly some there in 1970. The thing that I am curious about is what had happened to the increment, are they going perhaps to Necker to breed, is the breeding habitat on Nihoa that limited, they cannot accommodate more than 60-70 birds or whatever has taken place.

Now getting back to that 1964 banding of 50 chicks - there were more present I suspect about 10 or 15 more chicks present but we did not have enough bands and all we had were 50 bands and these were all used up. So the fact that a number of chicks were banded does not necessarily indicate that that was the total amount of chicks there.

Now we get over to your table about your observations - the population estimate on 18 March, 1915 by Munter of 500 birds. I think is highly suspect. I don't know about Richardson's observations in '53 and '54. I suspect very strongly that he didn't get up there. He said none seen on a partial survey. He may not very well have gotten up there or couldn't recognize Black-footed chicks when he saw them as distinguished from Laysan Albatross chicks. There are a number of other ornithologists which I've been associated with who could not distinguish the difference between chicks until it was pointed out to them.

Now on the '64 - now all these trips subsequent from 1964 all the way to the end of 1968 you are just citing the Pacific Project as the authority. I'm curious as to just why. Actually this information was gathered primarily by Bureau personnel who made up the bulk of our party. We made room in our party for Smithsonian personnel either one or two persons. Most of the data was gathered by us and pooled and as I recall in 1964 either Amerson or Wislocki ever got over to the Miller Peak area. They went up Tanager Peak looking for Red-tailed Tropicbirds to band and pretty much left the censusing of the island seabird populations to us. We then gave them this information as we did to Bob Fleet in September '64, Doug Hackman in March of '67 and you in March of '68. Yet your reference and your remarks are all Pacific Project people. I don't consider this kosher at all. The reference or the citations should be Pacific Project and US Bureau of Sport Fisheries and Wildlife.

Now in March of 1964 under remarks there were not 50 young present. That should be changed to 50 chicks banded. And then another sentence a few more were present but were not banded. Then in March of 1965 there were not only 35 young present, there were 35 chicks banded. However, most all present were banded. We may have missed 5 or 10 individuals. Then in March of '67 this population estimate of 5 is very poor, because Miller Plateau was not visited on this trip by anybody and we should so indicate. The population estimation of 5 birds seen about ship. This number of 5 has no significance at all and I don't believe it even belongs in this paper. We did note Black-footed Albatross about the ship. They were following the ship clear on from Oahu up to Nihoa and all the way up the Line. Whether they were birds associated with the island or not, who knows. I think the proper

statement in there should say: None observed but no effort or no time could be taken to conduct a census on this trip.

Then we go to March of 1968 and we say 59 chicks found on Miller plateau; that should be changed to 59 chicks banded on Miller plateau. One or two may have been missed. Then a new figure in 1968 - August 24 and 25 - population estimated as zero, none observed at any time, even though the island was well covered. That should take care of the Black-footed section.

⑩ Now let's move on to the Laysan Albatross - I agree with you that the large estimate of 1,000 birds by Kenyon and Rice is certainly erroneous. I'm sure that they must have confused these with boobies, and I don't think it was with the Blue-faced, I think it was primarily with Red-footed because this colony up there, the Black-footed colony, has a large number of Red-footed nesting up there as well as a fair number of Blue-faced. But the Red-footed are so much more numerous than Blue-faced that coming up with a figure of 1,000 obviously they must have been counting Red-footed.

⑪ In the second paragraph, the second sentence under populations - the lack of nests on three of the March visits may indicate that in some years Laysan Albatross nest unsuccessfully. You have just successfully. Here again we may have missed one or two chicks among the Black-foots. It's a possibility. So going on to annual cycle you'll say - second sentence - existing data did not indicate that Laysan Albatross in Nihoa have a cycle any different from that found on the other Leeward Islands. I think this is very true and if this is true, well then the breeding season or their presence under status going back up to the top, should be from at least early November through May or June instead of from at least December. ⑫

Breeding habitat - second sentence - you say Kridler indicated that the five chicks he found in March 1968 were on Miller Plateau. I'm not indicating that, that was a fact. The sentence should be changed to or "and Kridler banded 4 chicks he found in March 1968 among the Black-footed colony" or maybe we should say "within the Black-footed colony on Miller Plateau". Having banded umpteen thousand chicks of both species I'm quite familiar with the difference between the two. ⑬

Now under other areas of occurrence - Kramer in December 1961 and Walker in March 1964 both noted that all Laysan Albatross were on Miller Plateau. This is not true. I don't know about Kramer's observations, but in March of '64 we found a few as much as 100 feet or so down on the south slope of Miller Peak. ⑭

Banding - the Pacific Project did not band 6 adults in March 1964. As I mentioned before Amerson and Wislocki were over on Tananger Peak working primarily with Red-tailed Tropicbirds. The Bureau of Sport Fisheries and Wildlife banded 6 adults in March 1964. On March 8, 1968, four chicks were also banded by BFSW personnel. ⑮

Get over to the tables - March 1967 population estimate of 1 - remarks - no young seen. Here again, the area was not censused up there on Miller Peak. The figure of 1 gives a false impression of what was there. There could have been 10 there for all we knew. The proper remark under here should be "areas not censused". Then in March 1968 correct 5 chicks counted on Miller Plateau to read "four chicks banded on Miller Plateau". Then an additional entry - August 24-25, 1968 - population estimate 0 - remarks - none observed anywhere. (6)

On to the Wedge-tailed Shearwater - the section - populations - the second sentence I think should be taken out of here. Banding and recapture work is fine if you're going to band and recapture the whole population in order to document adequately the number present, but if you're just going to band and recapture just a few how can you base a population estimate on this type of work. Unless you're going to band in selected sample areas. I'd strike that out entirely. Instead I would say "In September they are present by the many thousands. At dawn the steady chorus of calling results in a dull roar over the whole island, muffled only by the roaring surf at lower elevations." This is an experience we had in September 1967 and then again in September 1968. It was really something to hear. There was so much calling that it all blended into one steady roar. I haven't heard that on any other island including Laysan. Unless the person spends the night on the island which apparently Richardson did not there on 1 March, he does not get any idea of what may be on the island at night as birds come in from the sea to roost there.

The next page under breeding habitat. I would suggest striking out the following words. "All observers who noted the nesting area of these birds found them". Just start out saying "Throughout the slopes of the island nests have been found in a wide variety of situations." A small proportion of the population digs burrows in the ground where such digging is possible. Because of the shallow soil cover and rocky nature of the island few deep burrows are possible. These birds make do with whatever is available.

Nest mortality - you say Wetmore observed that Nihoa finches destroyed hundreds of eggs that were unattended by adults. Now this is the same situation which occurred on Laysan and I think some comment about it is warranted. And this is a fact of human disturbance of nesting colonies of seabirds. Nobody to my knowledge has ever made a determination of how many nests are left unattended under normal circumstances. But as soon as a human starts looking through a nesting colony regardless of what it is, Sooty Tern, Frigatebird, Common Noddy Terns, and disturbs this colony the birds fly off their nests, you're going to have the finches move right in after them. There's no doubt that there is a certain amount of predation taking place under normal circumstances. However, I think that statements like this about Nihoa finches destroying hundreds of eggs that were unattended by adults gives a false impression of what the probability or the probable predation is under normal circumstances when humans do not interfere. And when humans do interfere and you want to insert a statement like that you'd better clarify it by mentioning human disturbance.

Under banding - you might also insert 87 were banded by Bureau of Sport Fisheries personnel on March 14, 1965. ✓

Getting over to your table again - in 1964 23 and 24 September, here again, poor Bob Fleet had a hard time making it to the top of Miller Peak and almost passed out from the heat. The Wedge-tails I'm positive were not sampled well enough or counted well enough and so when you say a statement of 2,000 downy nestlings present, that would be a very rough guess and it should be so qualified. If it is a rough guess let us not intimate that it is otherwise, do you not agree? (17) ✓

Now in July 1966 (20 July - 1 August) this trip which Heiden took with myself and Berger and Ernest Tasaka, the population estimate was ours not Heiden's and you should say that an estimated 10,000 nests present, and that's all it was, a very rough estimate. Then an addition to the table - 1968 24-25 August, population estimate, many thousands. Under remarks - so many were calling at the same time in the early dawn hours that the island area was filled with a loud and steady roar. The same would hold true for the September 1967 remarks. ✓

Now the Great Frigatebird - Munter's estimate of 50,000 - all I can say is "yee gods"; where did he drag that figure from? In your last paragraph under populations - in your various numerical estimates, etc., there's an error. In March 1964, we made a very damn good estimate as to the number of birds on the nests and what was probably in the sky. Walker, myself and O'Neal covered the ground very thoroughly and blasted very thoroughly to come up with this figure and we feel it is a very good one. I notice on the observations page that there is quite a bit of, or rather I should say, quite a few observations seen from offshore. And other than indicating the presence of the birds there at that time I don't know of what value those figures have. Under ecology - breeding habitat - second paragraph - last sentence - in March 1968 these Frigatebirds nested etc....Strike that out and instead insert the sentence: this was true also during March 1965, 67, and 68. Then on Nihoa these birds nest mostly in small colonies which contain a dozen to 30 nesting pairs, add or insert after pairs the phrase "while distributed over the island." "Others also are found nesting in widely scattered individual sites." Then I would strike out the sentence "the larger colonies tend to be located just above areas below which are considerable decreases in elevation" this does not necessarily hold true. I don't think it's the rule either. Larger colonies definitely are at the heads of the valleys and some are at the base of the walk out croppings at the heads of the valleys and some are above. (18) (22) (19)

Then we get on to your tables page - the last entry March 1964 - 1,280 active nests - most with eggs. This count was made by Bureau personnel and not by Pacific Project people. In July 1964 when the population estimate - just say none made, instead of the question mark. Anybody reading this then of course should be satisfied. When you leave the question mark he wonders what exactly does that question mark mean. (20) (23)

And with everything else for the rest of these observations, at least give us a little credit for information which was gathered mostly by us. March 1968, you might insert under remarks that the nesting phenology this year was behind that of 1964. There appeared to be a little later. I'll have to check my notes here on the August 1968 trip.

On to Bulwer's Petrel - I disagree with your statement and none of the numerical estimates have much validity as no adequate effort has been made to census populations. A figure of at least a quarter of a million in July of 1966 was not too bad. If anything it was a little low because many thousands more came to the island at night. This is the most abundant species of birds on the island during that time, far outnumbering Sooty Terns and even the Common Noddy Tern. They were just all over the place. Under populations - the second sentence - after the second sentence you might wish to add "most abundant in July and August". I take issue with your last sentence of the first paragraph under annual cycle - you're saying on the other hand descriptions of callings by birds in March 1967 seem to indicate that Sooty Storm Petrels were heard rather than Bulwer's Petrels. That may have been the case in March 1967 but one would gather from that particular sentence that this may have also been the case in these other March observations that they probably were Sooty Storm Petrels. Now we did not see any in March 1968 and in March 1964 I know there was one present because I took a picture of it. I took it out of the hole in the rocks, put it on the rocks, took its picture and put it back in, so the statement that numbers seen in March 1967 by Pacific Project personnel is an error. There were others there. I don't recall making a count on them, but I definitely know there was at least one present. (24) (28) (25) (30)

Getting down to the last paragraph - "only young were found from mid-August through mid-September, etc..." I would strike that whole sentence out. Instead substitute "hundreds were observed in late August, 1968 during the day, their numbers increased to thousands at dusk as those returned from fishing at sea to roost on the island during the night. They were flying all around us at dusk at night, flying into the tent, just like the shearwaters, and were very abundant." Then under breeding habitat, just what do you mean when you say "oh, Vanderbilt and this other chap Kridler and Heiden noted the species was found throughout the island but few described the nest sites." Do you mean the few of us, or the others besides us? I would say that nests are found in innumerable nooks and crannies both under vegetation and within bare areas. Even though Heiden banded a large share of the adults in July and August of 1966, the rest of the party gave him considerable help. He would have never done the job by himself.

The observations on the island - the 1966 July entry - you cite Berger (27) as your reference. Berger accepted my estimate. He had no idea just what was out there. He was flabbergasted by it all. The same pertains to Heiden. Your footnote about Kramer and Beardsley noted that Bonin Petrels were common and on eggs. I'll agree with you on this, Roger. I'm sure they must have been mistaken. They must have meant the Bulwer's Petrel because we've never seen any Bonins all the time I've been there.

Last entry - 1967 24-25 March, population estimate 1,000. Under remarks - hundreds of adults seen during the day increasing to thousands at night. Several downy chicks were found. USBSFW.

Christmas Shearwater - under populations - last sentence - starting with these estimates and going on to say it's possible that intensive banding might reveal greater numbers present. Banding would not, censusing would. I would strike out the word banding and substitute censusing instead. Banding has its place but it is not everything. Annual cycle - your first sentence I'd say this, there's no doubt about it at all but I would strike out the word "is somewhat earlier" and just say "is earlier". We know it is, somewhat is a weasel word. The same pertains to breeding habitat. In that sentence this species tends to nest on higher elevations. Should be: This species nests in higher elevations. Tends is another weasel word. ✓

Next page - sentence among the rocks, etc. After September 1967 add "and near Miller's Peak in August 1968. Now you're citing Heiden again, and what he observed and here again, Heiden saw some things but most of the observations were made by myself, primarily myself and Kasaka and Berger. The last paragraph about Wetmore's statement "they nest in little shelters, etc.." my comment is that this is correct, according to my observations. I believe we banded some Christmas Island Shearwaters there, but just a few birds. ✓

Still on Christmas Shearwaters

Another entry way down here - in 1968 24-25 August, population estimates 6 - under remarks - 3 adults and 3 chicks found near Miller Peak. The chicks were almost completely feathered except for a small amount of down on the top of the head and the nape of the neck. USBSFW. ✓

Red-footed Booby - last sentence - builds nest in low bushes of Pritchardia palms. If you are going to mention Chenipodium in the other species accounts accounts you might as well mention it here and say builds bulky nests in Chenipodium, Sesbania or Pritchardia palms.

Then under populations - the second paragraph I'm going to take exception to your statement there. You say that the relatively low level of accuracy of the more recent estimates makes it impossible to state that the population actually was larger at that time. Don't base all your observations on one trip, Roger. The March estimates except for those in 1967 and 68 were very good especially that of 1964 and we made a special point of getting good counts on the Frigates and also the Red-footed Boobies. (33)

Getting back reading through the account again - under status - I would strike out everything that pertains to nests including my comments. You cover it here under breeding habitat. Status should just be numbers, occurrence and things like that. The nesting is taken up in your breeding habitat. No sense being redundant. Banding - we banded a number of adults there and I'll have to check my records on this. On your table sheet - the last entry of March 1967 I lined out a sentence about only 5 percent of the nests with eggs, none with young. That should remain in. (32)

Now here again in March 1968, Roger, I don't know how many nests you counted but I'm quite sure that the sample count of 115 nests was my work. Yet you list it as Pacific Project. Add 1968 - 24-25 August, population estimate - hundreds - and under remarks - scattered throughout vegetated south slip of island, large colony of 150 nests on Miller Plateau. These nests contained eggs to flying young. (24)

And over to Brown Booby - I'll have to get my 1968 August notes in on this. The last sentence under annual cycle - I would say "However, judging from recent observations, fledgling is largely completed by the end of August, not September. I wouldn't really say that these birds at any time nest in concentrations. They are somewhat near each other but this is not really a concentration as such. So the first sentence on the second page, I would change the also often occur in small concentrations along the ridge above the cliff basin on the north edge of the island; just say they also often occur along the ridge above the cliff base on the north edge of the island. Then the next sentence after "the use of sites in this area" I would add "because of inaccessibility." At the end of that section add "in March 1965 most nests were found near Tananger Peak and along the western ridge from Miller Peak to Dog's head Peak". (25)

Your table sheet - the 1965 13-14 March entry - breeding status - eggs and young present, most nests contain two eggs. At the end of this table add 1968 24-25 August - population estimate minimum of 50, and then under remarks - 14 observed in one group on Tananger Peak. Immatures were flying. No nests or downy young were found. Here again we rather thoroughly covered the island and if any were present, I think we would have found them.

Now the Blue-faced Booby - under status - I don't think you should mention nests on ground primarily in areas of higher elevation because you take that up in breeding habitat. Under annual cycle - second paragraph - first sentence - eggs are laid primarily in March and April and at least a few are laid in May and June. I would strike out "and at least a few are laid in May and June" and instead substitute "while others may be laid during summer months up to and including August", and I would strike out the statement "in most years apparently little breeding occurred later in the year". I would strike that out completely, I don't think that's correct. I would strike it out and capitalize the The presence of recently fledged young in mid-June, etc.... (37)

Breeding habitat - second sentence beginning with "on many surveys" strike out "most nests were noted on Miller's Peak and Miller's Plateau" I would strike out Miller's Peak and just say Miller's Plateau. The next sentence beginning with "on several surveys" strike out the words following March 1968 a concentration of ... and instead substitute "a number of nests were located just above the cliff base along the north side of the island between Miller's and Tananger's Peak". On banding I'll have to check my records, Roger. I don't think we banded any Blue-faced there but I'll just check them to make sure. (36)

Now on the last page here on your observations, your tables - all comments about Bureau and the Pacific Project observations hold as on all these other species accounts. Add one more - 1968 - 24-25 March - population estimate 200-250 - most abundant on Miller Plateau. Nests there contain eggs to almost fledged young. Many immatures flying. USBSFW. August 7
(30)

Red-tailed Tropicbird - here again nest on ground and shelf of rock or vegetation. If you're going to take that up in your breeding habitat I think it's redundant including it here also. Second sentence beginning with "probably at least one or two birds present in all months" strike out "probably at least one or two birds" and substitute "low numbers (10s) present in all months but much more common from March through September." This statement under populations after the last sentence "in any case the March 1965 survey was the only recent survey on which active nests were found." I'm trying to scratch my head what we found there in September of 1967 then say what - (39) do you mean by an active nest? Now, if you've got a young bird there which hasn't fledged yet, that's still an active nest. And in September 1967 we saw some large chicks, however, in August 24-25 1968, we found nests containing eggs being incubated to those containing almost fully fledged young. and found over 50 nests during the course of other activities, and here we didn't make any special search for nests. If we had, we probably would have found quite a few more. So therefore, in view of this information, I would completely strike out the sentence there under populations in any case the March 1965 survey was the only recent survey on which active nests were found. This certainly is false. So, in light of this last trip of ours, Roger, we're going to have to revise several other statements here. The section - annual cycle - the second paragraph - first sentence that starts "the earliest that eggs were recorded" you might have to substitute "and the latest they were recorded were 24-25 August instead of 7-15 August" and I've no doubt that there may be some egg laying taking place after that time. (40) The next page ecology - breeding habitat - second sentence "one observer Heiden believed that they were most abundant in the vicinity of the cliff face, etc..." I don't believe that. They're pretty well distributed. That West Palm Valley, especially the rim rock areas, there's quite a few small caves in there and they're quite common there too, and I'm almost inclined to believe that they're more abundant there than the vicinity of the cliff face between Miller's and Tanager's Peaks. So I take issue with that sentence here in the light of what I have found over there in the western part of the islands. Here again, Richie didn't get over there at all. Pretty rough going there and most of the time we lined up going down that slope. I would insert a sentence "Common in rocky crevices at the base of rimrock outcroppings over west end of island and West Palm Canyon". The last sentence "many also nest beneath dense vegetation" strike out many and insert the word "some". Generally when they're under the dense vegetation Roger, they're in a little puka in the rocks. We might instead say "most nests are found in rocky cavities" and let's put it this way "most nests are in rocky cavities and are found all over the island. Rocky canyons and outcroppings are favored.

Give some one is in another place

Red-tailed Tropicbird
 Herring Gull
 Bristle-thighed Curlew
 Red-billed Tropicbird
 P. 11
 Black Noddy

Insert the last entry 1968 24 to 25 August population estimate 2 to 300. Under Remarks nests contained eggs being incubated to almost fully fledged young. Over 15 nests recorded during course of other activities USBFSW. Don't think we banded any Tropicbirds on Nihoa.

Now we're going to take a break here and try and polish off some of these others.

Nihoa - The Herring Gull.

Observations- Banko, Walker, and Kridler observed an adult near Derby's Landing. The bird was first observed by me who pointed it out to both Banko and Walker who didn't know what it was. I sneaked up on the bird with the shotgun and was able to identify it without any doubt as being a Herring Gull, and the other two took my word for it. My attempt to collect it was unsuccessful as the bird flew off before I could get within shotgun range. The second sentence - Herring Gulls are one of the gulls most commonly occurring - let us just say - Herring Gulls are one of the gull species most commonly occurring in the Hawaiian Leeward Islands. And also this Leewards Islands, Roger, for your information the State Department of Economic Planning, Board of Geographic Names, submitted a number of changes of places and Islands here in Hawaii to the Board of Geographic Names in Washington who approved them, and one of these is that the Leeward Islands will no longer be referred to as the Leeward Islands but instead all these Islands from Nihoa up to and including Kure will now be called the Northwestern Hawaiian Islands. So, I believe that since they have been officially changed to the Northwestern Hawaiian Islands that we should so reflect it in these reports. Do you agree?

Bristle-thighed Curlew - I'm going to have to check my 1968 August notes. It seems to me ^{that} we saw one or two at that time. ^{that} sentence - two others were seen skulking in the rocks at about 150 to 200 feet near the base of Canyon 7, 8, and 9 of March 1968. I don't like that word "skulking." Skulking intimates or infers that they were sneaking around when actually they were not, they were perched on top of the rocks and calling. Just say - two others were observed among the rocks. And again Hawaiian Leewards Chain to Northwestern Hawaiian Islands. (42)

The Red-billed Tropicbird. We have never observed any; that remotely look like it, during our trips there it's possible we missed it. So far as I know this record by Wetmore is the only one from Nihoa.

Black Noddy - I don't know your authority for calling it the Black Noddy instead of the White-capped Noddy Tern or the Hawaiian Noddy Tern. I would be interested in it. Unless the name is officially accepted by an Ornithological organization like the AOU or the Cooper Society, or something like that. I think this business of one calling it Black Noddy, one calling it that, and a third person calling it this, is very confusing. (43)

In the Species Account - Populations - the second sentence - consequently since the north cliff face is seldom carefully observed, you had better change that - consequently, since that north cliff face cannot be carefully observed, or has not been able to be carefully observed. I don't know just how you are going to carefully observe it unless you get down on a ship in a small boat and just stay there and watch things, or lower yourself down with a rope and swing all over the face of the cliff, how are you going to carefully observe it. You never did go down to the bottom of the Devil's Slide with us, did you Roger? No, I don't think you did, but in August 1968 John Simcock and I went down to the very bottom as far as we could go and we had a real good look at that north cliff face and were very surprised at the amount of overhang. There must be at least a 30-40 or 50 foot overhang of the top and looking at it made you wonder just whether perhaps we weren't being a little careless walking along the edge of that when we are on the top. Even looking down from the top and peering over the top of it and looking down the face there is a lot of this cliff face you couldn't see because of this overhang. About the only way you might be able to get an idea of what was down there is by heaving rocks against the cliff face and getting some idea of the small terns which flew out and you would get some idea of their abundance. But this again would be an extremely rough way of doing it.

I question very seriously - very much Vanderbilt's observations that this species nested particularly in Middle Valley. They also noted that there are no distinct colonies and the birds seem to mix freely with Brown Noddies. Of all our times there and with all the nesting taken on we have yet to find a nest of this tern in Middle Valley or on the South Slopes. I question that observation very much. I'll have to check my August 1968 notes.

Brown Noddy or Common Noddy. A correction in the last paragraph of Status. Change the sentence to read, "Birds nest on the ground in both vegetated and clear areas." These birds nest just about everywhere.

Populations - you speculation that the population estimate for July-August 1966 seems particularly large compared with other numerical estimates but no others have been made at that time of the year. You continue by say, "Consequently, it is impossible even to speculate whether this estimate is unrealistically large." On the basis of our observations there that month and that year the birds were extremely abundant and we feel that the estimate of 20,000 is very, very conservative. Knowing how common they are, breeding there at Pearl and Hermes Reef, also French Frigate Shoals in the summer months, it would be very safe to assume that the peak of the breeding season is during the summer months. Therefore, I would say that your sentence beginning with Consequently, should be stricken out, and also your inference that this particular count may be wrong in over-estimation is also wrong. The birds were extremely abundant at that time, and it certainly needs to be reworded. You are assuming that all of Wetmore's counts are extremely accurate. Having worked an awful lot with Wildlife people in my 60 years I have found out that some of the supposed big names in ornithology, unless they actually counted birds, got pretty wild with some of their estimates. I

particularly recall a photograph of 20,000 snow geese being circulated among biologists and Refuge Managers, many of who have had quite a few years and when asked to make estimates as to the number of geese they thought were on the photographs, estimates ran from about 500 to 40,000. So to sit back and speculate on the basis of written observations and making judgments as to which is the correct census figure and which is the incorrect one is kind of putting yourself out on a limb, so I would suggest striking out the words in that sentence, "seems particularly large" and insert "is large."

The third sentence, starting with "the general trend of estimates indicate that birds number at least in low thousands are present in summer and fall" and say that "the general trend of estimates shows that birds numbering at least in the thousands are present in summer and fall."

Then getting way on down to the last sentence on this page, under Annual Cycles, you say "judging from data gathered on other Leeward Islands" lets change that again to Northwestern Hawaiian Islands.

Another error, on your Table sheet for 1968, 24 to 25 of August, nests contained eggs to flying young, many adults, estimate was in the low thousands but probably less than 5,000.

On Wandering Tattler, entry 1968 August 24 to 25 none observed USBFW. And here again your references and remarks should include the Bureau.

Blue-gray Noddy. On Annual Cycle, Roger, the first sentence beginning with "Too few detailed observations." You go on to say, "Very little observations indicate an extended breeding season from at least December through October-November." I don't think it goes as far as October-November according to our observations and in checking in your table over here I see that there are no observations for October or November so it appears that maybe in some years ^{breeding} may extend into September and October but our last several trips in September, the one in 1967 and the one in late August of 68 we didn't find any nesting activity these two years, although we did find it on the 23-24 September 1964.

Under Banding I've inserted 18 which we banded in March of 68 and then 1-2 in late August of 68. Then on your Table Sheet of 1968 I added August 24 to 25.

Information. We did not make a special count; we did actively look for nests but we found none. The population I'm saying is in the high hundreds but I think it was probably less than 1,000.

Golden Plover. Under Status - "uncommon migrant" I think I would add the words "uncommon but regular migrant." We seem to see them every time we are there. The population estimate for the plover under March 1968 trip should be approximately 50, and we would say scattered all over island, usually as individuals. I have to get my 1968 notes down here and see what we have for this last trip in August. I think it ran to about 10 to 15 birds, individuals scattered over the island

Blue-gray Noddy (lets get back to that a minute) (garbled)

Gray-backed Terns. The question I had here Roger was on your Sheet 2 under Breeding Habitat, the sentence starting with "Colonies were found nesting on North Cliff face March '64". It is very possible that Amerson and Wislocki saw something on the Cliff face but generally it is rare, that we seen the Gray-backed Terns on the Cliff face itself, such as we see the noddies. The gray-backs nest just about at the bottom of the island up to the top although they are, as you mention here, the maximum densities are in the canyons and gulches; however, in March of 1965 I went all the way down from the top of Miller's Peak clear to the southwest point of the island along the extreme west side and saw many Gray-backed Tern nests all the way down from the top to the bottom on the ridge. They prefer more open areas on the cliff faces than the sooties. I banded 91 on March 14, 1965, and we haven't had any returns or recoveries on them at all. In 1968, August 24 and 25, the numbers were in the low thousands; however, they swelled at night. Here again it would just be an extremely rough estimate, I think there probably were less than 3,000 birds or so. Their numbers increased at night but I have no idea just what the number might have been. We found a few young almost completely feathered, still not able to fly, but an awful lot of young which were capable of flight. These birds really defend their nests when they have eggs in them and I remember teasing one with the palm of my hand by holding it above the bird about 5-6 inches while it remained on the egg and it actually jumped up and speared me in the palm of the hand with the tip of its bill. Of course, it didn't amount to much but it drew a little blood. Give credit to the little things for being spunky.

I'd like to get back here to the frigatebirds, Roger. This is our

March 1968 trip that you were on and I don't know whether you got this data or not but we kept track of the frigatebird nests and actually totalled about 2009 nests and I suppose we might have underestimated about 100 or so nests but I think it is pretty close. The nest location is as follows: Now this will be for both the frigates and Red-footed Boobies. On the west slope of Miller Canyon, starting at our camp site up to the top of Miller Peak frigatebird nests 300, red-footed 105. Now on the east slope of Miller Canyon from the the camp site to the pinnacle, frigate 250, red-footed 50. Now on the saddle between Miller and Tanager Peaks, you know where we generally walk right on the top of the cliff face, frigate nests 40 and no red-footed observed, but halfway up the ridge between Miller and Central Valleys I didn't observe anything. Just below Miller Peak on the same ridge approximately 130 frigate nests. the east slope of the Devil's Slide up Miller Peak 25 frigate nests and 160 red-footed nests. Just below the peak on the south facing slope of the island frigatebird nests 50, no red-footed. Miller's Plateau just don't seem right, let me check this again. Yes, this is Nihoa all right and March about 50 frigate nests and 250 red-footed. Kenyon took the lower east side of the island and counted 147 frigate nests and 131 red-footed nests. Ernie Kasaka, as

you recall, took West Palm Valley and the southwest part of the island there, down at the lower elevation, and he counted 434 frigate nests and 182 red-footed. Then the Central Valley, east of East Palm which you took Roger, you counted 291 frigates and 164 red-foots, and in the middle elevation from the east side to the top of Tanager Peak which John Sincock took he counted 322 frigate nests and 135 red-foots. So that comes to actually a total of 2,009 frigates and I suppose we could say ^{we probably missed some} there probably were 2,100 nests and about 400 red-foot nests. Most of the frigatebird males were displaying, some were displaying on empty nests and some were displaying on what appeared to be nest sites, no evidence of nesting but they probably would have nested there later on in the season, and a sampling of 55 nests I found 29 with eggs, none with chicks, and 26 empty. This would come to about 53 percent with eggs and 47 without eggs. Probably 70-80 percent of the males were displaying on partially or completely constructed nests; most immatures were soaring, and no chicks found.

This comes pretty close to what we found in 1965 and I estimated at that time, which is just a few days earlier, 2,500 nests without 70 percent containing eggs. So these counts are very close and I think they are excellent counts, including ^{that of} March 1964, however, there we had almost twice the number of active nests. Why this should be I don't know. It could very well be that the breeding season was 3-4 weeks advanced so that when we landed there in March of 1964 there were a lot more nests. As you will recall on our last trip there in March of 1968 there were an awful lot of birds displaying and quite a few males flying around and these could very well have begun nesting sometime in late March.

We'll get to the Sooty Tern here. Now this count we made in March of 1965 was pretty good; it is really the only time we made any effort to determine what really could have been there in the way of Sooty Tern populations. One of the things that makes it so difficult to estimate the population of these birds, Roger, as you made have found out, is that there are large colonies nesting under the Chenopodium, extensive stands of Chenopodium and Sida, or Ilima as the Hawaiians call it. I certainly agree with you when you state that populations of this species are much more under-estimated than over-estimated. (56)

Ruddy Turnstone all seems to be pretty good, Roger. I crossed out "occasionally and just say "common migrant, present in small numbers." (57)

Now we'll get over to the White Tern or Fairy Tern, Roger. I don't like that sentence under populations; Wetmore's estimate is also larger. It just sounds funny to me. I don't know what Wetmore did. Did he actually lower himself down the face of the North Cliff and work on the wall? In other words, how did he estimate what was there? All we could do was throw rocks down all along and see how many Noddy and Fairy Terns would fly out and get an idea of what could possibly be down there and these would be minimal figures, and we have no way of knowing the number of birds which did not fly out, which were not sufficiently frightened enough to fly out. This past August John Sincock and I worked ^{there} our way all the way down to the bottom of the Devil's Slide from the top/near Miller's Peak and got a different perspective on the shape of that North Cliff and were quite impressed /about Wetmore's estimates any more inaccurate than the recent ones (58)

and surprised as to the amount of overhang. We thought that we could see most of the Cliff from up on top along the ridge there between Tanager and Miller Peak, but actually found ^{that} it probably overhangs 25, maybe sometimes more, feet, so trying to get an idea what is along the cliff face from the top is impossible because there a lot of the area you can not see and the rocks which we were throwing down probably had been landing half-way down and frightened out the birds from just the lower portion of this cliff face. When we got down to the Devil's Slide and glassed over the upper portion of the cliff face we could see numerous Fairy Terns in the folds and other little pukas there that we couldn't get any idea as to what the population may have been that were in these holes. So here again, not knowing how Wetmore got his estimates, he may have worked along the base of the north cliff face there by boat and got some idea by looking up. So your statement about we hesitate to conclude that his estimate was any more inaccurate than the recent one is kind of a funny sounding sentence. So I don't know just how that sentence should be rephrased but it certainly needs rephrasing.

~~Checking over here,~~ the thought just struck me that we haven't mentioned anything about behavior of birds here. Do you intend to cover that in a preface or something like that Roger? I think one of the things that should be brought out here is the extreme tameness of some of these birds. However, not all are so tame as others, for instance the 59, the Brown Booby is much more wary than either the Blue-faced or the Red-footed, and, of course, the red-foot on the nest won't get off, but they are, in turn, warier than the Blue-faced and are more apt to flush off their nests except, of course, when young are very small or the eggs are about ready to hatch. But it is certainly a lot easier to approach the red-footed and the blue-faced than the brown. We might mention the wedge-tails and the Bulwer's Petrels habit of when we are camped on the island and have our lights on they will fly into us and into the tent. I think that the Blue-gray Noddy is a little more wary than the Fairy Tern. You can certainly get a lot closer to a ^{nesting} Fairy Tern than you can to a nesting Blue-gray Noddy.

I don't know how often you noticed the Red-footed Boobies who put their bills on their back or those nestling frigatebirds and Red-footed Boobies that seem to have a habit, I think it probably is to keep cool, not only of fluttering the gular pouch and having the mouth open but also drooping head down over the edge of the nest so that the head hangs way down below the edge of the nest.

I have a few odds and ends to clean up on these things, Roger. I think by and large it isn't too bad, although I think you are speculating far too much and as I mentioned before, on some of these speculations I think the data is rather meager and you are on a little thin ice. I think on many of these accounts we should state what has been seen and go a little light on all these speculations. Some of them, of course, as regard nesting and knowing the incubation period of some of the birds you can then back-date their probable time of laying. I didn't know just how I should treat this thing; I kind of chopped it up here and there. I would suggest that you read it over yourself and check on some of the phrasing. And here again, as I also mentioned before, you have spent a couple of days in March

on the island, the rest of the information to you, of course, is second hand. I personally have been on the island 9 or 10 times and after a while after visiting it of course March, July, late August, and September you kind of get a little bit seat-of-the-pants feeling so to speak on what is taken place there. Even though in a number of accounts we could only give passing attention to some of these species you kind of get an idea of what is taking place there. Of course our interest in the last few trips had been devoted to getting good population estimates on both the Miller bird and the finch and other information has been picked up alongside with it. Other trips, of course, like in July, we spent quite a bit of time just getting that damn sign on the island and getting that rigged up, and of course that's when I got creamed and my wife almost became a widow and I was pretty stiff for about four days and pretty badly cut up by the rocks when I got banged against them by the waves.

So as you have said about the White Tern estimates that some of the populations are rather subjective. We can only speculate as to what is present on the island at night; it is a dangerous place to work at night. We hope eventually for each of the species to establish transects on other sampling techniques whereby we can approach a little better estimate of the population. Then again, some of the bigger birds like frigates and the boobies, we can get a pretty good idea of what we have there in the number of nests while we're there. I don't know how thorough Kramer and Swedberg were on those couple counts they were on especially Ray. I have a feeling these were just general observations. What you seen from off-shore of course is no indication of what is on the island. One would surmise however that you would expect the relative abundance seen offshore to correspond with that which was present on the island, although this wouldn't always hold true. Of course Kramer's report on Bonin Petrels, I'm sure, was either misidentification or a misprint, or a typographical error, or he might have written Bonin Petrel on the island and right now I am beginning to wonder if in any of my notes on Necker whether we have seen any Bonin Petrels there. It seems to me that the first place we ran into them was at French Frigate Shoals although it is quite conceivable a few might be on these islands at other times of the year. Who knows?

On the August 1968 trip just general observations were made, Roger. I haven't gotten around to writing the report for my outfit yet. But we made just cursory observations of the sea birds there. Most of our effort was directed toward trying to find something out about the Miller bird, searching for nests, and we were fortunate to find one nest. One of the fellows with us, Bob Eddinger, was trying to help us catch Miller birds so we could take a small blood smear by making a pin prick on the leg and getting a drop of blood out and smear it on our slides, and we were trying to catch some to check plumages and the like and he made a slight (garbled) with his net there on Miller Plateau both flew low but the net swung past the Sida he exposed a nest with one egg. This is apparently only the third nest known to science and we felt it a good opportunity to try and get some movie footage of it and I spent quite a bit of time getting this footage. Very good lighting and the like; I understand it is now in the Washington office. We were quire thrilled over this, the only thing is

the little critter was quite wary; the least little bit of movement on my part it would get off the nest and maybe take a half hour or three-quarters of an hour to get back on. It came sneaking underneath the vegetation and sneak over the lip of the nest and be down on the eggs in just a flash. It got off in a flash too, so my footage of course is of the bird incubating the eggs and the nests itself with just the eggs after the bird had flown off. I had been hoping to get something showing the bird perched on the edge of the nest but it the bird was always just too fast for me. Just like what has happened to us so often, you get off the island before you really want to as sea conditions get a little worse.

We chugged down on to Necker, probably made the best landing and take-off we ever made. We had two geological survey people with us for taking core samples of the rock and they had some rock drills and drove some holes the rock for the new sign. Wound up with John Sincock and I spending all the time we had on Necker putting up the sign and I just had only enough time to crawl to the top of Annexation Peak and just eyeball the length of the island before we came down again. I hope that this sign stays. We got it guyed with a number of marine wire cable and if it blows down this time damned if I'll put another one up, somebody else can do it.

Got on up to French Frigate Shoals and ran into the same situation we had on the trip when you were with us. Got pinned right down on Tern Island and couldn't go anywhere else. This was real aggravating. Ship wasn't able to get its small boat off after the first day and they just locked out there and finally even in very rough weather had to start working buoys and pretty well banged up their boat. Of course, there wasn't too much on Tern Island so it was a rather frustrating two or three days that we, or rather four days, that we spent over French Frigate Shoals. But then we chugged on up to Laysan where the ship dropped us off and about 11 days later picked us up.

Got some good population figures from Laysan as to what was there excepting of course the many, many thousands of shearwaters and Bonin Petrels. But other than those I think we got some pretty good counts. John and I type-mapped the island. Our botanist friend confined most of his activities to, after the first day when he gave us help with the seals, just investigating the Portulaca. He's working on his doctorate on Portulaca so of course much of the time was spent on it. The other chap we had with us was of not too much use. Wandered around the island taking pictures, gave us a little help with the seals, and a little help with the Laysan Teal on our Teal count. We tried to dry-trap Teals but worked the bird to the throat of the trap and then they just fly over the top of it. Weather by and large was pretty good, very warm, and of course the flies were miserable. We tagged a number of seals.

I was rather surprised by the small number of Blue-faced Boobies and also Red-footed Boobies. We checked around for the Sooty Storm Petrel area there in the south end that you had found in March but found no evidence of them. The lagoon was extremely low and we were able to walk right across the bottom about two-thirds of the way on the north side. A bit of our time

was spent checking on the Laysan Finch trying to come up with a reasonable population estimate on the Teal. The squalling of the Sooty Terns day and night as well as the moaning of the shearwaters made sleep rather difficult. We had no radio communication; we were able to contact the ship the first night when it was about 65 miles from us on the way to Midway and then lost communications, so were quite fortunate that we had no bad accidents or the like. John Simcock got bitten on the foot by a yearling seal. It just barely broke the skin, luckily he had on a pretty good pair of leather shoes so the bite wasn't too serious although his foot bothered him for three or four days and was rather stiff. We were rather disappointed with the two fellows we had with us, these two PhD. candidates, one a botanist and one an ornithologist, and rather surprised in the lack of interest the ornithologist showed in the bird life there. Of course this made wrestling seals rather difficult, they were rather reluctant to jump on the seals with us, I think they were somewhat frightened by them. Several times it wound up with John and I trying to wrestle the things and almost getting bitten in the process, while these two stood off and watched rather apprehensive about jumping in there with us.

I hope to get around to writing up the notes here sometime this month after we get our budget estimates in. I now have an assistant, pretty good boy. He was a project leader at Lake Andes but its going to take a little time for him to get used to working on this area and becoming oriented and conversant with our multiplicity of duties here, especially enforcement angle. I'll try and get these notes cleaned up tomorrow and get the tapes and recorder back to Bob Pyle so he can send them on to you. I know you have been waiting for them and its been bugging me that I couldn't get them to you sooner, but it seems to be one damn thing after another, turning over here, you have to drop what your doing and rush off to take care of the new crises. This is where the man in operations envies the research man.

I also tried to get in my leave time the tail end of this month but looks as if I am going to sacrifice five or six days of it and then lose it.

Give my regards to Binion if hes back there and tell him I'm sorry I didn't get a chance to talk to him more over the phone, but I had a meeting in my office there.

Tape #2

Gene's comments - Necker

Comments, suggestions and corrections by Gene Kridler to Roger Clapp's original draft on the Necker Island Account.

Species Account: Brown Booby - The change - first paragraph - Status - evidently a rare breeder - change to evidently an uncommon breeder. Change recent estimates from 25 to recent estimate 40. Third paragraph - Annual cycle - change other Hawaiian Leeward Islands to other Northwestern Hawaiian Islands. Next page - on the bottom I've added the 1969 data where we counted 20 nests all of which contained eggs. And, Roger, this is the reason why I've changed the maximum recent estimate from 25 to 40.

Move on to Red-footed Booby - Now, Red-footed Booby, common breeder - maximum recent estimate 1,000; change that number to 1,400. Roger, here again the 1969 data is responsible for the change. One of the comments I had also: did Wetmore include chicks in his estimate? If he did, we are real close in 1969 with his estimates. You see he was there in the summer and by that time many chicks would have been probably fledged or pretty close to fledging. Now we had a minimum count of 700 nests and we even may have missed a few. Were we to multiply this by 2 because of our breeding pair here in March would give us 1400 adults, then add the probable 700 chicks, even 600 chicks, considering that perhaps 100 die, which isn't likely, were we to add these 100 chicks, that would then give us 2100 birds probably in June, both adults and immatures in June. This then would come pretty close to Wetmore's estimate.

The next page under breeding habitat - you say in March 1965 Kridler indicated that most nests were found on the north slopes. This still holds true. From general observations of other Marchs and also in September and then again here in 1969. I don't recall ever seeing the nest of the Red-footed Booby on Northwest Cape. These birds certainly seem to favor very strongly the Chenopodium as well as an occasional Sustania plant.

In the last page on the table for the Red-footed Booby I've entered the 1969 data and also the information on the sample of the time I counted, checked, 118 nests, 70% had 1 egg, while 30% contained no eggs at all and which were very new; and here again in your 1967 March information throughout all your tables for all species, you

• just list the Pacific Project and we would appreciate adding either my name or the Bureau's name to this. Because this was the time Doug Hackman went with us and certainly Doug was a minority and we gathered the bulk of the information. Of course we've gone over this all before and I'm sure it's been pretty well cleared up.

Let's get on over to the Frigatebirds - looks pretty good, Roger. I've added the March 1969 data and the first page under Frigatebirds - second paragraph - populations - at the end of the first paragraph after 1965 and 1967, I've added this sentence "the number of nests counted in March 1967 and March 1969 were almost identical, 832 versus 850". It's amazing how close both counts came and both counts were counted in a similar manner by us and were essentially a head count. We may have missed a few nests but certainly less than perhaps 10 or so.

Annual cycle - the fifth line - I've inserted very between a and small proportion of the nesting population. And here again, this is unfledged young there in March and we would assume that there are very few if any unfledged young in February also. Then the last paragraph on the page. You say, a few eggs may be laid in late February but no egg laying probably occurs in March and April, let's scratch out that probably. Most egg laying does occur in March and April, certainly in the latter part of March and early April on the basis of our March trips. And then when you go through the text here, there's a number of probablies and I think we're weaseling a little bit too much there. Now, also the last word and before we get into the next page - you say hatching probably occurs primarily in May or June but may occur as late as early August. Here again I've scratched out the probably because based on the counts of the nests that contain eggs or quite a few eggs, I imagine we could say that hatching occurs primarily in May or June. On the next page, the first large paragraph, in parentheses you have the statement Immatures seen by Kridler in July 1964 were almost certainly young from preceding nesting seasons. I've scratched out the almost certainly.

Next paragraph - breeding habitats - I've added - line three - towards the end - where you say "dense Chenopodium bushes and Fisher reports" after Chenopodium bushes and insert also utilize almost every bush of Sustania scattered along the ridges and higher slopes. And this is the case. There's ~~Sustania~~ there scattered all along the

Done

higher slopes and the ridges and just about everyone of them has a nest in it and sometimes two depending on scraggly the particular plant is, so we will start a new sentence with Fisher 1903a:779 reported etc. Then I have a note on the side of this paragraph here saying "during each March trip we note that Frigates are scattered all over the north slopes of Summit and Flagpole hills in among the Red-footed Boobies. And in addition, they are scattered all over the tops of^{all} the hills and upper slopes of the other hills other than those mentioned. By George, these frigates, you start running into them when you start getting to the top of Annexation hill there in amongst the old hay (?), and they're scattered all along clear around over/^{almost}to the east end there. And there are very few that go down below the top of the south slope because it's so sheer and there are so few plants there but as you look down the north slope, of course, there's quite a bit of Chenepodium going, sometimes at least half, if not a little bit further, down along this slope and you see the frigates scattered all along nesting down there amongst the Chepodium, as well as of course, right at the very top.

Now to this banding business - you've got all the banding records, Roger. I don't recall whether we've ever banded any frigates but we did band albatross and a number of other things there on Necker and I think that probably we may wish to include this under Banding and Movements. I think we've given you all our return data on any of the birds which we tagged on Nihoa and Necker and other islands and were recovered elsewhere. Although, here again, we banded a number of Blue-faced Boobies as I recall on Necker and I don't think we banded much more than a few Blue-gray Noddies and possibly Fairy Terns on this particular island. Whereas on Nihoa I recall the March 1964 trip, we banded several Wedge-tailed Shearwaters. Through the various March and September trips a few Blue-gray Noddies, however concentrating most of our work on Nihoa on the finches and Miller bird. So, on your table for Frigatebirds on Necker I have added the 1969 data and the population estimate, Roger, is the absolute minimum. We counted 850 nests, and we possibly missed a few and if you would double this, it would give us the number of breeding adults and that's 1700. Now, we didn't here again, make an account of other adults flying and there were quite a few immature frigates and I had asked

Done
cf for Nihoa

I don't recall who was checking the frigates, but I think I asked him to check also on immatures, sitting around, and there was some confusion about it, and they were not counted. Otherwise, I do know that there were a number of immature birds flying and roosting and I imagine there again it's just a Cloud 9 figure, I wouldn't be surprised if there were maybe 200-300-400.

Golden Plover - I have nothing else to add; although we didn't see any Plover in March 1969 and perhaps here again negative information might be just as important as positive. There are low numbers but being as how we didn't see any at all in March 1965 and only 2 in March 1967, perhaps for 1969 we should also add zero.

The same thing pertains to the Wandering Tattler. We have negative information for March 1969 also.

Nothing to add on the Ruddy Turnstone. I think I would agree with the observations that although never occurring in large numbers, turnstones are the most abundant and most frequently seen ^{Gene - ur} shorebird that visits both. Let's keep the tense consistent so if we say turnstones we should say are the most abundant and most frequently seen shorebirds; so let's make birds plural. The occurrence however, on Necker, as far as numbers are concerned is rather irratic.

Sanderling - I have nothing to add there.

Nor about the Glaucous-winged Gull.

Gray-backed Tern. 1969 data added on the bottom of the table. The population estimate of 1300. This is what we would call D-data, Roger. We mean it is not essentially a headcount but it has been a very close estimate based on samples not as precise as the things which we do on the Millerbird and the finch and the like, but pretty close, and very good data.

The text - annual cycle paragraph - change 3 March surveys to 4 March surveys and add 1969. The bottom, last sentence, in March 1969 almost 25% of those present were incubating eggs. Under breeding habitat - I don't know what Fisher saw in 1903 when he says only very sparingly on the broad shelves with *Sterna fuscata*, Sooty Tern, more recent observers have added a little to the statement while I'm going to add some right now even though we don't have it in this report, but I'm going to base this on our

Done

my observations on all these trips we've made to Necker and we did see them on the broad shelves, especially on Northwest Cape, the Shark Bay side. They seem to favor the higher elevations more so than the Sooties, however, the Sooties are found I guess about all elevations; in fact on Northwest Cape they're just out of the sprays of the waves, that is the general run of waves that they have there. You won't find the Gray-backs down as low as the Sooty Terns; they're generally up on the top. There are all these broad shelves all along Shark Bay side of Northwest Cape and they're also found scattered all along on the higher elevations of the hill. So, on the basis of this I will take exception with Fisher's statement that they are only very sparingly found on the broad shelves, especially since his observations are based on one trip.

The second page - Gray-backed Terns - first paragraph - last line - where you say they frequently nested on inaccessible cliffs on various slopes of the island - I changed the period to a comma and added including the upper parts of Northwest Cape. Then the 1969 data has been added.

This observation by Richardson - one bird seen the 20th of December 1953. I'm just wondering how significant that one bird is? Certainly on our observations on Midway there for a couple of years and I don't know what Woodward and the rest of the boys have found out there on Kure, but boy, when those Sooty Terns come in, they're in and in large numbers. Yet on the 19th of March 1913 Daily Reports, eggs and small young which would correspond with our 1965 trip when we figured there were 50,000 on the island and 20% with chicks. For the island as a whole,

having 3 nearly fledged young and then again we saw the same thing again in 1967, all stages from eggs to fledged young, so perhaps they do start breeding in early December.

Then the next paragraph on population, you say there are too few recent estimates to adequately document maximum numbers. I have crossed out recent, and said that there were too few estimates to adequately document maximum numbers. We have recent estimates and we had 8 estimates since and including 1964 compared to five running from 1902 to 1962, so we should say there are too few estimates to adequately document maximum numbers, and

SI-MNH-332
4-9-64

SMITHSONIAN INSTITUTION
DIVISION OF BIRDS
PRESENT AND PAST MONTHLY SUMMARY OF BIRD ACTIVITIES -
ISLANDS OF THE CENTRAL PACIFIC

* SPECIES ACCOUNT-SPECIMEN
1 1963
2 1963
P POPULATION OR NUMBERS
B BREEDING INFORMATION

	Jan.	Feb.	March	April	May	June	July	August	Sept.	Oct.	Nov.	Dec.

certainly scratch out that recent. You go on to say that these estimates indicate that the Sooty Tern is probably the most abundant breeder on Necker. Let's just take the probably out there, they are the most abundant breeder on Necker, with the possible exception of the Common Noddy, and when I checked back through our figures I still say that the Sooty Tern definitely is, Roger, and not probably, so let's scratch the probably out.

The second page on Sooty Terns line 8 beginning with "over the island", you have birds nested on bare rocks on the Northwest Cape and main island. You apparently are referring to the March 1965 report however, since this is being coauthored, it isn't necessary to continually refer to a comment by somebody else in the report, especially in the case where I am one of the coauthors because I have first hand knowledge, and as such I am in a position to state what we have been finding within the last five years, so therefore: I have changed it to read: birds nest on bare rocks on Northwest Cape and the main island and under Chenepodium from above the cliffs to the tops of the ridges. This is fact, not based on one visit, but on a number of visits. Then the 1969 data is added for the table sheet, under

Brown Noddies - first sheet - first paragraph - line 3 - I have changed mid to late March. Also the next sentence beginning with some nesting may occur in all months but in most years. I have lined out some nesting may occur in all months and instead have started the sentence with In most years nesting apparently occurs principally between April or May and October-November. Next, population - in the second sentence - I don't think it's necessary to say since a degree of error in visual estimates/^{for} large populations may be considerable, I believe that just about everybody knows that. Therefore scratch that out and begin the sentence "we cannot conclude that there has been any change in the population since." You begin with annual cycles, and say the various observations on the breeding status of these birds are insufficient to determine whether there is a usual annual pattern of breeding; not so; I believe we can safely say that there is a definite annual pattern of breeding with breeding as we mentioned before beginning probably in early April and going on clear through October-November.

didn't change to what Gene said since evidently was

The next line - observations from 2 recent March visits should read observations from 3 recent March visits and in parenthesis add (1969). Continuing down to line 6 under paragraph of annual cycle - beginning with the word breeding season had begun. Change that to breeding season had begun which conflicts with the March records. Next sentence - next line began by at least mid May but suggested; I'm changing that to began by at least mid May and suggest an egg peak in July and not June, basing this again on what happens on Nihoa too about this time. You have made the statement that the September 1964 observations, and those from September 1966 indicate egg laying occurred in August or September and suggesting a fledging peak of late September to early October. Now I'm beginning to wonder about the breeding report in ^{December} September 1953 by Richardson. The March 1965, 67, 69 visits reveal few birds and none of which were known to be breeding. This confirms pretty much what we found to be the situation on Nihoa so I don't know what Richardson bases his statement on there in 1953 when he said that 20 December 100-150 a definite breeding season beginning. Unless 1953 was very unusual from the other trips, but here again as I mentioned before, March 1965, 67, and 1969 we found no evidence of breeding and very few birds, so this seems to be contrary to what Richardson claims he found there in 1953. So, you're saying then ^{thus} that/the data available show that breeding has occurred in all months, but also suggests that the amount of breeding in some pairs is probably January through March, is considerably reduced. Well, I don't know, / ^{I'm beginning to wonder about that Dec. '53 data.} As I said that certainly is a variance from what I found out in these recent March trips unless there has been a change, which I doubt, so I'm going to suggest lining out thus the data available shows that breeding has occurred in all months but also suggests that the amount of breeding in some periods is probably January through March has been considerably reduced to saying that the amount of breeding in some periods is probably January through March is considerably reduced. Then in the table - on observations - Brown Noddy on Necker Island, and I have included the 1969 information.

The Black Noddy - here we're going to run into a problem on nomenclature since Peterson calls it the White-capped Noddy, our Bureau especially the Banding Office refers to it as Hawaiian Noddy Tern, and here we're calling it the Black Noddy Tern - so I think we should come to an agreement as to what is the name which is in the most common use in the proper name. You might want to talk to John Aldrich about this since these taxonomists move back and forth between minutus and tenuiristris as to what's the Black Noddy, Black Noddy Tern, Hawaiian Noddy Tern and the White-capped Noddy Tern, so instead of putting down what Warren King thinks they should be, I think we should put down what authorities like Aldrich might say.

On your species account - page one - I would agree with you on status. This might be the case with Hawaiian Noddy Tern, probably does not breed in all months every year. On the table - 1962 - none reported by Kramer and Beardsley - in view of what we saw there in July, I'm just wondering whether they were there and they just didn't report it.

*I ignored
I talked to
Gene about
this*

*Black
Noddy*

not true

Under Ecology of Breeding Habitat you have a statement that little information is available on nesting area or nesting sites, let's scratch that out and we'll give you that information. This bird nests primarily in clumps or small holes in the rocks there on Cape in rather steep areas. It does not lay its nests; the Sooty or Gray-backed Tern, but instead in some little hole or pucker on the steep side of a cliff and it is very possible that there might be some nests on the very steep slopes of the main island especially on that north side because we have low rocks down on that north side and have found a few of these Noddies as well as the Blue-gray Noddy come flying out. We frequently see these birds in small groups, oh maybe 50-75, rather small flocks of 50-75 perched out there on the extreme east end way down there on the rocks jutting out of the ocean and they frequently congregate there apparently roosting or resting from their fishing activities. The same would pertain on the far north point of Northwest Cape. These seem to be favored roosting areas.

Mocking Bird - looks ok. Should have had Blue-gray Noddy come next - apparently this Mocking Bird sheet got in there somehow.

Blue-gray Noddy - 1969 data - inserted in the table. The second last line under populations paragraph is that the species is a common resident and that populations are in the order of upper hundreds or lower thousands and I think that we should change that to populations fluctuate from the upper hundreds to low thousands. We don't know enough about the populations to determine whether there is a migration off or whether they are there throughout the year in essentially the same numbers. All we can say is that they do fluctuate on the basis of the visits.

Now getting back to the first sentence in the same paragraph under Populations where you say recent estimates are too few and too variable. Here again, I just think we should scratch out the recent and just say estimates. Certainly our recent trips are more in number than those since 1902. Our September 1968 visit to Necker we did not have time to make any bird counts. The time that we had was spent almost entirely on getting that sign up, putting up the sign, whereas the one botanist we had with us walked around the island looking for Portulaca. Now in 1953, how does Richardson assume that they were apparently beginning to lay unless he found some eggs or some nests.

I think under ecology of breeding habitat all observers report or suggest that these birds nest over much of the island and in particular on the steeper slopes and cliffs. We could add to this that many of the areas are relatively inaccessible and as a result have not been checked. Here again, by rolling the rocks down the sides of the cliffs especially on that very steep north side of the main island you get Blue-gray Noddies and Hawaiian Noddy Terns come sailing out from far down below and undoubtedly they are nesting as well as roosting there throughout the year but how are you going to check them?

You might want to mention the fact that by throwing rocks or stones or rolling rocks down the sides of the cliff that we disturb those down below so that or we should say by throwing rocks down the cliff that we do disturb the birds in that manner in order to get an idea what may be down there and that the Blue-gray Noddies do come out.

Some more information on the Blue-grays is that the time earmarked to '69 I notice that we did not have this entered on a report but most of the birds which we found were incubating eggs.

Now we'll get on to the White or Fairy Tern and here again we're going to have to decide which is the proper usage. I've added the 1969 data onto the table and note again that in our report under comments we hadn't indicated anything at all on the breeding phenology but we found birds incubating eggs and chicks almost full grown, so I'll add that on to the report and we should put this on this account here.

Now in your statement here under populations - you say this apparent lack of seasonal variation is quite different from other Leeward Islands such as Laysan and Lisianski and should be corroborated by more sophisticated survey techniques and visual estimations. What sophisticated survey techniques have you made for Laysan and Lisianski? It's my opinion that just about everybody makes a visual estimation on all these islands unless we really take the time to go into very time consuming transects or complete head counts as far as these birds are concerned. I think we should mention too that in most of these cases these are daylight counts and that undoubtedly the night time the populations would be much higher, so I'm going to strike that whole sentence out and just leave it stated the first sentence which is a fact without a lot of speculation. The next sentence data indicate that breeding occurs throughout the year and suggests

did not comment to Gene on this p.3 in letter ab 19 Feb 70 since his conclusion probably not true

that eggs and young can be found in all months. The data indicates that breeding occurs throughout the year and that eggs and young can be found in all months. If the data indicates breeding occurring throughout the year it also indicates that eggs and young can be found in all months. Here is a situation that possibly should be made probably way at the beginning of the species accounts that in some cases time just did not permit intensive counting or censusing of all the species of birds to be found on these islands. The magnitude of the job is just much too great for the time allowed. Priorities also differ too. Then again the inaccessibility of many of the nesting areas for some of these species such as the Hawaiian or Black Noddy Tern, the Blue-gray Noddy and the Fairy Tern makes estimations I think rather minimal. You can't count what you don't see unless you get an idea what you have seen from a certain percent of the habitat or similar habitat.

Done

And, under breeding habitat you say observations in 1902, 1966 and 1967 indicate that White Terns nested principally on the steep high cliffs. I think that we can just come right without having to cite those and say that they do nest principally on the steep rocky cliffs of the island and let it go at that and just say White Terns nest principally on the steep rocky cliffs of the island. Some also have been found nesting along the higher ridges and that's very true. We found the same thing true in 1957 as we did here on this last trip in '69 and there are times when they seem to be more abundant on one part of the island than the other.

Getting back to the paragraph on populations. We should insert a statement that many nesting areas are inaccessible to humans. These birds are not like the Goonies and the Boobies and the like where we can get to practically all of the birds of the particular species that happen to be nesting on the island. Got my pages mixed up here so we'll go on to the Black-footed Albatross.

The 1969 data has been inserted. On the account page the paragraph of populations, second paragraph beginning with "most recent March estimates" change most to some. Next sentence "on leeward islands" change to on the northwestern Hawaiian Islands.

The number of young fledglings may at times be considerably less than the number of nests with eggs. Not is usually considerably less. This isn't always the case. They of course naturally are less than the number of nests with eggs, there's always the loss there but not always considerably less or, this is not always the usual case either. This certainly doesn't seem to be the case there at Pearl and Hermes. Certainly with this species it doesn't seem to be the case as with the Laysan Albatross.

Getting on down to the last sentence in that paragraph. Where you state "in any case it seems likely that current breeding populations are considerably smaller than indicated by Rice and Kenyon I'd scratch that out entirely and say that Albatross nesting population on Midway fluctuate ^{from} /year to year and there is no reason to believe that those on Necker would not vary also. And as explanation since it is impossible to separate breeding adults from nonbreeders on the island at this late date, this would be the most minimal figure. This is getting on down to populations. In the final paragraph under populations, it is clear however that the 1966-67 populations were either considerably smaller or considerably less successful than in March 1964-65 population I have added March 1964-65 or March 1969 population. Add the sentence "during the latter the entire island was head counted for Albatross and a total of 175 chicks of this species was tallied. A few may have been missed but certainly no more than 10 or 15." Then my explanation that since it is impossible to separate breeding adults from nonbreeders on the islands at this date, this would be a multi-minimal figure. Were one to estimate 25% more for nonbreeders like Kenyon and Rice did in December, populations would be close to 600 or more. Now in this regard, talking to Harvey Fisher and others, I would assume that as the season progresses you're going to get a higher and higher percentage of nonbreeders mingling in there with the breeding birds and many of the breeding birds will be off/hunting food for the chicks. Consequently a considerable proportion of the Albatross present on the island would be nonbreeding birds, so this 25% figure then would probably be a low one and it could very well be that you may have as many nonbreeders there as breeders.

In your paragraph explaining about what takes place on Kure and the like, I have added if Necker is comparable to its nearest neighbor Nihoa, chick mortality would be much lower than at Kure. Observations in September show that albatross remains are far less noticeable or evident than on the flatter islands such as Laysan or Southeast Island at Pearl and Hermes Reef. We just don't see that many dead chicks there on Nihoa or Necker at that time of the year. Why, I don't know, I'm just inclined to think that mortality for some reason or the other is lower than on the flatter islands.

Next page - on annual cycle. Change Leeward Islands to northwestern Hawaiian Islands. This change is going to be taking place continually throughout the text, Roger, if we are to be consistent with this latest name change.

Then on the paragraph on ecology on the second page of the species account. The last sentence, I've added March 1969 observations have confirmed this after the sentence about none listed on Northwest Cape. I don't recall seeing any nesting Black-footed Albatross on the Northwest Cape. The few albatross chicks you see there are almost invariably the Laysan. Then in 1969 on the table, I've added the 1969 March data for Laysan Albatross. Now here in March of 1969, we actually headcounted 510 chicks and may have missed but a few but this probably would be a small handful. So, we consider 510 chicks and double this for your breeding pair, at this time now, 22 March, you would then have a little better than 1,000 breeding pair, breeding birds, added to this year your 510 chicks, so you come up with a minimum population of 1550. Now here again, just how many of the adults were nonbreeders we didn't count adults this trip because of the shortage of time and we felt that the counting of chicks was much more important especially a year to year to year or every so many years because certainly the number of nonbreeding birds is going to fluctuate considerably from year to year and they don't tell us too much. So the 1550 would then be an absolute minimum figure and undoubtedly the populations must run well over several thousand. Now the 510 chicks headcounted is very close to the 550 young counted in 1965 and if we're going to have comparable data with the number of breeding pairs we calculated 1,100 in March 1965 and were we to calculate breeding pair here in 1969 we would then come up with 1,020, which is very close. Now when we checked this data with that of

March 1967 when we only saw about 150-200 half grown young, would then indicate that the breeding population in March 1967 was certainly less than either 1965 or 1969 and based on complete chick counts on Pearl and Hermes Reef the last 4 or 5 Marchs would certainly seem to indicate that the number of breeding albatross or the breeding population on an island will fluctuate considerably from year to year and I think that pretty much confirms or rather is confirmed by work done by Robbins and Fisher there on Midway where at times if I recall correctly, the breeding populatinn may be a third of what was present of what was present the preceeding year. So then, if you then want to start comparing with what Rice and Kenyon saw there in 1957 you're going to have to take this into account that it's very possible that what they saw in '57 may have been a big year for breeders as compared to some of these other years, and I suppose were we to continue checking each year, each March, that we would possibly come out with some sort of cycle there.

Then under annual cycle - you state that several sets of observations in June 1923, July 1964, March 1967, I've added March 1969, reveal that this species has a later nesting cycle on Necker than the Black-footed Albatross. Nests have often been noted in other areas where both breed. And this is true. The Laysan always seem to be a little behind the Black-foot and here again I don't think that the success at Kure is the same as at Necker. We sure don't see many remains of dead chicks like we do on some of the flatter islands like Southeast or Laysan and those we do see at Laysan especially seem to be congregated around the perimeters of the lagoon.

Then getting back to Kenyon and Rice's estimate - there's always the very real possibility that by censusing them from the air they might have mixed some of these up with the Red-footed and Blue-faced Boobies since all seem to be nesting together.

The next species page - the first sentence - it is possible that the breeding period could be briefer than on that island since the Necker population is far smaller. I don't think so, Roger, I've lined that out, I'm inclined to feel that the breeding period would be the same on Necker as it is on Laysan. As far as seeing a chick is concerned, in either one or the other species, I recall, we saw several Black-footed

chicks there on Laysan clear into September, but this could very well be somewhat an unusual case then considering the thousand of birds which nest there, you're going to have a couple of extremely late ones. This trip we didn't observe where all the young were found but the majority were on the slopes of Summit and Bull hill, the upper slopes and scattered all along the ridges with a few over on Northwest Cape, so even if I did specify how many young we found during July of 1964 on the tops of each of the hills, you must remember in July 1964 that the bulk of the chicks have left, however based on our March trips we could safely say that the bulk of the chicks are scattered all over the upper slopes of all the hills. Northwest Cape, however, has usually less than 25-30 chicks. It is not used anywhere near as much as the upper slopes of the hills on the main island.

Bulwer's Petrel looks pretty good. On the table page I have added our 1969 observations, ^{although} number C - a number of holes were searched for their presence with no success - could very well be we may have missed some, but I would think that we did enough looking that if any were around we should have seen at least one.

Wedge-tailed Shearwater - on the first page - second sentence of the first paragraph - present from about March through October - I have lined out about and inserted at least. And the table - I have entered our 1969 data and the interesting thing was that on this trip almost all the birds observed were in pairs.

Getting back to the annual cycle - the sentence beginning in the fourth line down - in the middle of the line - birds possibly court and dig burrows for about 2 months before first eggs are laid, I've inserted, dig burrows where possible.

Christmas Shearwater - we have two March records and the reason for that is listed on the last line of the observations. On 22 March 1969 another was observed. Since observations on other islands during this time indicate that they occur in pairs it is very likely that another one was present.

Sooty Storm Petrel - I think you'd better take that status out as hypothetical and state one documented record. The mere fact that this bird didn't happen to be collected at the time isn't saying that it wasn't there, so you say the bird was not collected ~~and~~ the record cannot be verified; well, by whom?

Please note that I banded two on Nihoa in March 1969 and there were also others, however, I didn't take the time to band them and there's no doubt that the one seen there in 1965 certainly was a Sooty Storm Petrel and could not be confused with anything else.

Red-tailed Tropicbird - no major changes, except on the last paragraph in the annual cycle - last line - where you until late March or April, I scratched out late March and inserted early April. We haven't found anything in March yet. Then the last paragraph I've inserted a sentence "Kridler states that shallow small caves on the ledges along the upper portions of Bull, Summit and Flagpole hills are favored as are crevices on the upper west slopes of Northwest Cape." Were I to look for a nesting bird, I'd drop down below the top of either Annexation or Bull hill, maybe about 1/3rd of the way down and work along the ledges over there and would certainly find if there's any breeding on the island there would be a few there. The 1969 data was inserted. Now here in '69 we did not have a chance to work too much along those ledges and so we didn't find any young.

Blue-faced Booby - 1969 data has changed the maximum recent estimates from 250 to 500. The third line - the sentence - nests on the ground on the higher slopes, I have added "and on ridge tops". Now you have a sentence here "thus we suspect that the Necker mid-summer populations do not exceed 300-400 birds." I say "thus we suspect that the Necker mid-summer populations do not exceed 500-600 birds." And where you say that this figure is considerably less than that reported for June 1923 by Wetmore suggests that fewer birds now nest on Necker, I've said this figure is less than that reported for June 1923 by Wetmore. And I'm wondering just how accurate Wetmore's figures were. If accurate, how representative, since it was for only one year and just a very brief period for that year. And we know that populations will fluctuate, so how can we suggest that fewer birds now nest on Necker when we compare data from a number of springs here recently to one record going back there in June 1923?

And, on the next page - ecology and breeding habitat - on several subsequent visits June 1923, July 1964, March 1965, 1967, I've added "and 1969.

Then on the next page table I've inserted 1969 data and here again we had an actual nest count of 230 nests and here again it's possible we might have missed 5-10

but not very likely, because we got excellent counts. And a sample of 101 nests reveal 6% with no eggs, 5% with 1 egg, 58% with 2 eggs, and 15% with 1 young and 60% with 1 young and 1 egg which should give us a pretty good idea as the nesting phenology of this particular trip. Actually, I inserted the data on the bottom of the first page of tables and it should be over on the second where it belongs, so this then pretty much takes in the species accounts, Roger. You might wish to rewrite it on the basis of this latter data and my comments, it certainly should be but as mentioned before, the fact that Wetmore and Richardson are more or less well-known names in ornithology shouldn't be any reason that their figures should be accepted any more than anybody else's especially if they didn't make any actual headcounts. I do have some questions on the reliability of some of the data especially that from the aerial estimates as compared to good ground coverage. Unless aerial photographs can be counted under a mike, under a sampling system, or completely, in many cases they are less reliable than a good ground count. Unless you're counting ducks over a pond and can't see them from the ground.

I'm going to send you a copy of our March 1969 report which will also have data which will be of value to you in the island accounts for some of the other islands. Now, I'm going to check over your history page and see how that looks.

Eugene Kridler to Roger B. Clapp

January 1970

This belt, Roger, contains answers and comments relative to the questions you asked in your letter of November 19th, 1969.

Your first question on Nihoa-Necker - you say "in this report for both Nihoa and Necker, that is the March-April 1969 report, you refer to the trip taken in 1968 as having been made in September." You're talking about two different trip reports so according to the tenor of your first question I assume you're talking about the fall trip of 1968. I had listed the Necker dates as 28-29 August and gave Nihoa as 24 and 27 of September. The latter is an error. August is the correct month for 1968. We were on Nihoa from August 24th through August 27 and then on Necker August 28th through August 29th, so that should settle that.

Now, question two. What is the significance of your class data system A,B,C,D, so forth? We started classifying our data as to what we figured was the reliability. This then will give other people that follow or others that read our reports some idea of whether this was just an educated guess or whether it is very good, firm data, so in essence it's as follows.

Class A data is essentially a head count with very little room for error. Most of the Class A data will really pertain to all areas like Pearl and Hermes and some of the others where we actually went out and counted almost every individual.

Class B data - are all data which are + and - 20% error with a 95% confidence level.

Class C data would be + or - 50% and a 95% confidence limit.

Class D data is essentially an educated guess.

Now your next questions pertain to Nihoa and here I think that you are referring to our March 19-April 6, 1969 trip. You ask on p. 3 under Sooty Storm Petrel Remarks that 2 individuals were banded. This is the first record of nesting of this species on Nihoa, can you give me additional information on this? I went back to my field notes and this was a pair of adults which were taken out of a burrow about one-half the way up Nora Canyon. I took the birds out and banded them and put them back in again. I saw a number of other pairs in the little puchas and nooks and crannies as I was going up Nora Canyon but we didn't have the time or I didn't take the time to do anymore banding because we were trying to get ~~our~~ transects for the Millerbird and the Nihoa Finch out of the way because we just had one day on the island and as I had mentioned earlier we never were able to finish the latter because of an unfortunate event whereby the Coast Guard forgot about the Emergency signal and blew it while they were engaged in a routine man-overboard drill. So, about the Sooty Storm Petrels - this was an adult pair taken from a burrow about one-half way up Nora Canyon.

Question no. 4 on page 4 under Blue-faced Boobies - you comment that young had hatched. I went through my field notes again and in my notes I have eggs to flying young and mostly small young. The report itself was written by Dave Olsen and he was a little bit brief on some of these comments. Therefore, once again, it should be eggs to flying young with mostly small young. Here again, some of the fellows did forget to record the percent which were in the egg stage as well as the small downy young stage or the half-grown young stage and the like. Then I never was able to get up to Miller Plateau where the bulk of the Blue-faced Boobies were nesting. I just got up to Miller Peak and was on my way down the other side

Nihoa

- need to
✓ results
table

- SSP

- BFB

N. haas

to the Plateau when the ship had blown its whistle and as a consequence I never did get over to the Plateau. The data then would be considered as Class C. It's a little better than Class D but certainly not nowhere near as good as if I would have been able to get over to the Plateau.

Question 5 - For both Brown Booby and Red-footed Booby the comment is eggs still being incubated and you say I assume this means that no young were seen and that most ~~nests~~ contained eggs. My field notes show that most of the Red-footed Booby nests contained young although there were quite a few that still had eggs but I wasn't able to conduct a sample count to get percentages. As far as the Brown Boobies were concerned, we had them anywhere from eggs to downy young as well as some immature birds which were flying.

RFB
BB

Now no. 6, no comment is entered for either Gray-backed Terns or Blue-gray Noddy. Does this mean that no birds were nesting or merely that no attempt was made to look for nests? No attempt was made to look for nests, Roger. Although as I recall I don't have my records with me here now, because I'm dictating to this tape at home and I didn't bring everything with me. It seems to me that at this time of the year there would be quite a few Blue-gray Noddies nesting although we didn't make any attempt. The fact that there was no comment after this does not indicate that the birds were not nesting. I suspect they were. It's just that we weren't checking where they nest. The same would apply to the Gray-backed Terns.

GBT
BGN

On page 7 under Necker - Christmas Island Shearwater - Birds seen. I have nothing in my notes but it's possible that one of the other fellows may have seen a Christmas Island, I don't recall at this time. Just kind of rummaging around at the back of my mind it seems to me that the bird was seen on the top of either Flagpole Hill or Summit Hill, but I'm not sure. I think I pointed it out to George Laycock (?) who was with us but I don't have it in my notes. Now, we spread out over the island and every man was assigned several species and he devoted himself to only those species. The figure you should use would be 850.

Yes, in the material which I sent to you we had listed Sooty Terns as 25% on eggs. Going back to my field notes I note that I had corrected the report to 75% and the 75% is correct, checking my reports I have Sooty Tern eggs very fresh and about 75% had laid, so use the figure 75.

On your question 9 under Hawaiian Tern - you state that approximately 80% on eggs. Does this mean that the rest were not nesting or that they were on young? This means that the rest were not nesting. Even though on previous March trips we did see at least 1 young, apparently they were a little later this year or we just didn't see any young. Now, as you recall on the south slope of the island itself was quite sheer and there are Hawaiian Noddies down in there and it is possible that there might have been some nests down there that contained young but we never have been able to get down there because of the sheerness of the cliff.

(He corrects himself as follows) Page 7 Necker Christmas Island Shearwater the same would also apply to questions 8 and 9 which would all concern Necker.

Question 10 would also apply to Necker. I note in a report that you and John Sincock did a vegetation map. We did, we type mapped the island but the map isn't fully prepared yet. John Sincock is working on it but he

hasn't completed it as yet, not to my knowledge anyway.

Now we get to Laysan. I list 3 Green-winged Teal, Roger. Not one of them was a male that I could determine to be a male. They were all female type birds and here again, this time of the year, if there had been a drake there, even an immature one, he should have been pretty much along in his full courtship plumage, so since none of these birds resemble anything close to a male they obviously were females. And of course we can't pin it down to subspecies because the birds were seen in flight, and I had a pretty good look at them as they were flying, having seen hundreds of thousands of Green-wings, I'm sure that they were Green-wings but I couldn't tell you what the subspecies was.

The Pintails - checking my notes - actually there were 5 birds and not 3, so there is an error in the report. The 5 birds seen were 2 males and 3 females. Here again I had a pretty good look at them, so the report should be corrected.

Now the 14 Shovellers seen - these were all counted as they were flying away and we made no distinction as to whether they were males or females. At this time of the year the shovellers many of the drakes are a lot farther behind acquiring their spring or courtship plumage and most of them were the species of Dabbling ducks and I don't recall just who made the observations, I think it was mine but going back to my field notes I have nothing on the sex other than just that 14 shovellers were seen.

The breeding status of the Blue-faced Boobies - I think somebody else was counting the Blue-face, no here we are. I checked 15 nests on the Blue-face and of these 9 contained eggs, 4 did not have any eggs, 1 contained a chick, and also another one contained a chick with one egg, so therefore you have 9 with eggs, 4 with no eggs and 2 with chicks. One with one single chick and one with a chick and an egg.

Now for the Red-footed Boobies - I checked 123 nests and of these 40 contained eggs, 20 were fresh nests but were empty, and 89 were birds on nests which I did not check. I just counted them as sitting on their nests but never was able to get in there and find out what they were doing.

Now for the Frigatebirds - I counted 390 birds on nests and of these which I checked 48 were incubating eggs, 32 of the nests did not contain eggs, and 310 of the nests I was not able to check to see what was under the birds sitting on the nests, so I'm going to enter that right down in that report here too. You see, I gave some of my field notes to Dave and he consolidated them and was a little bit on the brief side. As far as I know nobody else checked any of the nests of the Red-foot or the Blue-faced Booby or the Frigatebird to see what they were doing, they just counted birds on nests. The reason for some of this checking and not checking is that here again, when we are making our Finch transects we divided the island between the members of the party and each one was not only responsible for making his Finch transect through this particular section of the island but was also responsible for checking or counting the seabird species and some apparently did not understand that they were to make a check of some of the eggs or perhaps forgot about it, so the only data that I have are the field notes and I recall that I think I was the only one who was making any attempt to get any idea of what percent of the nestsmight contain eggs, what percent of the nests had chicks, and so on.

Here is another note I have about the frigate. I say that a female frigate had two eggs in one nest. I just don't know how unusual this is but I think this is the first time that I have seen two eggs in one nest, now whether they were both fertile or not, I didn't have time to check.

So then, this will be here for all these other species that I've just been talking about. This is the reason for the discrepancy, we all counted nests wherever we could but apparently I was the only one that tried to check the status of some of them.

Hawaiian Noddy Terns - on your percent with nest count is that a sample of 700 nests or different sample number that you took? If so, how many were in the sample? Here again, nobody else checked, but I did and I checked 100 nests and this was distributed among several colonies. Now I did find that we had some small colonies scattered around the north side of the lagoon and the birds were nesting in the Seavola up near the sandy beach part. I also recall that there was a very small nesting colony on the southwest corner of the lagoon, here again in the Seavola, fairly close to those two coconuts way down there on the edge of the lagoon. My notes say there are a colony of 150 nests in the Scaevola north of the lagoon. I checked 100 nests scattered around of Hawaiian Noddy Terns including some which were in the ironwood tree at the landing. So this then is a distribution of 100 throughout a number of the colonies and 100 nests checked, 71 had eggs which were well incubated but I don't know just how many days but I broke open several and they had relatively large well developed embryos. Also, of the 100 sampled, 24 contained small downy young and 5 were new but empty.

Your 14th question - have you any additional information whether Sharp-tailed Sandpipers were seen? If they were not mentioned, they were not seen. Just a minute now, I take this back. On page 14 of the report I have 1 Sharp-tailed Sandpiper seen loafing and feeding along the lagoon and as I remember it I scoped the lagoon pretty thoroughly checking to see if there were any oddballs there. The Sharp-tailed Sandpiper observation then for Laysan, for this April period was mine, because I have it here in my field notes. Sharp-tailed Sandpiper - 1 and the date it was observed

was March 27, 1969. I have a notation here on Common Noddy. One Common Noddy on egg and also 1 incubating a rather large but still downy chick. On the Sharp-tailed Sandpiper change that to 2 seen. My notes show one seen earlier and one seen later the same day. I'll dig around in my July 1967 trip notes and see what I can dig up when I get back to the office.

You say August-September 1968 trip you fill in personnel for several visits - Nihoa-Necker, but I need this sort of information as well as perhaps as general accounts for the rest of the islands visited. I don't quite understand what you're asking for, Roger. All of the trips the personnel would be the same for all the islands with the exception of August and September 1968. In August-September 1968 that particular trip the two Geological Survey people Richard Dill (?) and Dalrymple (?) landed on Nihoa and Necker with us and went as far as French Frigate Shoals where they flew back early the next day or even later that same day we landed, so they did not continue on and make the rest of the trip with us. Therefore, for Laysan Island there were just the 4 of us, John Sincock, myself, Darryl Herbs (?), and Bob Eddinger, and after French Frigate Shoals the only other island we were able to visit was Laysan. The Coast Guard dropped us off and picked us up eleven days later.

Now, your next paragraph I don't quite here again understand. You say judging from what you print in the scientific table I guess there isn't anything to add to Necker and I think I have most of the bird stuff for Nihoa from when I was out with you. I think however that in your earlier letter commenting on a Nihoa report, you made some statement to the effect that you got some pretty good population data on Laysan. We don't have that or anything from islands beyond Necker. I think we've sent you just about all the reports for all the trips which we have made for all the islands. Is this what you mean?

Now your next paragraph you say I also lack any notes from the February 1969 visit to Pearl and Hermes Reef. A copy of the report is being sent right along with this belt, so you should be able to extract what you need from that.

Lisianski - I'm sort of puzzled by the population estimates for Fairy Terns and Hawaiian Noddy Terns. Here again, I have to consult my notes. You say Dave Olsen tried to consolidate the data from notes and I think he made some errors during the transcription, so let's see what it looks like.

Under Hawaiian Noddy Tern - 30 nests, under population. That's an error. The correct count would be 260 and they are as follows: 60 nests were found in the Seavola just around the single lone growing coconut tree on the east shore. The other coconut tree, by the way, had its top blown off, or is gone. Then there is an ironwood tree to the south of that, 100 or 200 yards now, I walked down into that and counted 120 nests in that particular ironwood tree. Then on the south end or near the south end, there's a ^{live} Casurina (?) there and I counted 60 nests in that particular Casurina or ironwood tree on the south end. Then there's a dead Casurina a little bit near that and I counted 20 nests in that. Now it strikes me that either we overlooked those Casurinas just north of the landing on the west side or they didn't have anything in there but it doesn't quite seem possible that there wouldn't be any in that particular tree, but here again I don't remember for sure. Now getting back to that first colony seen in the Seavola at the base of the coconut tree, approximately 50% of the nests contained young, and the other 50% were mainly eggs, there might have been 1 or 2 empty nests there.

Fairy Tern- you're right, the Fairy Tern and the Frigate data is identical and the figure which should be used for Fairy Terns is 13 but there are not 13 nests, these are just 13 Fairy Terns seen, although I'm

trying to remember whether some contained eggs or not. I don't have any notation of any eggs in my field notes here, so therefore the comments for Fairy Terns are: 32 nests with eggs, 14 nests without eggs, etc., is Frigatebird data.

Question 16 - I gather that the nest counts on Frigatebirds, Blue-faced Boobies and Red-footed Boobies was very exact but there's no class data entered for Brown Boobies. Yes Roger, this is a complete count so it should be Class A data. Just let me check my notes just to be sure. Yes, this is a complete count, so enter the symbol A for class data for Brown Booby. The Gray-backed Tern nests, enter that as Class A also. The only notes I have on the Gray-backed Tern is that most contained chicks, some rather large.

Now questions on the May-June 1969 report - if it's all right with Dave I think I'll use part of his account on Necker Island and Necker Island history as his experience with the sharks should certainly get across how difficult it is to land on that island. Go ahead and use it.

Question 17 - would it be all right if I stated to the effect that most birds have young - this is on Gardner Pinnacles. This is ok, go ahead.

Question 18 - Dave says yes, he can remember that some of the Blue-faced Boobies had rather large downy young while other nests had eggs and he says that the Common Noddy nests with eggs was noted but he doesn't recall anything about the Brown Boobies. And the absence of breeding Sooty Terns is incredible and Dave says yes, he thought so too and considering that any shelter on Gardner Pinnacles I don't see how any of these birds get anywhere where they would be protected by the winds for any severe storms in that area I would think would have rather devastating effects on some of the nesting populations.

Your last comment about the Monk seal data - why don't you go right

ahead and write up the comments. It doesn't look as if I'm going to get to it and rather than hold this thing up, why don't you just go right ahead. We just made one correction - May 29, one was seen by Dave Olsen on Nihoa. The only thing lacking is a comment from Sincock there in June this year. I'll try and dig that up.

Necker Island for the Monk Seal - 1968 August - several were seen there but we didn't have time to make a thorough check. We just were so engrossed in getting that damn sign up, also we're sending you our field trip report for August-September 1969.

Eugene Kridler to Roger Clapp

January 1970

On Scientific Visits to the island, Roger, I have inserted on your sheets the visits made in August 1968, March 1969, and May 1969, the persons visiting the island, the purpose of the visit, and any remarks.

Now in September 1968, we spent a good share of our time (John Sincock and I) erecting the Refuge sign with some help from the graduate students we had with us and a little bit more from the Geological Survey people who drilled the holes in the rocks for us so we could insert the base of the sign in the rock and cement it in. This did not give us any time to look over the island at all, John and I anyway. We did camp on the island and there were an immense number of Common or Brown Noddies there on Northwest Cape as well as Sooty Terns but the next morning we went right back again to the erection of the sign because the Coast Guard was kind of in a hurry to get on the way. The only opportunity I got was (John didn't get hardly any chance at all to look around) to climb to the top of Annexation Hill and just kind of look over the general island. (Herbst?) The one Grad student we had with us wasn't too helpful, the other one/is getting his degree in the Portulaca of Hawaii and as a result, although he made a brief botanical survey, he found nothing new other than the five plants which are established there and spent most of his time collecting Portulaca and looking it over. The two Geological Survey people from the Menlo Park office there in California, Dick Duell(?) and Ben/^{Dalyrimple}(?), helped us with the sign and then they collected quite a few core samples of the rock which they took back with them to test for the magnetic properties of the rock but as yet we have not received any report from them on their findings.

The March 1969 trip, we thoroughly combed the island, counting just about everything that was on it with the possible exception of getting way down into the bottoms of the slopes especially on the south side, the very steep south side of Necker, the lower slopes. In some ways we probably shouldn't have even landed on the island, the surf was pretty rough, but we made a landing without too much problem, but when it came time to get taken off by the Coast Guard in the late

afternoon, there were tremendous seas pounding into the landing site, estimated at somewhere around 25-30 feet and it took us almost an hour to get off with considerable difficulty. By the time it came time for our John Sincock and I to get off there were 3-4 and sometimes 5 foot of water smashing over and crashing over the platform itself and on several occasions it washed John and I back against the backwall and banged us around a bit. We all managed to make it off with only a few lumps here and there. I'm sure it would be an adventure which you could very well do without. I think that somewhere in this report, possibly in the general discussion of Necker, we should indicate the hazards, the extreme hazards, involved in the landing on the island.

Then in May, Dave Olsen looking for a different way to land on the island other than on that west side, moved in with a rubber boat quite a bit into La Shark Bay and swam ashore and as you can see in the May Report, he experienced or had quite an adventure with sharks in getting ashore and this of course is another very real problem, in any landings on Necker there is the high populations of sharks around the island.

Now in all island reports we should state the islands are part of the National Wildlife Refuge, Hawaiian Islands National Wildlife Refuge, under the jurisdiction of the Bureau of Sport Fisheries and Wildlife with immediate administration by the Wildlife Administrator here in Kilauea, Hawaii. Also, that the islands have been designated as national natural research areas or natural areas by the Director of the Fish and Wildlife Service and as such the management is essentially to let natural events take their course as much as possible. Thus, we do not manipulate the Refuge as anywhere near as much as what we would on many of our mainland areas, the management objectives being different and also the landings on the islands are restricted to scientists under the permit from the Bureau of Sport Fisheries and Wildlife only. General public use is not encouraged. One of the main reasons for this of course, is the possibility of the introduction of exotic weeds in the forms of seeds carried in the clothing or equipment, the possibility of the introduction

Eugene Kridler to Roger Clapp

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of some pest insect, and, of course, the possibility of predatory animals such as the rats or mice. I think also as far as hazards of landing on the island, the rocky islands like Nihoa and Necker and Gardner Pinnacles especially, could all have some mention made in the account of the extreme hazards involved and there have been times especially during the winter and spring months, when landings on the flatter islands like Laysan, Lisianski and Pearl and Hermes Reef, have also been hazardous and in several instances landings were postponed or eliminated entirely because of dangerous surf conditions. I believe that you are a member of the party there on Pearl and Hermes Reef in March 1965, that experienced quite a bit of difficulty in coming out of Pearl and Hermes Reef because of storms.

On your chronology of Necker Island, for the 1960's, I have entered in the data for the last three trips as well as the information regarding the Executive Orders and Presidential Proclamations establishing the islands as a refuge as well as the authorities for them. This should also be included right at the beginning on the history or at least a general discussion or summary of Necker Island. The first of course is back there in February 3, 1909, Presidential Executive Order 1019 by President Theodore Roosevelt establishing the Hawaiian Islands Reservation and Necker being included with it as one of the islands. They placed it under the jurisdiction of the Department of Agriculture. Then in 1940 President Franklin D. Roosevelt in Presidential Proclamation No. 2416 changed the name of the Refuge to the Hawaiian Islands National Wildlife Refuge. This order by the way is dated July 25, 1940. Jurisdiction was with the Department of Interior, the Fish and Wildlife Service, and it was about ⁱⁿ 1955 and the reorganization of the Fish and Wildlife created two bureaus, the Bureau of Commercial Fisheries and the Bureau of Sport Fisheries and Wildlife which now has the jurisdiction. I think that on your third page on the History of Necker, after your second paragraph, you might wish to quote Emery (sp?) on page 122, Bulletin 53, or at least that

last several pages when he discusses the type of marae as being found in inland Tahiti which were some of the more ancient forms of marae found on Tahiti as contrasted to the later forms of marae brought in from Marae Taia (?) around the 12 and 13th centuries. So he then concludes his Bulletin by saying "therefore unless or until evidence to the contrary is brought forth, it seems reasonable to adopt the view that the Necker culture is a pure sample of the culture prevailing in Hawaii before the 13th century and that prehistoric as well as the historical Hawaiian culture may be considered Tahitian in origin." Now in regard to this charcoal and wood found in Bow Cave in 1923 and carbonated by Liddy, note that the age was placed at around ¹⁶⁶ years \pm 200 years, which certainly is a wide range. Now, it's very possible that somebody could have landed on Necker and stayed there a while and not been part of the original peoples which Emery suggest, possibly around 700 years ago and where Liddy dates it as possibly 166 years \pm 200 years, one could assume that it might go on back to the early 1700's or rather early 1500's or as late as the 1900's, with the possibility that it could have been from one of the early ships that landed there, in historic times. Where they found the charcoal and wood, ^{if} they had to dig down in the dirt for it, there is a good possibility it is from one of the very early ones. So, then going back to Emery's last statement in his book, he feels that it would be reasonable to adopt the view that the Necker culture was a pure sample of that prevailing in Hawaii before the 13th century.

I think that when mentioning ship names in all your historical accounts, it might be a good policy to capitalize every letter in the name of the ship. In your page where you say some months of political pussyfooting followed. I'm just wondering whether the term pussyfooting should be used or "political maneuvering" should be substituted instead.

This will then conclude this series of comments, Roger, and I'm going to put the answers to the questions you sent in your letter of November 19 on a separate tape, so I'll see you on another tape.

I think this pretty much takes care of your answers, Roger, if you have any more, shoot them back. I'm sorry about the delay in getting this to you but this thing came right of the midst of rather feverish preparations for a big meeting here on the rare and endangered wildlife. We had the Assistant Deputy Secretary of the Interior for Fish and Wildlife Marine Resources here; we also had Director Gottschalk (?) and then our Regional Director and what with making all the preparations for the meeting, preparing itineraries, and making arrangements for field trips for them to the neighbor islands during December, and some of them stayed clear on up until Christmastime, so as a result, everything else had to be put on the back shelf and this really put us in the hole. I was hoping to take some vacation time and as it stands, it looks as if I'm going to lose close to 80 hours of vacation leave which comes to just about ten working days. Then in a couple of days I have to shove off for Portland for about a week for a number of things and it looks as if I might just be relieved of a number of my other duties here to concentrate on the rare and endangered program here in Hawaii. This then means that the rest of the load will depend on poor Dave Olsen's shoulders. We still didn't get our wilderness report out and that thing is beginning to be a little on the overdue side, and this is something we just have to get out.

So with this then, Roger, I'm going to close and shoot this thing off in the mail tomorrow. It's getting quite late, it's almost past midnight and I'm going to hit the sack. So, in Hawaiian, I'd like to thank you for your Christmas card and may we wish all of you back there (can't spell the Hawaiian-AB) a Happy New Year!

(2)

Birds Returned by E. Kridler, et al on Southeast Island, Pearl and Hermes Reef
 During the Period Feb. 10 1969 through Feb. 12, 1969. Data Received from
 Eugene Kridler, 20 October 1969

Black-footed Albatrosses (76)

737	<u>Original Banding Data</u>	737	<u>Original Banding Data</u>
Not Found 01273 [†]	Not found in Kridler BSWF list - If prefix were 767 then: 73 Lu-SE - 21 Mar. '65 97 Au-SE " 40 Au-SE " 61 Au-SE " 83 - Au-SE "	32397 ***	Au-SE - 1 March 1963
" 02097 [†] SDC		32460	" " 3 March '63
" 02140 [†] SDC		32472	" " "
" 02261 [†] SDC		32554	" " 4 March '63
" 02383 [†] SDC		32691 ****	" " 6 March '63
31034 SDC	Au-SE - 26 Feb. '63	32702	" " 7 March '63
31240 SDC	" " 27 Feb. '63	32747 SDC	" " "
31250 SDC	" " "	32789	" " "
31293 SDC	" " "	32819 SDC	Lu-SE - 8 March '63
31436 SDC	" " "	32923	Lu-SE - 8 March '63
31531 SDC	" " 28 Feb. '63	33056	Lu " 4 March '63
31804 SDC	" " "	33237 SDC	Lu " "
31822 SDC	" " "	33698 SDC	Lu " 8 March '63
32039	" " "	33703 egg	" " "
32016 SDC	" " "	34105 SDC	Au " 1 March '63
32116 *	" " "	<u>Green plastic band, No. #</u>	
32205 SDC	" " 1 March '63	34123 SDC	Au " "
32227	" " "	34128 SDC	Au " "
32301 **	" " 28 Feb. '63	34211 SDC	Au " "
32308 SDC	" " "	34219	Au " "
32339 SDC	" " 1 March '63	34288 SDC	Au " "
		34308 SDC	Au " 2 March '63

* paired with 32397
 ** paired with 32691

*** paired with 32116
 **** paired with 32301

Black-footed Albatross (cont.)

<u>737</u>	<u>Original Banding Data</u>	<u>737</u>	<u>Original Banding Data</u>
34411 SDC	AU-SE - 2 March 63	42025 [†]	Don't have in BSW - Not P&SP list from Kridler
34419 SDC	" " "	25785	LU - Grass Island - 26 June 1963
34588 SDC	" " 3 March 63	767	
34702 SDC	" " "	01875	BSW - LU-SE - 21 Mar '65 Not P&SP
34708	" " "	02006 SDC	BSW - AU-SE - 21 Mar. '65
34726 SDC	" " "	02023 SDC	" " "
34787 SDC	" " "	02024 SDC	" " "
34797 SDC	" " "	02106 SDC	" " "
34913 SDC	" " 4 March 63	02163 SDC	" " "
35046 egg	AU-SE - 6 March 1963	02176 SDC	" " "
35085 SDC	" " "	02187 egg	" " "
35092 SDC	" " "	02260 SDC	" " "
35098 SDC	" " "	41510 [†]	listed on schedule as Blue-faced Booby - All- banded on Lissianski 12 March 1965 (and returned as such on Lissianski in March 1968)
35102 SDC	" " "		
35169 SDC	" " "		
35363 SDC	" - 7 March '63		
35405 SDC	" " "		
35074 SDC	AU-SE - 6 March '63		
37159 [†] SDC	listed on schedule as NU - Great Frigatebird - Banded North, 23 June '63		
39011 SDC	LU-SE - 20 June 1963		
39053	" " "		

Blue-faced Boobies (14) 9 M, 4 F, 1 U

<u>737</u>	<u>Original Banding Data</u>	<u>737</u>	<u>Original Banding Data</u>
30163 F	AU - Southeast - 27 Feb. 1963	47530 ⁺ M/2	Schedule lists as B.F. ** eggs Albatross AU-banded Eastern, Midway - 17 Dec. 1963
30176 M	AU - Southeast - 27 Feb. 1963	47531 ⁺ M	" " "
30202 M	AU - Southeast - 27 Feb. 1963	767	
30211 M	AU - Southeast - 27 Feb 1963	47529 F	AF - SE - 22 Mar. 1968; w/2 inc. egg Pr. / 558-83479
30222 F	AU - Southeast - 27 Feb 1963	47536	AM - SE - 23 Mar. 1968; w/2 very sh. inc. eggs Pr / 737-30201
30225 M/egg	AU - Southeast - 27 Feb 1963	588	- Not PRRP Prefix - If 558 then
30219 M	AU - Southeast - 27 Feb. 1963	83466 ⁺ M	NU - SE - 19 June 63
30262 M/egg	AU - Southeast - 28 Feb. 1963	83481 ⁵⁵⁸ F	NU - SE - 19 June 63

Red-footed Boobies (13)

<u>587</u>	<u>Original Banding Data</u>	<u>737</u>	<u>Original Banding Data</u>
83795	A.U. Southeast 22 March 1968 on empty nest	30402	AU - Southeast 1 March 1963
91131	S-U - North - 29 Aug. 1967	38082 ⁵	AU - Southeast 20 June 1963
737		95942	AU - Kure Atoll 12 Dec 1961
30070	AU - Southeast 26 Feb. 1963	757	
30072	AU - Southeast 26 Feb 1963	27618	SU - Trig(?), F.F.S. 13 Aug 1966
30238	AU - Southeast 28 Feb 1963	43065	AU - Southeast, 25 Sept. 1966
30256	AU - Southeast 28 Feb 1963		

Brown Boobies (6)

<u>737</u>	<u>Original Banding Data</u>	<u>737</u>	<u>Original Banding Data</u>
30135 /egg	AU - Southeast - 27 Feb 1963	30287 /egg	AU - Southeast 28 Feb. 1963
30282 [*] /egg	AU - Southeast - 28 Feb 1963	37569	AU - Southeast 22 June 1963
30296 /egg	AU - Southeast - 1 March 1963	767	
		00037	Not PRRP band - My list gives this sequence as a BCFW band but this particular band is not listed - Previous band is a male Brown Booby SE - 21 Mar. 66

** with probably result of misread prefix. If 767 instead of 737 -
then 30 banded SE, 22 Mar. 68, AM, w/2 fresh egg; 31 banded SE, 22 Mar. 68, AM w/inc. egg

* on original schedule as a Red-footed Booby

+ - numbers double checked against original list sent by Kridler

List of Bird Returns and Recoveries Sent to R.Clapp by E.Kridler in letter of 20 October 1969

Band Number	Species	Where, When, and by Whom Banded	Age When Banded	Where, When, and by Whom Returned/Recovered	Remarks
767-00377	Blue-faced Booby	Necker Island March 15, 1965 H.I.Nat.Ref.	At least one year old A-U	Necker Island March 10, 1967 B.S.F.W.	
662-06052	Ruddy Turnstone	Lisianski Island March 11, 1964 E.Kridler	At least one year old A-U	Lisianski Island March 20, 1968 E.Kridler	Captured and released
662-05888	Sooty Storm Petrel	Southeast, P.&H. March 21, 1965 E.Kridler	At least one year old A-U	Southeast, P.&H. March 22, 1968 E.Kridler	Captured and released
662-06053	Ruddy Turnstone	Lisianski Island March 11, 1964 E. Kridler	At least one year old A-U	Lisianski Island March 20, 1968 E. Kridler	Captured and released
662-06044	Ruddy Turnstone	Lisianski Island March 11, 1964 E. Kridler	At least one year old A-U	Lisianski Island March 20, 1968 E. Kridler	Captured and released
682-16128	White Tern	Laysan Island March 27, 1966 H.I.Nat.Wild.Ref.	At least one year old A-U	Laysan Island March 18, 1968 E. Kridler	Captured and released
793-03025	Ruddy Turnstone	Lisianski Island Sept. 25, 1967 H.I.Nat.Wild.Ref.	A-U	Lisianski Island March 20, 1968 E.Kridler	Captured and released
757-88153	Laysan Albatross	Lisianski Island March 11, 1964 E.Kridler	L-U F	Eastern Is., Midway Feb. 28, 1969 H.I.Fisher	Captured and released
767-01353	Black-footed Albatross	Southeast, P.&H. March 21, 1965 H.I.Nat.Wild.Ref.	LU	4 S Florence, Oregon July 5, 1968 F. Walcome 124°00'E, 43° 50' N.	Dead, band removed Found dead

Band Number	Species	Where, When, and by Whom Banded	Age When Banded	Where, When, and by Whom Returned / Recovered	Remarks
767-00067	Black-footed Albatross	East Is., F.F.S. March 24, 1966 H.I. Nat.Wild.Ref.	L-U	14 W C Sebastian, Ore. Sept. 23, 1968 P. Ivanow 124°40'E, 42°10'Nv	Entangled in fishing gear. Released, band left on bird
61-171706	Millerbird	Nihoa Island Sept. 24, 1964 E. Kridler	U-U	Nihoa Island Aug. 25, 1968 E. Kridler	Recaptured and released
757-88911	Laysan Albatross [Kridler band list in Clapp's possession gives island as Laysan†]	Lisianski Island March 11, 1964 E. Kridler	Too young to fly when banded L-U	At Sea 142°27'E, 32°34'N. Feb. 14, 1969 Dr. Tatsuo Udagawa	Entangled in fishing gear
767-00087	Black-footed Albatross [My list of BSFW gives date of banding as Mar. 24, 1966 **]	East Is., F.F.S. March 3, 1966 H.I. Nat.Wild.Ref.	N-U	At Sea 177°41'W, 39°57'N. July 11, 1966 USSR Acad.Sci. Ringing Centre	Shot
767-02122	Black-footed Albatross	Southeast, P.&H. March 21, 1965 H.I. Nat.Wild.Ref.	A-U	At sea 177°30'E, 37°20'N. May 16, 1967 USSR Acad. Sci. Ringing Centre	Shot

* Almost certainly error in Clapp list as all other March 11 dates appear to be Lisianski and all Laysan dates as March 10, 1964

** Error by banding office as Kridler 1966 report gives dates of visit to F.F.S. as 21-24 March

Roger - I stuck a note on
the letter to Gene and said
that # 18 3 lines from
bottom "weather" was probably
"winter".

November 19, 1969

Mr. Eugene Kridler
P. O. Box 151
Kailua, Oahu
Hawaii, 96734

Dear Gene:

Thanks for your many shipments of data of the recent weeks. As usual I have a few questions to ask on various things. For purposes of convenience, I think I will number the questions so that when you reply to me on the tape it will make cross checking easier.

My first set of questions pertains to the trip report for March-April 1969. In some instances, there are some ambiguities here that I can't quite understand and in a couple other instances I wonder if you have any additional data. Following are my questions:

Nihoa-Necker

1. In this report for both Nihoa and Necker, you refer to the trip taken in 1968 as having been made in September. However, in the scientific visit tables you filled out you listed the Necker dates as 28-29 August. You did give Nihoa as 24-27 September, but in the letter you wrote me last year you consistently referred to the dates as 24-25 August. Was it actually August? And is the 24-25 date correct?
2. What is the significance of your class data system - a, b, c, and d? What does it mean when you have an estimate without a class data symbol?

Nihoa

3. On page 3 under Sooty Storm Petrel you remarked that "two individuals were banded" and that this is first recorded nesting of this species on Nihoa. Can you give me any additional information on this? Were these a pair from the burrow, were both birds banded as adults or one of them young, or what?
4. On page 4 under Blue-faced Booby there is the comment that young had hatched. Would I be correct in saying that most nests contained eggs but some young had hatched? On all other March visits by far the greater proportion of nests have had eggs, so I would suspect that some were probably present on Nihoa this trip as well.
5. For both Brown Booby and Red-footed Booby, the comment is "eggs still being incubated." I assume this means that no young were seen and that most nests contained eggs.

6. No comment is entered for either Gray-backed Terns or Blue-gray Noddies. Does this mean that no birds were nesting, or merely that no attempt was made to look for nests?
7. On page 7 under Necker Christmas Island Shearwater - where was the bird seen; in what habitat? This is just to add a little detail in the otherwise brief account.
8. Under your comments on Great Frigatebird, in the letter you sent me, you listed 700 nests counted for the March trip. In the report the figure listed is 850. Is this last figure an estimate to allow for nests not counted? In the material you sent me you had Sooty Terns listed as about 25 percent on eggs. In the report, however, the figure 25% has been corrected to 75%. Which of these is the figure that you consider correct?
9. Under Hawaiian Tern you stated that approximately 80 percent on eggs. Does this mean that the rest were not nesting or that they were on young? I noticed that on two previous March trips (67 and 68) you did see at least one young.

Necker

10. I note from the report that you and John Sincock did a vegetation map of the island. It might be a good idea to include this in the report since I am pretty sure that Lamoureaux will not have anything as up to the minute as this would be.

Laysan

11. Do you have any additional information on the sightings of the Pintail, Shoveler, and Green-winged Teal? Were they males, females, or what, and where were they seen, and so on? Was the Green-winged Teal Anas crecca carolinensis? In other words, did you see an adult male? As you may remember the only previous sighting that was clearly carolinensis, is, I believe, the one you mentioned seeing on Midway some time ago. This and possibly this Laysan observation, both unpublished, would thus be the only substantiation for the occurrence of this subspecies in the Northern Hawaiian Islands.
12. Do you have any information on the breeding status of Blue-faced Boobies, Red-footed Boobies, and Great Frigatebirds? I get the impression that you made quite an effort on counts on this trip and it would be nice to know if there is anything else I can add for these species.
13. On the Hawaiian Noddy Terns. On your percent with nest content is that a sample of 700 nests or a different sample number that you took? If so, how many were in the sample?

All Islands

14. Have you any additional information on whether Sharp-tailed Sandpiper was seen? I think with the report^s you^{ve} sent me now so far, I am almost complete except perhaps for your latest. I still lack, however, the rest of your notes from the July 1967 trip to Pearl & Hermes. The notes I have (which you let me copy from your log book last spring) were very detailed on albatrosses, and I imagine you have some other information on the various other things you saw.

For August-September 1968 trip you fill in personnel for several visits (Nihoa and Necker) but I need this sort of information as well as perhaps a general account for the rest of the islands visited.

Judging from what you print in the scientific table, I guess there isn't anything to add for Necker and I think I have most of the bird stuff from Nihoa from when I was out there with you. I think, however, that in your earlier letter commenting on the Nihoa report, you made some statement to the effect that you got some pretty good population data on Laysan. We don't have that or anything from islands beyond Nihoa and Necker.

I also lack any notes from the February 1969 visit to Pearl and Hermes.

Lisianski Island

15. I am sort of puzzled by the population estimates for Fairy Terns and Hawaiian Noddy Terns. For the former, 13 nests are listed as the population and in the latter, 30 nests. However, under comments 260 nests are listed for Hawaiian Noddies. The comments under Fairy Tern and frigatebird are identical which I suppose resulted from accidentally typing the frigate data in the tern column. Is there any info on the tern nest contents?

Southeast Island

16. I gather that the nest counts on the frigatebirds, Blue-faced Boobies, and Red-footed Boobies were very exact, but there is no class data entry for Brown Boobies. Was this a complete count too? Do you have any status on the nesting of the Gray-backed Terns?

Questions on the May-June 1969 report. Most of these questions should be directed, I imagine, to Dave Olson. Comment - if it is all right with Dave, I think I'll use part of his account of his landing on Necker Island in the Necker Island history since his experience with the sharks should certainly get across the point of how difficult it is to land on that island.

Gardner Pinnacles

17. Under Gray-backs the comment is most have young, varying age from those just pipped to young that are flying. Since it doesn't sound quite right to indicate that pipped eggs are young, would it be all right if I stated something to the effect most birds have young, varying in age from hatching to flight stage? Some pipped eggs present?
18. On Blue-faced Boobies, Brown Boobies, and Common Noddy Terns it is stated that most have young. Were there some eggs of each of these species present? I must say, Dave's recent observations are strongly at odds with earlier observations from about the same time of year. My only guess is that there must have been one helluva nesting failure earlier in the year. Numbers of Blue-faced Boobies, Common Noddies, Gray-backs and Sooty Terns are way down from any other May-June estimate. The absence of breeding Sooty Terns is incredible. The more you, we, and others do in the Leewards, the more it seems that there are a lot of unresolved problems about changes and populations from year to year. My guess is that a lot of these changes are due to really phenomenal late ^{summer} weather storms. The comments on the loss of the sign on Necker, certainly indicate that seas are sometimes extraordinary out there.

Monk Seal

19. I have been pulling together all the various data on Monk Seals to send to you (as well as Green Turtle data - of which more in my next letter). Enclosed are tables for Necker and Nihoa on the Monk Seal for you to look over. Do you want to write the comments for this species account or shall I?

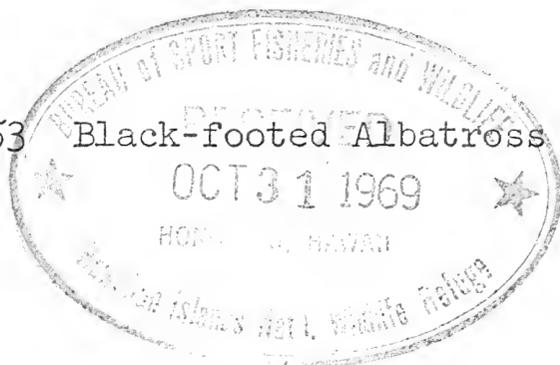
It looks like a sure bet that June is the end of the POBSP. I am really going to have to get cracking to get the various reports out. Will write again soon.

Best regards,

Roger B. Clapp
Pacific Ocean Biological
Survey Program

List of Bird Returns and Recoveries Sent to R.Clapp by E.Kridler in letter of 20 October 1969

Band Number	Species	Where, When, and by Whom Banded	Age When Banded	Where, When, and by Whom Returned/Recovered	Remarks
767-00377	Blue-faced Booby	Necker Island March 15, 1965 H.I.Nat.Ref.	At least one year old A-U	Necker Island March 10, 1967 B.S.F.W.	
662-06052	Ruddy Turnstone	Lisianski Island March 11, 1964 E.Kridler	At least one year old A-U	Lisianski Island March 20, 1968 E.Kridler	Captured and released
662-05888	Sooty Storm Petrel	Southeast, P.&H. March 21, 1965 E.Kridler	At least one year old A-U	Southeast, P.&H. March 22, 1968 E.Kridler	Captured and released
662-06053	Ruddy Turnstone	Lisianski Island March 11, 1964 E. Kridler	At least one year old A-U	Lisianski Island March 20, 1968 E. Kridler	Captured and released
662-06044	Ruddy Turnstone	Lisianski Island March 11, 1964 E. Kridler	At least one year old A-U	Lisianski Island March 20, 1968 E. Kridler	Captured and released
682-16128	White Tern	Laysan Island March 27, 1966 H.I.Nat.Wild.Ref.	At least one year old A-U	Laysan Island March 18, 1968 E. Kridler	Captured and released
793-03025	Ruddy Turnstone	Lisianski Island Sept. 25, 1967 H. I. Nat. Wild. Ref.	A-U	Lisianski Island March 20, 1968 E.Kridler	Captured and released
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* Almost certainly error in Clapp list as all other March 11 dates appear to be Lisianski and all Laysan dates as March 10, 1964

** Error by banding office as Kridler 1966 report gives dates of visit to F.F.S. as 21-24 March

→ *March 24, 1966 is correct. It was so entered on schedule sent banding office*

↓ *Error in Clapp list. Kridler banding records show island as Lisianski.*