C.A.P. 318

BOARD OF TRADE

CIVIL AIRCRAFT ACCIDENT

Report of the Second Independent Review appointed to consider the Accident to Elizabethan Aircraft G-ALZU at Munich on 6th February 1958 and to Report whether blame for the Accident is to be imputed to Captain Thain

LONDON: HER MAJESTY'S STATIONERY OFFICE

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18th March, 1969.

Sir,

On behalf of my colleagues, Professor A.R. Collar and Captain J.R. Jeffrey, and myself I have the honour to present our Report upon the question, referred to us by the Board of Trade, whether blame is to be imputed to Captain J. Thain for the accident to the aircraft G-ALZU at Munich on 6th February, 1958.

A transcript of the oral evidence received by us and the body of documentary evidence which we considered are available if required.

I have the honour to be, Sir,

Your obedient servant

E. S. FAY

The Rt. Hon. Anthony Crosland, M.P.,
President of the Board of Trade.

C O N T E N T S

Part I INTRODUCTION

1. On 8th April 1968 we were appointed by the President of the Board of Trade to conduct an Inquiry in private with the following terms of reference:

> "To consider in private such evidence as may be presented with regard to the accident to BEA Elizabethan G-ALZU at Munich on 6th February 1958, being evidence which was not considered by them when they reported to the Minister of Aviation on 18th August 1960 and having regard to such evidence, to the matters considered by them in that Report, to the Report of the German Federal Office of Aviation relating to the re-opened Inquiry into the said accident and to the Memorandum of the Royal Aircraft Establishment at Farnborough on the Application of the Results of Slush Drag Tests, to report to the Board of Trade whether, in their opinion, blame for the accident is to be imputed to Captain Thain."

2. As the terms of reference indicate, our Inquiry was a re-opening of the Inquiry held in 1960. The former Inquiry, however, was of a different nature; we were then required to consider representations made by and on behalf of Captain James Thain and to report whether he took sufficient steps in three specified matters. We were then limited both by the three questions asked of us and by the fact that the only evidence we could receive was such as Captain Thain was able to lay before us. In the present Inquiry we were able to receive a wide spectrum of evidence and in order to deal with the question of blame we had of necessity to form a view as to the cause of the accident at Munich on 6th February 1958.

3. We commenced the Inquiry in London on 10th June 1968. The evidence, both oral and documentary, was sought out and assembled by the Treasury Solicitor and presented to us by Mr. Vivian Price of counsel. Mr. John May, QC and Mr. J. Wood, instructed by Messrs. Evan Davies and Co, appeared on behalf of Captain Thain. We sat to receive evidence on fifteen days. A number of German witnesses were kind enough to come to London to assist us but some witnesses whom we deemed it important to see were unable to do so and in order to obtain their evidence we went to

Germany and sat at Bremen and Frankfurt on the 25th and 26th June. Otherwise we sat in London, and concluded the main part of the Inquiry on 23rd July. Certain matters emerged at a late stage which necessitated further investigation and we sat finally to hear evidence on 27th November 1968. On 12th June we went to Gatwick Airport and inspected an Elizabethan aircraft, similar to that involved in the Munich accident, which was kindly put at our disposal by Dan-Air Ltd. On 14th June we saw certain films which will be referred to later.

Previous Inquiries

4. Before discussing the evidence it is convenient to recall the previous investigations which have been held into the Munich accident.

5. First, in accordance with the Convention on International Civil Aviation a Commission of Inquiry was set up by the Federal German Ministry of Transport. The proceedings of this Commission took place in April, May and June 1958 and its Report was issued in January 1959. An English translation was published (CAP 153*): we refer to it hereafter as "the first German Report". The results of that Inquiry were summarised in the Report as follows:

> "During the stop of almost two hours at Munich, a rough layer of ice formed on the upper surface of the wings as a result of snowfall. This layer of ice considerably impaired the aerodynamic efficiency of the aircraft, had a detrimental effect on the acceleration of the aircraft during the take-off process and increased the required unstick-speed. Thus, under the conditions obtaining at the time of take-off, the aircraft was not able to attain this speed within the rolling distance available.
>
> The decisive cause of the accident lay in this.
>
> It is not out of the question that, in the final phase of the take-off process, further causes may also have had an effect on the accident."

6. After the first German Report was published Captain Thain and the British Airline Pilots Association (BALPA) on his behalf sought to have the Inquiry re-opened and proffered fresh evidence and arguments tending to disprove the existence of the above-mentioned rough layer of ice. The Commission declined to re-open the Inquiry; their decision not to do so was dated 14th March 1960 and examined the evidence and arguments at length.

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*HMSO, 8s. 6d.

7. Next in chronological order comes our previous Inquiry: this was held in April 1960 and our Report, dated 18th August 1960, was published as CAP 167*. The three questions asked of us were whether Captain Thain took sufficient steps

"(a) to satisfy himself that the wings of the aircraft were free from ice and snow;

(b) to ascertain whether or not in the conditions prevailing at the time the runway was fit for use; and

(c) to ascertain the cause of the difficulties encountered on the first two attempts to take off before making a third attempt."

We answered the first question in the negative and the remaining two in the affirmative.

8. In 1962 the International Federation of Airline Pilots Associations (IFALPA) asked the German Commission to re-open their Inquiry, basing the request largely on the results of investigations into slush drag conducted since the accident. In their written decision dated 29th January 1963 the Commission gave detailed reasons for refusing the request.

9. Meanwhile the Royal Aircraft Establishment were conducting experiments for the measuring of slush drag on different types of aircraft, including the Ambassador. Their findings as regards the Ambassador were published in April 1964. In July 1964 the Ministry of Aviation forwarded the material to the Federal German Ministry of Transport suggesting that it should be examined to see whether or not it tended to alter or supplement the findings of the Commission of Inquiry. After such examination the Federal Minister of Transport ordered that the Inquiry be re-opened. The re-opened hearing took place in November 1965. The Report of this Inquiry, dated 29th August 1966, was published in English translation (together with an RAE paper *(Application of the Results of Slush Drag Tests on the Ambassador to the Accident at Munich)* as CAP 292*. We refer to it hereafter as "the second German Report". This Report found that wing icing was present and was an essential cause of the accident; slush, it found, was a further cause "since it resulted in a reduction in performance during acceleration and may also have caused changes in trim which, in combination with other secondary effect, had an unfavourable effect on the unstick process."

Evidence

10. The present Inquiry is thus the fourth in chronological order. We had before us a considerable body of documentary evidence. This included photographs of the scene after the accident, the original statements of the witnesses obtained for the

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*HMSO, 8s.
**HMSO, 7s. 3d.

German Inquiry, transcripts of the tape recordings of the first and second German Inquiries, transcript of the shorthand note of our previous Inquiry, all the documents placed before our previous Inquiry by Captain Thain, and technical reports from the Royal Aircraft Establishment and elsewhere.

11. We have carefully considered all the documentary evidence together with the testimony of the 27 witnesses called at our hearings and the arguments of counsel. Those of the witnesses who spoke about what happened at Munich were of course doing so more than 10 years after the event but we were fortunately able to consider their evidence in the light of statements made, in most cases, shortly after the accident. We are satisfied that all available material was before us.

Scheme of this Report

12. In accordance with our terms of reference our task was to consider both the evidence and the previous Reports and to report whether in our opinion "blame for the accident is to be imputed to Captain Thain". Before considering blame we have to find, if possible, what caused the accident, and to deal with causes we must first ascertain what happened. Our Report consequently proceeds as follows. By way of introduction Part II gives the non-controversial facts. In Part III we examine the evidence as to the aircraft's final run. Next we examine the two suggested physical causes, namely wing icing and runway slush: Parts IV and V respectively examine the general evidence on these subjects, and in Part VI we deal with the expert and scientific evidence. In Part VII we give our findings upon the facts of the accident and its immediate causes. We then examine Captain Thain's conduct: in Part VIII the command situation on the aircraft and the conduct of the take-off are examined and in Part IX the Captain's responsibility as regards wing icing and runway slush is considered. Our conclusions are summarised in Part X.

PART II THE FACTS IN OUTLINE

13. The aircraft G-ALZU ("Zulu Uniform") was an Airspeed Ambassador, owned by British European Airways, who gave this type the fleet name of "Elizabethan". The Ambassador is a high-winged monoplane powered by two Bristol Centaurus 661 engines; it has a tricycle undercarriage and also a single tail wheel as a protective device to ensure that the fuselage does not ground. Since no question arises as to any defect in the aircraft, no further details need be given save to mention that the port engine was fitted with a Peravia Recorder; this embodies a power-driven roll of waxed paper used to record, against a time base, data as to altitude, engine speed, and manifold pressure. The Peravia recording was recovered after the crash and throws some light on the course of events.

14. The aircraft was on the return stage of a charter flight between Manchester and Belgrade, carrying the Manchester United football team and journalists and others, the total number of occupants, including the crew, being 44. It landed at Munich in order to refuel. The pilot in charge of the aircraft was Captain J. Thain and his co-pilot was Captain K. G. Rayment, who was fatally injured in the crash. On the outward journey to Belgrade Captain Thain had flown the aircraft; on the return, including the attempted take-offs from Munich, Captain Rayment flew the aircraft from the left hand seat and Captain Thain sat in the right hand seat. Captain Rayment was in fact the senior of the two pilots. The aircraft had flown from Belgrade at a height of between 14,500 and 16,500 feet at temperatures in the region of -21° to -25°C. (In this Report all temperatures are given in degrees Centigrade). During the descent to Munich through cloud, the wing de-icing equipment was operated: this comprises a petrol-burning heater used to supply hot air to the interior of the leading edge of the wing, and is fitted with a device which automatically cuts out heating at about 90 knots and thus functions at the time of landing.

15. The aircraft arrived at 1417 hours local time. (In this Report all times given are local time, which was one hour in advance of GMT.) It was snowing at the time, and snow and slush were lying on the ground, including the runway;

the screen temperature was in the vicinity of freezing point. The aircraft made a normal landing and after arrival at the apron Captain Thain went first to the Met. Office for briefing on the next leg of the flight, and next to the Air Traffic Control Office; Captain Rayment reported to the BEA office. Meanwhile, refuelling commenced at 1425 hours; the aircraft's wing tanks had a capacity of 1,000 gallons and they were filled, 726 gallons being taken on in the process. Mr. W. N. Black, the BEA Station Engineer, assisted in the refuelling, which finished at 1438 hours. The wings were not swept or de-iced.

16. At 1519 hours the aircraft obtained clearance to taxi to the runway, and at 1530 it commenced its first attempted take-off. The aircraft accelerated to approximately 105 knots when Captain Rayment abandoned take-off because the boost on both engines was fluctuating. Brakes were applied and the aircraft came to rest approximately 1,350 feet from the far end of the runway. Permission having been received to back-track, it returned to the starting point, and at 1534 hours commenced its second run. On this occasion, the throttles were opened more slowly and the starboard engine boost was steady, but at about 85 knots the port boost gauge fluctuated considerably and went above the permitted maximum of 60 inches, MAP. Captain Thain thereupon ordered the take-off to be abandoned and decided to return to the apron for consultation with the Station Engineer. The aircraft rolled to the far end of the runway and taxied back to the Terminal Building, arriving at 1539 hours. Captain Thain took over the controls while taxying.

17. Mr. Black came aboard the aircraft. He knew that boost surging was not uncommon on Elizabethan aircraft at Munich, owing to the airfield's height of 1,732 feet above sea-level. He so informed the pilots, and advised that the normal way of dealing with it was to inch the throttles back to maintain the required 57.5 inches of boost. The passengers had been off-loaded; they were recalled and the aircraft was again cleared to taxi to the runway at 1556 hours. Neither pilot had left the cockpit during the aircraft's 20-minute wait on the apron.

18. The aircraft reported "rolling" on its third and last attempted take-off, by radio at 1603.06 hours. It never became airborne. Fifty-four seconds later Mr. G. W. Rodgers, the radio operator, called Munich control but before he had had time to complete his identification, the transmission was cut short. The aircraft had traversed the entire runway and the continuation stopway, broken through the boundary fence and struck a house and trees, after which it broke up. The last R/T message ended with loud noises associated with the collision with the house. The port wing was torn off in the collision and the fuselage broke in two behind the wings. The forward part of the fuselage together with the intact starboard wing travelled some 600 feet beyond the house before coming to rest.

19. The cardinal question is of course why did not the aircraft become airborne during its run along the entire length of the runway. An aircraft lifts off or "unsticks" when the lift generated by its run exceeds its weight. Lift is generated by speed of forward motion in conjunction with the aircraft's angle of incidence, or angle of attack. Design calculations show that an Ambassador in the atmospheric pressure then prevailing at Munich and with Zulu Uniform's all-up weight of 24,739 kg had a minimum take-off speed of 105.5 knots, i.e. on attaining that speed it should unstick, provided that it already is at, or is then rotated to, the maximum possible angle of incidence namely $9\frac{1}{2}^{\circ}$. The minimum take-off speed will be increased if the aerodynamic efficiency of the wings is diminished, e.g. by the presence of ice, and the time and distance needed to accelerate to a given speed will be increased if drag is increased, e.g. by running through slush. In examining the evidence therefore, we must ascertain (1) the behaviour of the aircraft on its final run, with particular reference to speed and attitude; (2) whether ice or any other factor affected its ability to lift; (3) whether slush or any other retarding influence affected its speed.

PART III THE FINAL RUN

20. Like other airlines, BEA used the "variable decision take-off technique" which involved calculating in respect of any take-off a "decision speed" or "V_1" (at which the aircraft would be capable either of continuing and taking off with one engine inoperative or, in normal conditions, of being brought to a standstill within the distance available) and a "take-off speed" or "V_2" (at which the aircraft should be flown off). For this take-off Captain Thain calculated, and calculated accurately, V_1 as 117 knots and V_2. The procedure for take-off should have been as follows: advance throttles gradually to 57.5 inches manifold pressure; at about 80 knots rotate aircraft slightly to bring nose-wheel off the ground; at V_2 rotate further and unstick. In favourable conditions this aircraft should have unstuck after traversing about one half of the runway, which was 6,260 feet in length.

21. The evidence as to the final run is firstly that of witnesses who watched the run, secondly that of the survivors from the flight deck, Captain Thain and Mr. Rodgers, thirdly that regarding marks on the runway, and fourthly that afforded by recorded data.

<u>Eye-witnesses' evidence</u>

22. Of the eye-witnesses, those with the best view of the runway were the Air Traffic controllers in the tall Control Tower (see plan, Appendix 1). Herr Kurt Gentzsch was in charge; Herr Erich Lass was on stand-by; Herr Peter Poggendorf and Herr Gustav Taresch (a trainee) were at work. All four watched Zulu Uniform's run. Gentzsch used field glasses, and pressed the alarm button just before the aircraft left the runway. All four made statements within a day or two of the accident and three (Taresch was the exception) gave evidence before us. The following are extracts from the translations of their contemporary statements:

> *Herr Gentzsch:* "It began rolling normally and built up speed until it was about half-way along the runway; the nose wheel left the ground, but touched down again after about 60 - 100 metres. The aircraft continued to roll as far as the very end of the runway, then unstuck, but gained only a little height. Approximately

above the west boundary of the airport it seemed as if the aircraft was going into a turn and was not gaining any appreciable height."

Herr Lass: "The aircraft gradually built up speed, the nose wheel leaving the ground approximately half-way along the runway, but the aircraft did not become airborne within a period which could be considered as normal. I then observed that the pilot pressed the nose of the aircraft down again, until the nose wheel touched the ground, as if he wanted to gain extra 'play' in order to pull the aircraft off the runway. The nose did, in fact, leave the ground once again before the end of the runway, but I could not make out with the naked eye whether the aircraft actually became airborne. Rather, it appeared that only the left wing and hence the left wheel lifted, as if the pilot, by making a turn on the ground, was trying to avoid colliding with the house ... Whether during the attempted take-off, the aircraft became unstuck whilst still on the stopway, I cannot testify owing to the distance. I had, rather, the impression that, in the final phase, before the left wing hit the obstruction, the tail of the aircraft, at least, was trailing."

Herr Poggendorf: "The aircraft ... gained speed rapidly. About half-way along the runway the nose wheel left the ground for about 100 m, but then touched down again without loss of speed. At the threshold the aircraft rose from the ground, but scarcely gained height. Approximately on a level with the localiser, [it] made a slight turn to the right, but without gaining height. Immediately afterwards the aircraft crashed and at once caught fire."

Herr Taresch: "About half-way along the runway, the nose wheel lifted slightly. It touched down again about 60 m further on. The aircraft continued to roll, without any change of speed, along the full length of the runway, ran over the threshold lighting and went into a slight turn to the right. After running over the threshold lighting, the aircraft attained a height of about 50 cm, and its left wing grazed the house."

23. It will be observed that each of these observers saw the nose wheel lift about half-way along the runway and subsequently touch down again. Three of them thought the nose was raised for 60 to 100 metres. (Herr Gentzsch told us that he could judge runway distance by the runway lights which were 60 m apart). Herr Lass, however, differed about this distance: we asked him how long the nose was up and after careful thought he gave 11 seconds as his estimate, which in distance would mean about 500 m.

24. Other witnesses gave evidence about the aircraft's attitude during the run. Herr Siegfried Schombel, an ATC trainee, was on the second floor of the main airport building, to the west of the control tower. Another ATC trainee, Herr Heinz-Dieter Tismer, was in the same building, he thought on the first floor. Major Hans-Georg Brehme of the Bundeswehr, himself a pilot, was at the airport in charge of a detachment of soldiers on a helicopter training course. Each gave evidence. Major Brehme handed us a copy of a full report which he made on the day after the accident to his superiors. At the time of the run he was standing on the baggage ramp on the apron. The relevant part of his report read as follows:

> "The aircraft began slowly to gather speed, increased speed, was a bit faster than on the previous take-off attempts. Half-way along the runway the pilot tried to unstick the aircraft. The nose wheel lifted, but the main wheels remained on the ground. The pilot increased the angle of his attitude so that the rear end was very nearly dragging along the runway, and the aircraft lost speed. This occurrence took place on the last third of the runway. I now waited for a reduction of engine power and for a possible retraction of the undercarriage, because unstick from the ground was impossible; but neither took place ... The aircraft raced without lifting off the ground through the airport fence and crashed with its left wing against the house."

Herr Schombel gave a similar account: "During take-off it struck me that from about half way along the runway the pilot was struggling to get the machine off the ground, and I was struck by the unusually great angle of attack. The nose wheel was high in the air, the auxiliary skid, according to my observation, was on the ground. This attitude was somewhat reduced during take-off. The nose wheel remained raised off the ground."

Herr Tismer's account was: "During the [attempted] take-off it struck me that the pilot was trying for all he was worth to unstick the aircraft. The nose wheel was raised very high off the ground; the angle of attack was very large. The tail appeared to be touching the ground." In evidence Herr Tismer added that up to the middle of the runway the attitude seemed perfectly normal. He agreed that owing to the shape of the aircraft it was difficult to tell its precise angle.

Mr. Black also watched from ground level. In an interview with Captain Reichel, the German senior investigator, he said: "The aircraft's nose lifted after it had covered approximately the first third of the runway and then continued in this normal attitude until approximately half or two-thirds of the runway had been covered."

25. None of these witnesses observed the nose wheel fall after its initial rise, but all were at a lower elevation than the Control Room. Visibility at the time was given as 1.6 nautical miles; it was not snowing but was hazy. But many witnesses spoke of the cloud of spray thrown up by the aircraft and this cannot have helped accurate observation. Mr. Black said the aircraft was enveloped in spray during the whole run.

Evidence from the Flight Deck

26. Captain Thain's contemporary statement to the German investigators is published in full as Appendix 2a to the first German Report (CAP 153), and the portion dealing with the final run is reproduced as paragraph 20 of our 1960 Report (CAP 167). At 85 knots he noted boost surging on the port engine but controlled it as Mr. Black had advised. After this he noted air speed indicated at 105 knots and called "105". "The needle of the Air Speed Indicator was flickering slightly and when it indicated 117 knots I called "V_1" and waited for a positive indication of more speed. Captain Rayment was adjusting the trim of the aircraft. ... The needle hovered at 117 knots and then dropped 4 or 5 knots. I was conscious of a lack of acceleration, the needle dropped further to about 105 knots, and hovered at this reading. Suddenly Captain Rayment called out "Christ, we won't make it." I looked up for the first time and saw a house and a tree, all this time my hand had been behind the throttle levers, I raised it and banged the throttles but they were fully forward. I believe Captain Rayment was pulling the control column back, he called hurriedly 'undercarriage up' and I selected up ..."*

27. In his evidence at the 1960 Inquiry Captain Thain said that normally by the time he had uttered "V_1" the needle of the ASI would be past 119; this did not happen at Munich, instead the needle flickered. Up to then he had not felt anything wrong with the acceleration. He had no idea what time or distance had been taken up to this event. He told us at the present Inquiry that he did not notice any change in the aircraft's attitude, but would not expect to do so. He was not aware of brakes being applied and was certain he had not applied them himself. He had operated the override switch when attempting to raise the undercarriage.

28. Mr. G. W. Rodgers, the radio operator on Zulu Uniform, made a written statement on 10th February 1958 but had not given evidence at any previous Inquiry. The part of his statement dealing with the final run is as follows:

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*The main undercarriage could not in fact be retracted under load. Examination of the wreckage showed that the nose wheel had retracted but the main wheels had not. See para. 37 below.

"As the aircraft began to move down the runway I called 'rolling'. During the take-off run I was watching the boost gauges, as we had had two previous attempts to take-off, and I noticed that, when Captain Thain called 'V_1,' both needles were just below 60. Shortly after this I heard Captain Rayment call for undercarriage up. At about that moment I sensed that the aircraft was not lifting off the runway in the normal way, and immediately called the Tower with the aim in view of giving them 'airborne' if the take-off proceeded normally, or otherwise of alerting them if anything went wrong, although I did not know that at that moment a state of emergency had probably arisen. Before I could do more than give the call sign the aircraft crashed."

29. In evidence Mr. Rodgers explained that during the run he had been facing forward in his swivel seat. He sensed something amiss at about the moment when Captain Rayment called for undercarriage up. At that moment "the engines were going full bore". To transmit he had to lift the plunger securing his swivel chair, turn the chair to the left to face his instruments, lower the plunger, and operate the transmission switch. He had heard "V_1" called but did not hear any ejaculation from Captain Rayment. He could not say what length of time elapsed between "V_1" and "undercarriage up", nor could he say what it was that alerted him to transmit. He could not recall any indication of application of brakes. He was asked "Can you recollect having seen or heard or even sensed anything which indicated that the two pilots were not happy with one another or were getting across one another or misunderstood one another?" His answer was "No".

Tracks on the Runway

30. The tracks made by the aircraft at and beyond the end of the runway were inspected by airport officials soon after the accident. Their findings were summarised in the first German Report (pp. 11 - 12) as follows:

"From the point at which the aircraft had broken through the fence its tracks could clearly be discerned, extending back to the runway. The double track of the right side of the undercarriage could be followed back to the runway without difficulty. The left hand wheel-track was interrupted in places. Nowhere was there any nose wheel track to be seen. From the end of the runway to the fence, in the direction of take-off, the wheel-tracks showed a slight swing to the right. Two days after the accident, when the snow had melted, the tracks were particularly clearly visible. On the runway, about 50 m short of the end, a skid-mark began. It was clearly visible on the concrete and from the strewn sand which the wheels had pushed aside (see sketch map and photographs). This mark showed that at this point all four wheels of the main undercarriage were locked. This skid-mark continued for approximately a further 30 m beyond the end of the runway. It then stopped and there remained the impression of the free-running wheels on the grass surface. The track of the right hand twin wheels was strongly marked; that of the left hand wheels was uniformly clear throughout the whole length (250 m) of the stopway as far as the point at which the aircraft crashed through the fence. The left hand wheels had at times left the ground."

Reference may also be made to the photographs of tracks annexed to the first German Report as figs. 6 - 10 of Appendix 8 thereto, and to the plan of the end of the runway which was Appendix 9 to that Report and is here reproduced as Appendix 2. The last mentioned shows that towards the end of the runway the aircraft's track ran in the southern or left hand side of the runway and was veering to the left.

31. The above testimony needs to be supplemented by that of Herr Reinhardt Meyer, who gave evidence before us at Bremen. He has been a pilot since 1940 and was concerned with aircraft design. He was at Munich airport on business and was one of the first to reach the scene of the accident. He said "because I saw nobody looking for the wheel tracks and because I thought it would be important before it was covered with snow, I went to look". He observed the tracks mentioned above, and in addition, after walking on to the runway he observed the track of the aircraft's tail wheel, or, as he thought at the time, tail skid. This came to an end 100 - 150 metres short of the end of the runway: he could not see the beginning of it. The Ambassador has a single tail wheel. It was suggested to him by Mr. May that the track was that of the nose wheel, but this was a double wheel and Herr Meyer was clear that he had seen a track so narrow (he put it at 9 inches) that he interpreted it as that of a tail skid. He made a sketch map at the scene, and reproduced it in a report made to the authorities on 16th April 1958.

32. One curious feature of the ground markings is that in a photograph (fig. 6 of Appendix 8 to the first German Report) taken before dark on the day of the accident there appears a mark parallel with and outside the track of the twin starboard main wheels. This mark could have been made by the starboard tail fin when the aircraft was banking to the right. The geometry of the Ambassador is such that if this fin were touching the ground the port main wheels would be off the ground and probably the tail wheel also. The mark appears to correspond with a stretch of the grass stopway where the port wheels left the ground and where the starboard main wheels show a perceptible turn to starboard. If as seems probable it was a tail fin mark it shows that at this late stage the aircraft was at an extreme attitude.

Recorded Data

33. The tape recording of the R/T communication between Munich Control and Zulu Uniform showed that the aircraft reported

"rolling" at 1503:06. At 1504:00 the radio operator commenced to transmit. He uttered the words "Munich. From B-line Zulu Unif ..." when the transmission was broken off amid loud background noises. Assuming those words took 2 seconds to speak it follows that the collision with the house occurred at 1504:02, or 56 seconds after the commencement of the run.

34. The Peravia recorder was fitted to the port engine. It records data on a wax paper roll on a time scale of 1/16th inch to a minute. We were provided with a magnified photograph of the traces. On this it is difficult to interpret a trace with accuracy to seconds, but reasonable certainty to within 5 seconds can be obtained and the figures which follow must be read with that margin of error in mind. The manifold pressure trace showed that on the first run full power was reached after 14 seconds, on the second run after 25 seconds and on the third after 17 seconds. On the first and second run power was cut at 32 seconds from start. On the third run there was a drop in boost pressure, presumably caused by throttling back, 20 to 25 seconds after opening up, followed by reversion to full power 10 to 15 seconds later. Full power was maintained for about 15 seconds, after which pressure dropped below static, indicating throttle closing. The time from first opening to closing was about 50 seconds.

Provisional Assessment of Evidence of Final Run

35. A number of witnesses were struck by the exceptional angle of incidence of the aircraft towards the end of the run. This tallies with the tail wheel mark seen by Herr Reinhardt Meyer and convinces us that the aircraft went to extreme attitude. We know where the tail wheel mark ended (100 - 150 metres from the end of the runway) and although we do not know where it began, we are satisfied that the aircraft rotated to extreme incidence somewhere in the last third of the runway and more than 300 feet before the end of the concrete. Shortly after the tail wheel had come up again, the brakes were applied and remained applied while the aircraft ran off the concrete on to the grass slipway. The brakes were then released.

36. Somewhere in the vicinity of the far threshold the engines were cut. It is impossible to say whether this was before or after the end of the runway. The aircraft collided with the house and tree about 56 seconds after reporting "rolling". At 100 knots, which must have been its approximate speed at the time, the aircraft would traverse the distance between the threshold and the house in approximately 5½ seconds. It would be at the threshold therefore about 50½ seconds after "rolling", and it is roughly 50 seconds after start that the Peravia records throttle closing. The coarse scale of the Peravia however renders precise calculation impossible and it appears from Mr. Rodgers' evidence that the closing of the throttles came a little later: they were fully open when he commenced to turn to transmit, and if his turning

of his seat and switching on his transmitter occupied three seconds they were still open 51 seconds after the "rolling" message and not closed until after the end of the concrete.

37. Doubt also exists as to when the undercarriage was selected up. At the time of our 1960 Inquiry we were of opinion that this operation represented an emergency attempt to decelerate. We did not then see the contemporary flight manual, but had we seen it we should have been supported in that view: in the section dealing with emergency action after engine failure during take-off it stated "In extreme emergency the undercarriage may be selected up, after operation of the override." In June 1958 this passage was deleted from the manual and the following substituted: "Even in extreme emergency the undercarriage should not be selected up as this will shear the driving dogs connecting the jacks to the downlock release pawls. The main undercarriage will remain locked down but the nose undercarriage will retract." At Munich the main undercarriage did remain locked down and the nose wheel did retract, and one might be pardoned for thinking that this amendment, made four months after the accident, was inspired by these events. But we were assured this was not so. One of our witnesses was Captain G. M. Kelly, Inspector of Accidents with the Board of Trade, who had been the United Kingdom accredited representative at the German inquiries. It so happened that he had previously been with BEA and had been an Ambassador pilot. He told us that since 1955 pilots had been instructed during the refresher training programme that the main undercarriage would not retract under load and that this emergency procedure should not be used: he had himself discussed the matter in safety lectures while he was with BEA. We therefore have the alternatives that either Captain Rayment (who may well have recently re-read the manual during his rehabilitation mentioned in paragraph 156 below) had forgotten the oral advice and was attempting to decelerate, or that he thought he was becoming or was about to become airborne and hoped by getting the undercarriage up smartly to stay airborne. It was submitted to us that the latter was the case and that this alternative explained the brake mark because the drill provided, in the words of the manual "when safely airborne, brake the wheels (to avoid damage to the nacelles by stones thrown off the tyres) and retract the undercarriage". Interpretation of these events is not easy. Since Captain Thain operated the override he must have appreciated instinctively that the aircraft had not lifted off. On the other hand both he and Mr. Rodgers remember that the throttles were still fully forward when the order "undercarriage up" was given, and if Captain Rayment were hoping to decelerate he would have closed the throttles.

38. Towards the end of the runway the aircraft was well to the left of the centre line (see plan, Appendix 2). At and after the threshold it turned slightly to the right, a course resulting in its port wing colliding with the house and being torn off. Had the course not been changed the

fuselage would have collided head-on with the house. It was suggested that this banking to the right was involuntary and represented a starboard wing stall while the aircraft was partially airborne. We do not accept this suggestion; we think it was a deliberate turn in a vain attempt to avoid collision with the house. The banking took the aircraft's port wheels off the ground intermittently, accounting for some observers' erroneous belief that the aircraft had become airborne.

39. The outstanding fact emerging from events in the last third of the runway is that at some point the aircraft was fully rotated while the engines were at full throttle. The aircraft's speed at this point cannot accurately be determined. If Captain Thain's recollection is accurate the air speed indicator showed 105 knots at the moment of Captain Rayment's ejaculation. Whether this was also the moment of rotation we do not know: it may have been, since Captain Thain believes that Captain Rayment was then pulling the control column back. Or rotation may have been earlier, in which case the speed may have been a little higher.

40. What had been the aircraft's attitude further back along the runway, when it had sufficient speed, according to Captain Thain's and Mr. Rodgers' evidence, for normal take-off? Here there is a conflict between the four air traffic controllers, who all speak of the nose wheel rising at about the half-way mark and after an interval touching down (and expressly or by implication indicate that it rose again towards the end of the runway) and the other witnesses who said the nose wheel rose before or about half-way and who did not see it descend again. We prefer the evidence of the air traffic controllers. From the height of their tower they had the better opportunity of seeing what was happening at a distance of some 3,000 feet, and one of them was using binoculars. They were accustomed to observing take-offs and would be likely to note so unusual a feature as the nose going down. One cannot see why they should imagine it if it did not happen, whereas it is easy to understand other observers failing to see or to note it in the hazy visibility and the clouds of spray thrown up by the aircraft.

41. Moreover the falling of the nose wheel is consistent with Captain Thain's evidence. If he is right, something occurred at around V_1 to check the acceleration and induce deceleration. No suggestion of power failure has ever been made and the only possible cause of this phenomenon is a substantial increase in drag. The only agency which could, in the circumstances prevailing at Munich, have increased drag was an increase in the depth or density (or both) of the slush through which the aircraft was travelling. Such an increase would cause a powerful nose-down pitching moment and, if the nose were up, would bring it down. It is hardly likely that Captain Rayment would not have lifted the nose before reaching V_1. Moreover once the nose wheel is down, its drag is added to the retarding forces, and it further increases the nose-down moment. We discuss later the expert

evidence on this matter. Here it is sufficient to say that the expert evidence, Captain Thain's evidence, and the evidence of the four air traffic controllers provide a consistent picture of the aircraft's nose coming down somewhere after the half-way point or the runway.

42. As already noticed (paragraph 23 above) Herr Lass thought the nose was raised for a longer period than did the other two witnesses, though his evidence is consistent with that of Mr. Black. Herr Lass impressed us as being at great pains to be accurate. The accuracy of his observation is demonstrated by his seeing and remembering that the port wing and wheels rose towards the end, facts independently established by the ground markings. Further, his account is consistent with Captain Rayment following the instructions given in the flight manual that the nose wheel should be eased off the ground at about 80 knots, because the time required for the aircraft to accelerate from 80 knots to the 117 knots at which, if Captain Thain is right, something happened which was likely to be associated with the nose wheel coming down, is approximately 12 seconds. This accords remarkably with the estimate of 11 seconds which Herr Lass gave us. Again it may be that Herr Lass's eyesight enabled him to detect a change in the aircraft's attitude at an earlier point in time than his colleagues; the small change needed to bring the nose wheel off the ground cannot have been easy to detect until the aircraft was nearing a right angle to the line of sight from the Control Tower. On the other hand it is possible that Captain Rayment delayed raising the nose until the boost surge had ceased, in which event he would only have got it up a few seconds before, according to Captain Thain, V_1 was reached. This would accord better with the 60 to 100 metres of the other three controllers' evidence. Or it may be that Herr Lass's colleagues missed the initial easing off of the nose which he observed, and that they saw the beginning of an attempt to rotate the aircraft which was frustrated by the phenomenon which brought the nose back to earth.

43. We shall examine later the question of the aircraft's attitude in the light of the whole of the evidence. Meanwhile we can affirm that the direct evidence has convinced us that the aircraft was seen to be accelerating along the runway, that its nose was raised at or before the half-way point and that subsequently the nose came down and, after an interval, was lifted again. Whether the angle of incidence was, at any time during the first raising of the nose, sufficient to enable the aircraft to unstick in normal conditions, does not appear from the evidence and could hardly be expected to. What does appear is that the angle of attack was more pronounced towards the end of the runway during what we find was the second raising of the nose.

PART IV THE QUESTION OF ICING

44. Ice (by which we mean any frozen deposit) adhering to an aircraft affects its performance in two ways. By reason of its thickening and roughening effect it increases drag, and by reason of its effect upon the aerodynamic properties of aerofoil surfaces it reduces the lift of those surfaces while increasing overall weight. It may thus both reduce the aircraft's acceleration and maximum speed, and increase the speed necessary for it to unstick.

45. The evidence before us under this head is (a) meteorological evidence; (b) the evidence of the state of the aircraft before the final run; and (c) evidence of the state of the aircraft after the accident. We must also consider the expert evidence as to the relation between wing icing and performance in order to see whether any relevant deduction can be made from what this aircraft did.

Meteorological Evidence

46. No fresh meteorological evidence was called at the present Inquiry. We had a body of such evidence at the 1960 Inquiry, the effect of which was summarised in paragraphs 38 - 40 of our Report. Its salient features were as follows. The screen temperature at the airport fell from +0.1°C at 1400 hours to zero at 1500 hours and -0.2°C at 1600 hours - 3 minutes before the commencement of Zulu Uniform's last run. Snowfall was recorded at half hour intervals. "Moderate snowfall" was recorded from 1150 to 1520 hours and "slight snowfall" from 1550 hours to 1820 hours, followed by "moderate snowfall" from 1850 hours onward. The witnesses agreed that very little, if any, snow was falling at the time of the aircraft's last departure from the apron and during its final run. The total amount of precipitation in the seven hours ending 2114 hours was 5 mm (water equivalent). From radio sonde observations at 1300 hours it was clear that temperature fell as height increased; falling snowflakes would not have commenced to melt until encountering the ambient temperature at or near the ground. Dr. H. K. Muller, the airport meteorologist, told the first German Inquiry that until 2100 hours the snow was wet and fell in big flakes.

47. The meteorological witnesses at the 1960 Inquiry were Dr. H. L. Penman, OBE, FRS, head of physics Department at Rothampstead Experimental Station, and Mr. R. F. Jones, a Principal Scientific Officer at the Meteorological Office, Air Ministry. Mr. Jones estimated the amount of precipitation during Zulu Uniform's time at the airport at probably not more than 2 mm. We said (paragraph 20 of our 1960 Report):

> "We agree with his assessment, as regards the precipitation at the meteorological enclosure, which was 100 to 200 yards from the apron. Snowfall may vary in density within relatively small distances, and this assessment may not hold good of the runway, some 1,000 yards away from the apron. It is also important to note that temperatures may vary within short distances: Dr. Penman said 'Temperatures at the same level above ground can vary by several tenths of a degree quite easily ... so that a temperature of 0°C in the screen might be appreciably more or even appreciably less on the apron."

48. In 1960 we examined the factors affecting the temperature of the aircraft's wings before its final departure. In addition to the ambient temperature at Munich, these included cooling to perhaps -20°C during the high altitude flight from Belgrade, the use of wing heaters during the descent to Munich, and the filling of the wing tanks at Munich with 726 gallons of fuel at "not above about 0.0°C" (1960 Report, paragraphs 41 - 43). We should add that if, as the evidence to be mentioned below indicates, snow was melting on the wings during part of the aircraft's stay at Munich, heat transference would soon have substantially reduced the temperature of any previously warm part of the wing surface. On the other hand, in the centre of the wings, surface temperature would be influenced by cabin heating and in the vicinity of the nacelles by engine heat (and see paragraph 70 below). The outer parts of the wings would be the coldest, but whether their temperature was below zero at the time of departure we find it impossible to say. The major influences on this part at that time would be the temperature of the fuel in the wing tanks and the ambient temperature at the apron, and the evidence does not enable us to find either of these temperatures with precision to, say, one-fifth of a degree.

<u>The Aircraft before Take-off</u>

49. In 1960 we heard evidence from Captain Thain and Mr. Black as to the state of the aircraft at Munich. At our present Inquiry we heard evidence from Major Brehme, Herr Tismer and Herr Schombel (mentioned above, paragraph 24), from Herr Robert Wiggers, who had been upon the aircraft's wings in his capacity of refueller employed by the fuel company, and from the air traffic controllers, Herr Lass and Herr Gentzsch (paragraph 22 above).

50. The evidence falls into two parts, viz. that relating to the aircraft's first visit to the apron (1417 to 1519 hours), and that relating to the second visit (1539 to 1556 hours). Refuelling occupied from 1425 to 1438 hours: during that time Mr. Black as well as Herr Wiggers were upon the aircraft's wings. Mr. Black said "when I was up on the wing. the wing was quite clean and as the snow was contacting the wing the snow was melting immediately on contact". Herr Wiggers agreed, but thought some snow was lying on the wing tips: it was very little however as he recalled that when wishing to retaliate against a passenger who had thrown a snowball at him he did not find sufficient snow to make a snowball. At a later stage in the first period Captain Thain (whose examination of the position is detailed in paragraph 45 of the 1960 Report) saw from the ground "a thin film of partially melted snow on the starboard wing". This was at the forward edge. He subsequently saw melted snow running off the trailing edge. This was a period during which "moderate snowfall" had been reported. The evidence taken as a whole convinces us that when the aircraft first departed from the apron its wings were wet and not iced, but may have carried a thin slushy deposit.

51. But by the time of its second departure it had been out on the runway, made two abortive runs and entered the stage when the screen temperature passed fractionally below zero. "Moderate snowfall" was replaced by "slight snowfall" at some time between 1520 and 1550 hours. It is to this period that the cogent evidence of the state of the wings of the aircraft relates.

52. Captain Thain did not leave the flight deck during the return to the apron. Speaking of Captain Rayment and himself, he said "We both looked out of our respective windows and studied our respective wings and we found that we had lost that very thin film of partially melted snow which I had observed walking out to the aircraft, and from my seat the wing appeared quite clean." The engine nacelle interrupted his view of the wing, but he could see the ice indicator marks and further outboard; his eye level was below the wing level, but he could see the leading edge, and, because of the curvature of the wing, he could also see the upper surface for about the first 15 per cent of its chord. He emphasised that of the part of the wing within his vision he could see the metal with no snow upon it. Mr. Black walked round the aircraft during this period but saw nothing amiss. When the aircraft taxied away and reached a sufficient distance for the wing surfaces to be seen he told us in 1960 that he observed that the wings were clear of snow except for the wing tips.

53. The contemporary statements of the four Air Traffic Control officers in the Tower did not deal with this matter. We asked both Herr Lass and Herr Gentzsch what their

recollections were and (be it remembered, speaking 10 years after the event) Herr Lass said "some form of material was on the wings. The type of this material I would have called slush": and Herr Gentzsch said "I had a good look at the Elizabethan and can say with good conscience I saw some watery snow towards the wing ends - white spots." He indicated that what he saw was white watery spots, not continuous; he could not commit himself to an estimate of their size. Also in the Tower during part of this time was Major Brehme (he descended to the apron after the passengers re-embarked). He had been there since before the aircraft's first departure. He said that he saw what he put at 2 cm of snow on the wings when the aircraft first departed and that the same amount of snow was on it when it returned and remained there while he was watching. He said on the front portion of the wing there was no snow, "it was towards the end portion ... it was a rather slushy snow which now, due to drop in temperature, more and more clung to the surface." Major Brehme in our view exaggerated snow depths: he put the depth of snow on the runway at 10 cm, which was inconsistent with other evidence including the recorded depth of snow at the meteorological enclosure, where it was measured as 4 to 5 centimetres: see paragraph 89 below.

54. Herr Schombel and Herr Tismer also noted the state of the wings. In their contemporary statements they said:

> *Herr Schombel:* "I was able to see the upper side of the aircraft clearly from a distance of a maximum of 50 m ... snow remained lying on the wings ... after the mechanic had given the signal to taxi the snow remained lying on the wings in spite of the slipstream. It was wet, sticky snow."

> *Herr Tismer:* "I could see the upper surface of the wings clearly from a distance of approximately 40 m. I said to my colleagues 'They're not removing the snow from the wings' ... I also said that the snow was remaining in spite of the moving propellers, probably because it was so wet. The aircraft taxied out to the take-off in this state. I also noticed that the snow was lying on the wing surfaces projecting beyond the fuselage."

In his evidence to us Herr Tismer said "I am quite certain that in fact the snow was wet."

55. Another air navigation services trainee, Herr Hubertus Wollner, also observed the snow. He did not give evidence, but in his contemporary statement said "Snow was lying on the wings in such a way that in places the grey colour of the wings was showing through faintly. There was no snow behind the propellers. The snow was lying only outside the propeller slipstream, on the wings ... I can testify with absolute certainty that there was wet snow on the outer section of the right wing." To the first German Inquiry he

said the snow was grey-white and patchy. The patchiness he attributed to melting. An employee of Pan American World Airways, Herr Johannes Bogen, watched the passengers embark for the final run from a second floor window about 50 yards from the aircraft. He was reported by the investigators as remembering that some relatively wet snow remained on the wings and fuselage, but he did not give evidence at any inquiry.

56. The German investigators procured a photograph of the aircraft taken from the airport building shortly before its last departure by another trainee, Herr Neumann. It was reproduced as Appendix 7 to the first German Report. This appeared to show some snow on the wings. It seemed to be valuable evidence and was used as such by the German Commission (both Reports) and by us in making out 1960 Report. Towards the close of our Inquiry the Treasury Solicitor succeeded in obtaining the original negative of this photograph; a variety of careful enlargements were made for our use, and the negative was submitted for examination and interpretation to the Joint Air Reconnaissance Intelligence Centre (JARIC). A representative of JARIC gave evidence on our last sitting day. In the result the effect of the photograph is somewhat different from that which had previously been assumed.

57. In the first place enlargement showed, faintly, the identification letters painted on the starboard wing; these had not shown in the print previously used. Secondly, the whiteness of the wings, hitherto assumed to be due to a layer of snow, may have been due to light reflected from a wet surface. The forward door is shown opened upward in the photograph: careful examination shows such detail of the circular window in this door that no snow can have been lying on it, yet the door, which is the same grey colour as the upper wing surface, shows as white as the wing surface. The witness from JARIC went so far as to give it as his opinion that there was no ice or snow on the wings in the photograph. We cannot, however, follow him to that extent, and for this reason. The photograph shows the wing surfaces as grey in the path of the propeller slipstream and white outboard of this area. The part showing grey is the same colour and is at the same angle to the sky as the part showing white. The JARIC witness could offer no explanation of this colour demarcation, nor of the fact that the tail plane (also in the slipstream) appears in the photograph to be grey rather than white. Nor could he say that the photograph would have presented a different appearance if there had in truth been snow on the wings. We accept that whiteness in the photograph does not establish the presence of snow, but we have no doubt that the state of the wings behind the propellers must have been different from that further outboard. The difference may have been that between a clean wing and a lightly snow-covered or slush-covered wing, or it may have been the difference between a dry wing and a wet wing. Upon full consideration we find that the photograph is of little assistance. One thing it does demonstrate is how close the aircraft was parked to the terminal building.

58. Some additional evidence as to the behaviour of snow on aircraft's wings at the relevant time and place is afforded by the evidence of Captain E. R. Wright, who gave evidence at the 1960 Inquiry. He landed a BEA Viscount at Munich five minutes before Zulu Uniform's last run: in fact the Ambassador was holding at the end of the runway when the Viscount landed. Shortly after the time of the accident Captain Wright inspected his wings. He said "I found very little precipitation, only a little water, very melted snow, and very little of that. I had a stand to look up over the wing and found very little indeed ... There was not sufficient ... to have necessitated de-icing." Captain Wright's departure was delayed by the accident: by 1700 hours (one hour after the accident) he found a film of snow, in places freezing to the aircraft, and he had de-icing carried out. It is not surprising that he found very little precipitation at his first inspection because he landed during a period of slight snowfall. More important is that the snow was melting, but we do not know what factors may have affected the temperature of his wings.

59. No one who observed the upper surface of the wings of Zulu Uniform from above prior to the departure at 1556 hours said they were clear; except Captain Thain, whose vision from the flight deck was limited. The majority of the witnesses emphasise that the snow they saw was, to a varying extent, towards the wing extremities, where the surface, save for the first few inches, would be out of Captain Thain's view. The aircraft was parked close to the terminal building, and the witnesses in that building had a view from 150 feet or less, and from above. While they differ in detail we think the broad picture they present is a reliable one. We are satisfied that when the aircraft left the apron its outer wings, save for the front portion, carried some snow. The temperature of the fuselage and of the wings in the vicinity of the engines had been sufficient to melt completely the snow that had fallen on these parts. We are satisfied that the snow which lay upon the outer wings was wet and slushy.

<u>The Aircraft after the Accident</u>

60. Captain Reichel, the senior German investigator, landed at Munich at 2200 hours on the night of the accident and went at once to the scene. He did not give evidence before us. His evidence to the first German Inquiry was as follows:

> "The wrecked aircraft was covered with a layer of snow about 8 cm thick. The right wing (which was only slightly damaged) was still firmly attached to the fuselage and had not been exposed to the effects of fire, presented a completely even layer of snow. This was powdery and could be brushed aside with the hand without difficulty. Under this there was a layer of ice (the upper surface of which was very rough) frozen firmly on to the skin of the wing. When one ran one's

hand over it, it felt like a coarse kitchen grater. The very loose powdery snow lying on top had not blended at all with the layer of ice. It could, for example, be blown off without difficulty so as to leave the bare layer of ice exposed. I found the same condition at all points on the wing (which I examined thoroughly) with the exception of the part situated above the engine nacelle and in the region of the slipstream. Here, after the snow had been removed, the bare outer skin was visible, without any ice accretion."

In his evidence to the German Inquiry Captain Reichel said he examined the wing in eight different places at least, and inboard as well as outboard of the engine. He could blow the snow off, finding underneath a clear but very rough layer of ice without any snow clinging to it. "In the area of the engine nacelle and of the slipstream from the airscrews I did not find ice anywhere. The surface of the wing was, underneath the snow, which did not cling, completely smooth and free of ice." On the tail unit, which had been subjected to fire, he found no ice, but the same covering of snow. The ice on the wing was uneven and he got the impression that it was 4 to 5 millimetres thick.

61. Similar evidence was given to the German Inquiry by Count zu Castell, the airport manager, and by Herr Werner Goetz, the airport technical manager. Count zu Castell gave evidence before us. He estimated the ice as several millimetres thick; the investigators had not broken any off or measured it. It was not present on the front section of the wing. Herr Goetz told the German Inquiry that his car on arrival at the scene bore ice which had formed from snow lying on it: and he found similar ice on one of the threshold lights when he examined it at 1800 to 1830 hours.

62. The above evidence of ice on the wings relates to a time six hours after the accident. During those hours the temperature had fallen steadily to -3.0°C and slight snowfall had been followed by moderate snowfall before 1850 hours. Count zu Castell had gone to the scene immediately after the accident: 30 or 40 minutes later he had examined the aircraft's tracks at the western end of the runway and on returning to the wrecked aircraft he found there was a smooth unbroken layer of snow over the fuselage and wing. This was about an hour after the accident. By then the temperature was, he considered, below zero and he decided not to investigate the wing surface himself as he was satisfied that its condition would remain unchanged until the arrival of the official investigators.

63. A body of evidence was called at our Inquiry regarding the state of affairs at an earlier time. Herr Karl-Heinz Seffer was employed at the traffic and rescue services at the airport and went quickly to the scene, arriving before the fire engines. He took part in freeing Captain Rayment, who had been trapped in the cockpit, and stood on the starboard wing

inboard of the engine in doing so. He walked on the wing and descended from the trailing edge inboard of the engine. He was wearing rubber boots. He said he did not notice ice and felt that he would have slipped had there been ice. Herr Tismer went to the scene: he saw several people walking without difficulty on the wing. Herr Kurt Reshke was at the time an ambulance driver at the Munich hospital. He drove a Dr. Huber to the scene. He told us that Dr. Huber climbed on to the outboard end of the wing and, wearing ordinary shoes, ran along it to the fuselage without difficulty. Herr Reshke touched the edge of the wing and found it wet: snow was falling but melting. This witness's evidence is subject to the comment that he made no contemporary statement; his testimony came to light because in 1965 having read a newspaper account of the disaster, with a reference to icing, he telephoned the editor of the paper and made a statement to reporters.

64. Other evidence comprised the statements of Herr Otto Seffer and Herr Gerd Skwirblies, mentioned in paragraph 51 of our 1960 Report, and of Mr. Black, who gave evidence to the first German Inquiry that three hours after the accident he had tried to mask the BEA markings with rubberized tape and had had difficulty is so doing because the side of the fuselage was wet.

65. By far the most important evidence concerning the immediately post-accident situation came from Herr Reinhardt Meyer. Before going to look at the tracks, as narrated in paragraph 31 above, he had investigated icing. "I was thinking about possible reasons for the accident, and I know I was considering whether aircraft icing could have been the reason." He looked at and touched the upper surface of the port wing between engine and fuselage - the only part left attached. He "most probably" looked at and touched the starboard wing. "There was nothing like frost or frozen deposit. There was melting snow only." Shown photographs of the scene, he said "as I look at the photographs, I am sure I saw the starboard wing." And on it he saw snowflakes melting: not into water, but collapsing into slush. Its state was such that, he would have expected it to blow away on take-off.

66. So far as we can ascertain Herr Meyer was the only person to investigate the icing question within a short time of the accident. He looked with the eye of an experienced pilot and aircraft designer. What he told us was of course subject to the hazards of recollection after 10 years, but we should record that in his contemporary report (16th April 1958) he said "I saw no signs of icing either on the fuselage or on any part of the wings." If the ice which the investigators saw six hours later was present during the final run Herr Meyer's evidence can only be explained on the assumption that he looked solely at those parts where Captain Reichel found no ice.

67. Two main interpretations of the post-accident evidence have hitherto been advanced. According to the first, there was melting snow on the wings at departure, this froze on to the outer part of the wing during the run, and after the accident was covered by the snowfall; in the vicinity of the engines and on the roof of the fuselage there was sufficient heat to prevent ice formation. According to the second, what snow lay on the wings at departure was soon blown away; the aircraft crashed with clean wings and both the aircraft and the ambient temperature were sufficient for a time to melt the falling snowflakes. As the temperature dropped the water so produced froze on to the wing and as snow continued to fall it lay upon the ice layer. To help unravel this problem we must look at the expert evidence.

68. Mr. Jones pointed out that the whole of the pre-accident precipitation on the aircraft, about 2 mm (see paragraphs 46, 47 above), could not have caused the 4 to 5 millimetres of ice spoken of by the investigators, because clear ice has a specific gravity of the order of 0.9. He was unable to understand why the investigators found no ice under the snow behind the engines; this area must have been warm at the time of the crash but must have cooled below zero by the time of investigation (otherwise it would not have supported unmelted snow), and "you would then reach a position where the surface was at zero degrees, running wet with snow adhering on top of that. With the ambient temperature below zero there would inevitably be a freezing of this thin film." The only alternative to a film of ice would arise if the surface had not reached zero by the time of inspection, when one would expect to find a film of water, with damp snow above it. (That this surface on the starboard side may still have been warm is not unlikely: paragraph 70 below).

69. The German Commission took the view that if the ice originated in post-accident snowfall there would have been a blending of the ice with the superimposed snow, instead of the separate layers of dry snow and clean ice. Dr. Penman was inclined to agree: "As it is described to me now, there is obviously a discontinuity in the physical system and one feels there must be discontinuity in the history of the formation." Mr. Jones, however, did not agree: "I can visualise a film of water freezing and holding in it a few ice crystals which were floating on the top or resting on the top. Once that had frozen snow could continue to fall ... for some little time on top of that ... and you would still be able to brush away the snow and find the rough ice surface underneath."

70. The German Commission also found significant the absence of ice above the engine nacelles: had icing taken place after the accident it would have occurred here as elsewhere: engine heat would not have persisted long enough to prevent it. To this logical argument two suggested answers were put to us, one in 1960, the other at the present Inquiry. The earlier was the submission that the fire extinguishing powder

used in the area of the engines had lowered the freezing point of water and so inhibited freezing at the material places. We dealt fully with this in our 1960 Report (paragraphs 53 - 56) and reiterate that in our view it does not explain the absence of ice behind the engines under dry snow. The other point was based on an experiment conducted at Gatwick in 1965 to determine the rate of cooling of the oil reservoir in an Ambassador. The engine was run at 1430 hours. At 1445 hours the oil temperature gauge (at the bottom of the tank) showed 60°C. At 1615 hours it showed 45°C and a thermometer reading at the top of the tank showed 50°C. At 2145 hours the gauge was at 25°C and the thermometer reading 30°C. The outside air temperature dropped from +3°C at 1445 hours to -4°C at 2050 hours after which it rose again. At 2145 hours the surfaces of the top of the nacelle were warm to the touch. This experiment demonstrated the action of the oil tank as a heat reservoir and made it clear that the wing would cool much more slowly above the engine then elsewhere. Zulu Uniform's starboard oil tank was intact and not leaking after the accident. However cooling at Munich would be accelerated by the melting of the falling snow: the physical change from ice to water or conversely involves a heat transfer of 80 calories per gramme. (For each gramme of water frozen heat is liberated sufficient to raise 80 other grammes by 1°C.) And this line of reasoning cannot explain what was found on the port side, because the port engine had broken away from the wing.

Post-Accident Evidence on Icing Discussed

71. It is convenient first to deal with the post-accident evidence. We are satisfied that the investigators found ice six hours after the accident in the eight places they looked at. We are satisfied that its thickness was wrongly estimated and that it cannot have exceeded 2 mm and was probably much less. We appreciate that the reasoning referred to in paragraphs 69 and 70 above raises a prima facie case that the ice may have been present at the time of disaster.

72. This prima facie case is, however, in conflict with the evidence of the witnesses who say ice was not present at the time of the rescue operations. Of the rescuers it can rightly be said that their minds were concentrated on their task and they may be mistaken in their recollection of something irrelevant to that task. The same, however, cannot be said of Herr Reinhardt Meyer. He expressly examined the wings for ice and found none. We have seen him and find him an impressive witness. The other witnesses to this period support him. We think his evidence is to be preferred to the deduction from the state of affairs six hours later.

73. What, then, of the arguments founded upon the discontinuity between ice and snow, and on the absence of ice in the region of the engines? As to the former, it is capable of explanation by discontinuity in the snowfall. If wet snow

were falling at first - snow that was largely crystalline ice but with sufficient water to freeze and form the binding agent - it could deposit a layer of ice, given the appropriate wing temperature; if this ceased and were followed after a short interval by a fall of dry snow, there would be nothing to bind the further fall to the ice and the discontinuity would be created. As to the region of the engines, we confess ourselves puzzled by Captain Reichel's finding. Under the dry snow he encountered no ice, no slush, no water. This condition, if accurately observed, could only be caused by a dry snowfall on to a wing already both dry and at or below 0°C. But this part cannot have been frozen at the time of the crash, a few seconds after the engines had been cut. The only explanation we can see is a period of no snowfall during which the moist wing first dried and then dropped to freezing, after which dry snow fell.

74. Our conclusion on the whole body of post-accident evidence is that no ice is proved to have been present on the wings immediately after the accident. It is possible that it was present in some degree, and was not detected by any of the witnesses; possible, but in our view unlikely.

75. If ice had been proved to have been adhering to the wings immediately after the accident it would follow that it had survived the crash and had been present during the run. But the converse is not true, for two reasons. Firstly, the aircraft was subjected to violent and disruptive shock in the accident. The port engine and outer part of the port wing were torn off, the fuselage was broken in two aft of the wings, and the front half of the fuselage, with the intact starboard wing and the stump of the port wing, careered on for about 600 feet and came to rest after having turned through at least 270°. Any unfrozen slush which had not been removed by wind traction would most probably have been dislodged at this time, and the same may well be true of ice, although the degree of probability of ice being removed by impact vibration from the intact starboard wing is no doubt less than in the case of slush. Secondly the fire brigage made liberal applications of fire extinguishing powder to the vicinity of the starboard engine, and this powder, containing sodium bicarbonate, was shown at our 1960 Inquiry to have anti-freeze properties. It went readily into solution in water: in a 1 in 1,000 solution it lowered freezing point to -0.4°C and in 1 in 100 to -3.0°C. It would slowly melt any ice or snow on which it fell. The fire trucks were quickly on the scene - although Herr Karl-Heinz Seffer had preceded them, they arrived at the same time as Herr Reinhardt Meyer, who was himself one of the first to reach the scene from the airport. At our 1960 Inquiry Captain Thain described a fire under the starboard wing which was extinguished and then re-ignited: he had recalled the firemen to put it out again. The apparatus used, he said, produced a powerful jet of powder and it was concentrated around the starboard engine. This engine was given "a jolly good dousing". Though there is no evidence

to this effect it may be that some of the powder was used on the outer wing areas. Wherever it was used it would commence to thaw and remove ice.

76. The post-accident condition therefore provides no answer to the question whether the aircraft made its final run with ice on the wings because such ice may have been removed by impact or melting or both, and we are thus thrown back to our conclusions from the pre-accident evidence, as stated at paragraph 59 above, and must examine what would happen during the final run to the patchy layer of slush on the outer wings.

The Effect of the Evidence

77. In considering the effect of this evidence several matters must be borne in mind. Firstly dry snow falling on a sub-zero surface will not freeze to it. The binding element which converts snow into a frozen mass, and causes it to adhere to a surface, is water, freezing water. If the falling snow is damp it contains this binding element and if it falls on a surface having an above-zero temperature it will commence to melt and again provide the binding element. If in this condition the surface passes below zero binding to the surface will commence, and if the ambient temperature passes below zero the mass will commence to bind.

78. Secondly the temperature was falling very slowly through the relevant period. At 1400 hours, 17 minutes before the aircraft's arrival, the screen temperature was $+0.1^{o}C$. In the next hour it fell one-tenth of a degree to zero precisely, and in the following hour, to 1600 hours, it fell a further one-fifth of a degree, to $-0.2^{o}C$. It must be emphasised that these were temperatures at the meteorological enclosure and that temperatures at the apron or on the runway could vary by what Dr. Penman called "several tenths of a degree".

79. The witnesses agree that when the aircraft last left the apron, at 1556 hours, the snow upon its wings was wet. This means that although the screen temperature had passed fractionally below zero either the ambient temperature at the apron or the wing surface temperature or both were at least fractionally above zero, or the process of freezing was not complete. If this was so at 1556 hours, it was so a fortiori at 1519 hours when the aircraft first departed. It is reasonably clear that what deposit the outer wings then bore was subjected to the effects of wind traction and engine vibration on the first two runs. It is true that wind traction over an aerofoil surface it not so great as a layman might imagine (about 2oz per square foot at 120 knots six feet back from the leading edge) but this slushy snow might well at that time have rested on a film of water which would provide the lubricant enabling the deposit to slide off. This view conflicts at first sight with the evidence of Major Brehme, who said that the aircraft returned from the first two take-off attempts with the snow with which it had departed. We do not think there is necessarily a

conflict. Major Brehme saw snow on the wings at departure and snow on the wings at return; that it was the same snow is an inference which may not be valid. It was snowing at the time and snow falling while the aircraft taxied along the further half of the runway and the taxiway back to the apron may well have remained on the outer wings in sufficient quantity to present the appearance of a thin layer. (The snowfall was recorded as "moderate" at 1520 hours and "slight" at 1550 hours; the aircraft's return was half way between these times and we have no evidence precisely when the change took place).

80. The evidence that in the period before the final departure the snow on the outer wings was "watery" and that "the grey colour of the wings was showing through faintly" indicates that the snow was then still wet. We do not think that any ice or frozen deposit formed on the wings prior to or at the time of the first two abortive runs. Whether it formed at the time of the final run we find it quite impossible to say. There was snow on the outer wings; it was wet snow and if subjected to sub-zero temperature from the wing surface the film of water would in time turn to a film of ice, and if subjected to sub-zero temperature from the surrounding air the mass would crust over and gradually bind like the snow on Herr Goetz's car (paragraph 61 above). In the absence of such freezing the wet mass would probably be dislodged by wind traction or engine vibration. What in fact happened depends on a factor wholly unknown, namely whether on the taxiway and the runway the temperature had dropped sufficiently to freeze the deposit even partially. We have examined whether the wing temperature would have been affected in a downward direction by evaporative cooling or in an upward direction by air friction and have reached the conclusion that the influence of both these factors was negligible. We are left therefore in a state of doubt. Speaking of the recorded temperature drop to $-0.2^{o}C$ and the drop in humidity to 91 per cent Mr. Jones said in the report which he quoted in paragraph 59 of our 1960 Report "Slow freezing was then inevitable provided the air temperature in the meteorological screen was typical of the air over the whole aerodrome. When dealing with temperature to one-tenth of a degree C no meteorologist could state positively that this was so."

81. This state of doubt is not resolved by any other evidence. We have already indicated that the post-accident evidence does not assist us. In Part VI we shall consider the expert aerodynamic evidence; if this had shown that icing could have caused the aircraft to behave in a way otherwise inexplicable it would have helped to solve the matter, but as we shall see the expert evidence has no such effect. All we can say at the end of the day is that the outer wings of Zulu Uniform on its final run may or may not have carried a thin deposit of ice or frozen snow.

Part V THE QUESTION OF SLUSH

82. In considering the evidence about slush on the runway it must be borne in mind that it is the amount and velocity of the material displaced by the aircraft wheels that governs the degree of drag caused. The density, therefore, as well as the depth, of the material must be considered, and in the circumstances of this accident one must see whether there is evidence of changes in depth or density in different parts of the runway.

Slush Depth - Evidence

83. The principal witness was Herr Kurt Bartz, the Traffic Manager at the airport. With his deputy, Herr Alfred Meyer (not to be confused with Herr Reinhardt Meyer), he inspected the runway between 1535 and 1544 hours. The times can be fixed because the inspection was between the second abortive run of Zulu Uniform at 1535 hours and the take-off of another aircraft at 1544 hours. Herr Bartz and his assistant were in a car. They traversed the runway from east to west, and on either two or three occasions stopped and got out and examined the runway surface. They reported by radio: "runway covered with thin layer of slush". In a statement made three days later Herr Bartz said "the entire runway was covered with a thin watery layer of slush about ½ cm to ¾ cm thick". To the first German Inquiry (April 1958) he described the slush as "not snow but a watery gelatinous mass ... it was transparent, you could see the colour of the concrete through it." In the wheel tracks it was pure water. Asked whether the inspection lasted 10 minutes, he replied "no, not so long, perhaps 3 to 4 minutes." Asked "did you get out once or several times?" he replied "we left the car twice."

84. On 14th August 1959, when the authorities were considering the request to re-open the Inquiry mentioned in paragraph 6 above, a further statement was taken from him, in which he said "We examined very carefully the condition of the runway ... not just in the middle of the runway but very thoroughly on both sides as well. We got out to make sample tests on both sides ... to the right of the runway there is a natural slope which quickly drains off the water. On the left of the runway there is a special drainage arrangement." When, six years later, the second German Inquiry was held,

Herr Bartz again gave evidence. He was now more precise: he said inspections were made at three points on the runway, at about 200 metres from the east threshold, at about the middle, and at about 200 metres from the west threshold; at each place three inspections were made: at the centre line and at about 15 metres left and right of the centre line. (The runway was 60 metres wide). He said that at each spot he and his deputy measured the depth with a ruler. The depth was uniform from end to end of the runway: it was less at the centre than at the sides: the $\frac{1}{2}$ cm measurement was at the centre and the $\frac{3}{4}$ cm measurement at the 15 metres point right and left. The deputy, Herr Alfred Meyer, gave evidence, for the first time, at that Inquiry.

85. Herr Bartz gave evidence at our Inquiry. When it was pointed out that he had originally said he left the car twice but in 1965 he spoke of three occasions he answered "during the second examination we stated that we left the car three times, and this arose from the fact that during the second examination Herr Meyer was present and it emerged that in fact we left the car three times and not twice." It would appear that Herr Bartz in giving details was relying rather on his assistant's recollection than his own. We do not criticise him for this: it would be an exceptional person who retained detail after seven or ten years without refreshing his memory. In our view what is said within a few weeks or months of an occurrence is a more reliable guide than what may be produced from joint recollection and discussion years later.

86. Herr Alfred Meyer made no statement to the authorities at the time, and his recollection was first recorded at the second German Inquiry, in 1965. He spoke of the three checks at each of the three points but naturally enough could not be entirely positive: "I would not like to assert so firmly that three checks were made at each point. It is not impossible that one check may have been omitted. It is quite conceivable." "We measured 5 mm of slush in the middle, and on both sides the slush was a little higher: it was 7 mm. The wheel tracks went through this to the concrete: there was thus no residue of slush remaining on the concrete. There may have been some water in the tyre tracks but not to the height of the slush we measured."

87. Both witnesses spoke of the grass of the airfield being covered with snow, white in contradistinction to the grey or transparent covering of the runway: they explained that the concrete retained heat. They saw no ridges at the edge of the runway or variations in the cover anywhere on the runway.

88. There were two classes of other witnesses to slush, those who went to the western end of the runway after the accident, and two pilots who used the Runway, Captain Thain and Captain Wright. About half an hour after the accident Count zu Castell and Herr Endrass (an official of the Bavarian State Ministry for Economic Affairs and Transport) walked on to the western end of the runway and found the

slush up to the top of the soles of their shoes - ½ cm to 1 cm. This they related to the second German Inquiry. Herr Endrass walked at least 100 metres, and Count zu Castell went further, 250 - 300 metres.

89. Herr Reinhardt Meyer gave his recollection of the depth of slush when he examined the wheel tracks (paragraph 31 above) as ½ inch (1.27 cm.) Herr Tismer went to the blazing hut and then to look at the runway: he said "there were certain parts which had no snow on it because there were wheel tracks, but next to them there was snow at least 5 cm to 10 cm thick." Major Brehme, from a distance, estimated the snow on the runway as 10 cm. (For comparison the depth of the snow at the met. station was measured at 4 cm - 5 cm. Dr. Mueller at the first German Inquiry reported it as max. 5 cm. He added "of course one cannot compare the end condition of the snow in the climate garden with the end condition of the snow, or the snow substance, on the runway, where planes have taken off with propellers.")

90. Captain Wright landed his Viscount at 1558 hours. In his contemporary statement he said "the runway surface was covered 1 to 1½ inches of snow and slush, the NE end, i.e. the first two-thirds of the runway appeared to be snow, but the last third was mainly slush with some small bare patches (wet) in the centre of the runway with deeper snow at the sides." He repeated this in giving evidence at our 1960 Inquiry (see paragraph 73 of the 1960 Report), and said he was judging the slush by the depth of the ruts left by other aircraft. His eye level was about 10 feet above the ground. The slush retarded his aircraft: with a bare runway he would have had to brake but on this occasion he had slowed to taxying speed by about the mid-point of the runway and thereafter applied power. When he took off at 1720 hours he took about seven seconds longer than usual to unstick and used about two-thirds of the runway. He noticed no deceleration at any time on the take-off run.

91. Evidence was also given in 1960 by Captain R. T. Merrifield. He landed another BEA Viscount at Munich two days after the accident. By then it was thawing: the runway appeared clear enough although the surrounding grass was still snow-covered. About halfway along the runway he found a pool of water 200 - 300 yards long and extending between the northern edge and the centre line. He landed on the south side to avoid it.

92. Captain Thain's recollection, given to our 1960 Inquiry, was that on touch-down he thought he was on packed snow, but "when we got further down the runway we found that the precipitation or snow on the runway was rather different: it was watery, there were some bare patches, and the braking effect was quite satisfactory."

Slush Evidence Discussed

93. In evaluating the evidence, both density and depth need to be considered. As to density, the evidence indicates that the slush, while retaining sufficient ice to allow ruts or tracks to be made, was largely water, and that its density was, at all events in the western part, little less than that of water. This however is in apparent conflict with expert evidence which we have yet to consider, and we postpone further discussion. See paragraph 112 below.

94. As to depth, Herr Bartz was convinced that there was an even deposit over the entire runway, so level that the only difference, between 5 and 7 or 8 millimetres, was caused by the runway's slight camber. We are bound to say that we find the picture of billiard table evenness somewhat unreal. It is only rarely that snow falls so regularly as to produce a completely even cover. This was not a still day. The wind at the time of the accident was given as from 300^{0} at 8 kts. Herr Reinhardt Meyer, in his statement of 16th April 1958 said "from 11 am there had been fairly severe driving snow and at least two squall fronts had passed over." Moreover the runway was frequently disturbed and slush scattered by aircraft taking off and landing.

95. The extensive experimental investigation of slush drag to which we refer later has necessitated an examination of methods of evaluating slush depth. Mr. Maltby and Mr. Illingworth (their evidence will be referred to in paragraphs 108 - 111 below) told us that it is rare for comparatively few measurements to give an accurate picture. Save in exceptional circumstances it was necessary to take a very large number of measurements. In one representative case 450 measurements were taken of a snow covered runway 10,000 feet long and 300 feet wide and the depth of snow was found to vary between $\frac{1}{2}$ inch and 8 inches. Air temperature was -5^{o}C. No doubt melting would reduce the peaks and increase the valleys but in the circumstances described at Munich we think it probable that had measurements been taken at points other than the three, or two, spoken of by Herr Bartz readings both above and below those of $\frac{1}{2}$ cm and $\frac{3}{4}$ cm would have been obtained. We also think that the measurements would have been significantly greater at the edges than at the centre of the runway. As we have seen, the witnesses who gave an estimated depth differed widely in their recollections. Further consideration of the question of slush must be postponed until we have dealt with the expert evidence.

Part VI SCIENTIFIC EVIDENCE

96. In 1962 litigation was pending between the Manchester United Football Club as plaintiffs and British European Airways as defendants in respect of the loss sustained by the former in the Munich accident. Each side consulted an expert in the field of aerodynamics. The club consulted the Professor of Fluid Mechanics at the University of Manchester, Professor P. R. Owen, MSc, FRAeS, now the Zaharoff Professor of Aviation and Head of the Department of Aeronautics at the Imperial College of Science and Technology, London University. BEA consulted Professor A. D. Young, OBE, MA, FRAeS, AFAIAA, head of the Department of Aeronautical Engineering at Queen Mary College, University of London. The reports made by both these gentlemen to their clients were put before me, and in addition they both gave oral evidence. The reports were privileged documents and we are indebted to both the Manchester United Football Club and British European Airways for allowing them to be produced.

Professor Owen's Evidence

97. Professor Owen's first report to his clients, made in November 1962, was based upon an examination of the data furnished by the first German Report and our Report of 1960. He thought that slush drag must have been an important factor in the case and pointed out that the German Report assumed that the effect of slush decreased with speed whereas subsequent experiments had shown that, up to a certain speed at least, slush drag increased with speed. He felt that further investigation was necessary upon (i) the effect of ice deposits on the lift and drag of the aeroplane's wings; (ii) the effect of slush in the light of the most recent data; (iii) the evidence of certain eye-witnesses. As "a matter of some urgency" he asked to see the Technical Report prepared for the German Commission of Inquiry by Professor Dr. H. Schlichting, the Director of Flow Mechanics at the Technical University of Brunswick, and Dr.-Ing. K. Gersten, then of that Institute and now Professor at the Institute for Thermo- and Fluid Dynamics at the Ruhr University. This Report was, and is, available in English translation (it is referred to at paragraphs 18, 19 and 22 of our 1960 Report) and a copy was then furnished to Professor Owen.

98. After examining this document Professor Owen on 3rd January 1963 wrote a letter to the solicitors to his clients from which we take the following extracts:

> "My anxiety to examine the Schlichting report has turned out to be well-founded, but the result in my opinion is distressing from the point of view of your claim.
>
> I have examined Schlichting's calculations very carefully and have come to the conclusion that his estimate of the influence of the ice deposit on the lifting effectiveness of the aeroplane's wings was grossly exaggerated.
>
> The corresponding increases in drag which I calculate are also much smaller than Schlichting's; for the aeroplane as a whole, they lie between one-eighth and one-tenth of Schlichting's values.
>
> Since Schlichting's report was sent to me only recently my preliminary opinion was of necessity based on the information contained in the Reports of the German Commission and the British Reviewing Body; like them, I assumed (with reservation) the correctness of Schlichting's work.
>
> In the light of my present calculations I am forced to conclude that icing on the wings had at most a marginal influence on the take-off characteristics of the aeroplane and that slush must have been the dominant factor in accounting for the accident.
>
> It is a great pity that I have been forced to such a decidely adverse opinion at so late a stage, but it is one that could only be reached after a detailed study and check of the Schlichting Report which I received, as you know only two weeks ago.
>
> My view now is that the only way in which icing could have prevented take-off was for circumstances to combine in such a way that near the end of the runway the pilot attempted to lift the aeroplane off the ground during just the short interval of time (how short, it is impossible to say) during which the speed lay between 108.6 knots * and 1 or 2 knots higher; that is to say, by the barest chance, he chose the moment when the speed was between what was sufficient for take-off with a clean wing and what was necessary because of icing. If Schlichting's calculations were acceptable the difference between these speeds would have been large enough to have made such an event probable, for it would have amounted to about 10 knots; but with a difference of only 1 or 2 knots the best that can be said is that the possibility existed.

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*See paragraph 115 below.

I have also made extensive calculations to discover whether the ice had any substantial effect on the speed of the aeroplane at a given position along the runway. My conclusion is that if the wings had been free from ice the speed towards the end of the run would have been 1 knot higher at the very most. This demolishes the possibility of arguing that if the wings had been clean the aeroplane would have reached its take-off speed before encountering whatever it was that caused the retardation at 117 knots.

My frank opinion of the accident is this.

(a) The effect of ice on the take-off run was negligible.

(b) The failure to reach V_2, the take-off speed, can be accounted for in a convincing manner by slush on the runway. In this report the reconstruction suggested in the Report of the British Reviewing Body is highly plausible, and it is extremely likely that the behaviour of the aeroplane near the end of the take-off run was due to an encounter with either denser or deeper slush.

(c) The effect of ice on the take-off speed was only 1 or 2 knots. While it is possible that this could have accounted for failure to take-off, it seems to me that the chance of the pilot having selected just the moment when the speed lay between what was needed for take-off with an iced wing and what was possible with a clean wing are slender indeed.

(d) It is highly probable that the accident can be attributed entirely to slush on the runway."

99. Professor Owen made a written statement to us, which he confirmed in the witness chair. This statement is reproduced in full in Appendix 3, and may be referred to for the reasoning leading him to say that the effect of icing would have been limited to (a) reducing the aircraft's speed by a maximum of 1 knot, and (b) increasing lift-off speed by a maximum of 2 knots. It must be borne in mind in reading Professor Owen's evidence that he was assuming as a fact that ice was present to a certain degree (see paragraph 121 below). Whether or not there was in fact ice present to this or any other degree is a matter we consider later.

100. In his evidence Professor Owen emphasised that his calculations were on the pessimistic side. He had not, for example, allowed for a compensating increase in lift which might well have been present. On this he said:

"We have at our disposal data on the effect of roughness on lift, but these data usually apply to two dimensional wing sections, or to wings as in the case of the RAF 34 and the NACA 0012 sections as reported some time in the

R & M Series; they applied to a wing with finite aspect ratio but uniformly roughened. If part of the wing is roughened and that part of the wing suffers a decrease in lift coefficient, then one knows from the behaviour of wings of finite aspect ratio that the decrease in lift coefficient on the wing as a whole is not quite as marked as one would expect from simply the figure one obtains for the impaired part. There is the tendency of wings in finite section to even things out in that if one has a wing of finite aspect ratio and the outer parts are operating less effectively than the inner parts, the decrease of lift associated with them is partly compensated by an increase in lift over the inner section."

Professor Young's Evidence

101. Professor Young made his report to his clients towards the end of 1962. Like his colleague he took his facts from the two then published Reports, and he had Dr. Schlichting's technical report from the outset. His report is a long and closely reasoned document which we can only summarise briefly here. He begins by a study of the state of the runway, considering the total precipitation since the evening of the day before the accident, and relating it to a study of a runway at Bedford in comparable circumstances. He then says "I can only infer that the equivalent water depth lay between ¼ in and 1 in, and that indeed it may have reached both these limits at different parts of the runway and that the depths over the second half of the runway may have been significantly greater than over the first half."

102. Next he studies the extent and nature of the deposit on the wings of the aircraft. From the evidence of eye-witnesses he deduces the extent: "I have decided to work on the assumption that a deposit on the wings occurred only outboard of the propellers ... the first 10 per cent of the wing back from the leading edge was clear of deposit." As to thickness, after a lengthy examination of the factors involved, he concludes: "Since it was largely water and therefore of high density its actual depth was not more than about 3 mm. Finally he concludes that an equivalent sand roughness of such a layer might be about 0.4 mm.

103. He then proceeds, first to an examination of the drag effect of the runway slush, and then to an examination of the drag and lift effects of the deposit on the wings. The latter subject he treats as length; and his conclusion is: "It is immediately apparent that the effects of the 0.4 mm roughness on both lift and drag are far smaller than the corresponding effects calculated by Schlichting for 0.3 mm roughness" and he gives reasons for this.

104. Professor Young next proceeds to a series of calculations in which he varies a series of parameters, viz:

(a) slush drag coefficient

(b) state of wing surface

(c) attitude of the aircraft

and studies for a range of combinations various relations between slush depth, speed achieved, and runway distance covered. From the trends disclosed by these calculations, he deduces that the effect of slush far outweighs that of deposit on the wings: as regards the latter "hence, with 0.4 mm roughness on a 3 mm layer the lift-off speed will be increased by about 1 knot." He adds that if conditions happened to be so critical that differences of take-off speed of the order of 1 knot become significant, account must be taken of other factors, in particular the download on the tail unit needed to balance the nose-down moment on the aircraft as a whole,* and a possible weight contribution from impinged slush.

Professor Young's Reconstruction

105. Finally, Professor Young embarks on a possible reconstruction, basically the same as that given tentatively in Appendix 2 to our 1960 Report, but using different values of the parameters involved. In particular, the equivalent water depths assumed in the two studies are as follows:

	First part of runway	*Second part of runway*
Appendix 2 (1960)	0.43 inches	0.64 inches
Professor Young	0.30 inches	0.75 inches

Thus Professor Young assumes less depth over the first part of the runway, but more over the second part. Accordingly, in his suggested reconstruction the initial acceleration of the aircraft is greater, but the deceleration on entering deeper slush is also greater.

106. Professor Young's reconstruction may be summarised as follows**. If the water equivalent depth of the slush changed from 0.3 in (7.6 mm) to 0.75 in (19 mm) this would increase by 5,700 lb the drag force on the main wheels and result in a nose-down moment, to balance which about $3\frac{1}{2}^{o}$ of up elevator movement would be required. But the pilot would not have had time to make the necessary elevator movement before the nose wheel re-entered the slush, and the consequent increased drag would reduce indicated air speed from 117 knots

*See also Professor Owen, Appendix 3, paragraph 7(iii)

**For our Inquiry Professor Young recalculated the loads and elevator angles used in his reconstruction. His amended figures are given in this summary.

to 110 knots in about $7\frac{3}{4}$ seconds. To raise the nose, the increased drag would add 3^{o} of elevator movement to the $3\frac{1}{2}^{o}$ already required. If by the time the aircraft decelerated to 110 knots the pilot had obtained the necessary $6\frac{1}{2}^{o}$ of up elevator the nose would rise, but as soon as it was free of the slush the reduced drag would produce a nose-up moment which would rotate the aircraft until the tail wheel was on the ground. Professor Young then postulated the crucial question: could the aircraft have lifted off at this point? He concluded that the lift generated at that stage was very near the all-up weight of the aircraft but added that the $6\frac{1}{2}^{o}$ of up elevator would increase the download by some 2,000 lb which would be decisive in keeping the main wheels on the ground. If aircraft weight had been increased by the weight of impinged slush then lift-off would have been even less likely.

107. Professor Young summed up his views as follows:

> "It is concluded that the vital cause of the accident was slush on the runway and this was very probably the only cause. The effect of the deposit on the wings was at the most of marginal significance and then only when conditions were already very critical owing to slush. Under such conditions, however, there may be other factors, hitherto excluded from consideration, such as the download due to the elevator angle used to initiate rotation against the moment of the nose wheel slush drag and the weight and adverse aerodynamic effects due to impingement of slush on the aircraft which could have affected the take-off at least as much and probably much more than the snow deposit on the wings."

The Slush Tests

108. In 1961 and 1962 the Royal Aircraft Establishment, Bedford, developed experimental methods of measuring the effect of slush on the drag of an aircraft, and in November 1963 tests using these methods were applied to an Ambassador aircraft similar in all respects to Zulu Uniform. The results were published in April 1964 (RAE Tech. Note Aero 2968). Other aircraft were subsequently tested similarly, and a full report published in May 1968 (RAE Technical Report 68107). In addition the RAE prepared a paper *Application of the Results of Slush Drag Tests on the Ambassador to the Accident at Munich* which was published with the English translation of the second German Report (CAP 292). Evidence was given to us by Mr. R. L. Maltby, BSc, AFRAeS, a Principal Scientific Officer of the RAE, Bedford, and by Mr. J. K. B. Illingworth, MA, a Principal Scientific Officer in the Directorate of Civil Aircraft Research and Development, Ministry of Technology, both of whom were concerned with the above-mentioned tests. Mr. Illingworth was the author of the paper printed in CAP 292. We were shown a film of the Bedford tests and also a film "Project Slush" demonstrating similar tests conducted by the US National Aeronautics and Space Administration.

109. We need not recapitulate the details set out in the published material. The most important feature is that experimental data have been obtained to supplement and verify calculated data and that the drag effect of differing depths of slush on an Ambassador at different speeds can now be calculated with reasonable precision. Mr. Illingworth produced to us the graph reproduced at Appendix 4. This demonstrates that whereas the aircraft on a dry runway would attain V_1 (117 knots) after 3,000 feet, it would in slush of an equivalent water depth of 8 mm require nearly 5,000 feet to reach the same speed, and if the depth were 10 mm it would not attain 117 knots within the length of the runway.

110. Two other relevant matters emerged from the slush drag tests. The first is the question of spray. In our 1960 Report (paragraph 58) we suggested that the spray thrown up by the aircraft might have formed a source of precipitation leading to wing icing. The spray pattern thrown up by the Ambassador at 55, 70 and 85 knots is illustrated at fig. 24 of the Technical Report 68107 and Mr. Maltby gave evidence on the subject, which was also illustrated by the film. The tail and underside of the wings were subjected to spray, but little or none reached the upper side of the wings. Mr. Maltby reported that visible spray rose over the upper surface only on two occasions out of the 33 analysed. On one occasion at 50 knots in 1¼ inches depth of water a wisp of spray persisted for about one second and on the other, at 60 knots in 1 inch depth of water a light wisp persisted for about half a second. These were, in both instances, inboard of the engines. In Tech. Note Aero. 2968 it was suggested (page 8) that spray thrown against the underside of the tail-plane would produce a large nose-down pitching moment.

111. The second matter was aquaplaning. At a certain speed a wheel rotating through water ceases to displace all the water in its path. Thereafter there is water between the tyre and the runway. When this aquaplaning occurs drag is decreased and braking power is lost; it is even possible for the wheel to cease rotating on its own. The Bedford tests showed that an Ambassador, with tyres of the same pattern as employed at Munich, and with similar all-up weight, commenced to aquaplane when running over a water covered surface at about 75 knots. But it was clear that Zulu Uniform had not aquaplaned although substantially exceeding this speed. On its first run take-off was abandoned at 105 knots and when thereafter the brakes were applied retardation was found to be normal. (The speed of 105 knots was given by Captain Thain, and accords with the data shown on the Peravia recorder). On the final run all four main wheels were locked and skidded on the runway at the western threshold (paragraph 30 above) when the aircraft's speed probably exceeded 100 knots. It is thought that the onset of aqua-planing varies with the weight of material to be displaced and Mr. Maltby deduced that aquaplaning was postponed because the density of the slush was considerably less than unity. He thought the specific gravity of the slush, at places where the aircraft exceeded aquaplaning speed on water, could

not have greatly exceeded 0.5. He said "I can think of no other reason, but we do not understand the phenomenon well enough to be absolutely sure there is no other reason." If this reasoning is accepted it throws light on the composition of the slush on the runway. Moreover it means that slush drag went on increasing as speed increased, and did not diminish at the aquaplaning point. Mr. Illingworth's performance graph, Appendix 4, was drawn on this assumption.

112. We have no doubt that the aircraft did not aquaplane. However in view of the evidence of eye-witnesses that the slush was largely water we doubt whether the specific gravity was as low as 0.5. The phenomenon of aquaplaning is not fully understood, and its onset is affected by tyre pressure and slush viscosity and possibly other unknown factors, as well as by specific gravity.

Wing Icing

113. On ice formation we had evidence (in addition to that already mentioned) from Mr. Roman Szukiewicz, IngDip, AFRAeS, AFAIAA, Assistant Chief Aerodynamicist with Hawker-Siddeley Aviation Ltd., and from Mr. Eric Cyril Maskell, MA, FRAeS, FIMA, a Senior Principal Scientific Officer at the Royal Aircraft Establishment at Farnborough. We also took into account the evidence given at the 1960 Inquiry by Dr. Penman (paragraph 47 above), and by Mrs. R. V. Thain, BSc.

Mr. Szukiewicz's Evidence

114. Mr. Szukiewicz produced two reports of test flights in icing conditions during the Ambassador's Certificate of Airworthiness trials in 1951 and 1952. In a paper presented to the Inquiry he analysed the test results and examined their application to the facts at Munich. From the tests he drew the conclusions that icing caused no less of lift and that up to the maximum angle limited by aircraft geometry on the ground there was no premature flow separation which could be responsible for excessive drag increase: the drag increase of ice accretion would only be due to an increase of coefficient of friction because of roughness and local protuberances. He made calculations based on the most severe interpretation of the data, and on the assumption that the wing, from start of fuel tank to wing tip, was covered from 15 per cent chord to the trailing edge with a 5 mm layer of ice having dimples 3 mm deep - the total iced area being 600 sq ft. The result, he found, was that making the most pessimistic assumptions on suggested ice accretion, its contribution to deterioration in performance was negligible:

> "The take-off distance to 117 knots IAS will increase from 3,070 ft to 3,170 ft, and the acceleration at 117 knots IAS with nosewheel on the ground and clean wing of 2.7 knots/sec will reduce to 2.1 knots/sec for the case of tail bumper on the ground (angle of

incidence = $8\frac{1}{2}^{o}$) and with ice on the wing. Most of this loss of acceleration is due to incidence, the contribution due to ice being 0.14 knots/sec only."

Unstick Speed

115. From the aircraft performance data in his possession Mr. Szukiewicz produced a graph showing unstick speeds of an Ambassador taking off with clean wings in the circumstances obtaining at Munich. We reproduce this at Appendix 5. It will be seen that with the tail wheel just touching the ground the angle of incidence is 8.3^{o} and unstick speed is 111.8 knots. With the tail wheel assembly fully compressed the angle is 9.5^{o} and unstick speed 105.5 knots. (Professor Schlichting and Professor Owen (see paragraph 98 above) had taken 108.6 knots as minimum unstick speed: this is at an angle of incidence of 8.8^{o} and assumes a partially compressed tail assembly). At V_1, or 117 knots, the aircraft would unstick at an angle of incidence of 7^{o}. The above speeds are all indicated air speeds.

Elevator Movement

116. Mr. Szukiewicz dealt also with the question of elevator angle. He said that under normal conditions he would expect the elevator angle required for take-off at 117 knots to be about 10^{o} out of a total available elevator up movement of 20^{o}($\pm1^{o}$). If one postulated a pitching moment due to a slush drag of 7,500 lbs an additional 4.5^{o} of up elevator movement would be required. "In addition there will be a loss of elevator movement due to elevator cable stretch amounting to 2.9^{o} for each 100 lbs of stick force. To allow for pitch acceleration in this condition it is likely that, for all practical purposes, a pilot would need full elevator for take-off." (It will be noted that Mr. Szukiewicz's 4.5^{o} compares with Professor Young's figure of 6.5^{o} (paragraph 106 above). This difference is mainly due to the different amounts of drag assumed.)

Opinion of Mr. Szukiewicz

117. From his analysis of flight test results Mr. Szukiewicz drew the conclusion that the accident could be explained by the presence of slush alone. He concluded:

> "In my opinion in reconstructing the accident the sequence of events was as follows. With a moderate amount of slush on the runway the aircraft accelerated to V_1 = 117 knots IAS. This speed for all practical purposes is the rocking speed, as V_2 is achieved one second later under normal conditions. At this moment the aircraft entered a patch of deeper slush or puddle of water as described by Captain E. R. Wright at the Inquiry, which caused a deceleration. Two possibilities occur. Firstly, at 117 knots IAS Captain Rayment initiated rocking procedure, which was delayed due to a much higher stick force and stick movement than he was

accustomed to, and if he had raised the nose at all this would have dropped again with the increase of pitching moment due to the deeper slush. Or secondly, the pilot had raised the nosewheel well before 117 knots IAS, and at 117 knots IAS on entering a patch of deeper slush the aircraft experienced a violent nose down pitching moment, taking him by surprise. In either event he lost the chance to unstick at 117 knots IAS or thereabouts, and when the aircraft decelerated to 105 knots IAS he was already below minimum unstick speed, namely 106½ knots IAS."

118. Mr. Maskell produced a paper *Effect of Ice on the Ambassador Wing* which is reproduced in its entirety at Appendix 6 in order that his method may be fully appreciated. Broadly the same method was employed by Professor Owen and Professor Young when they reached similar conclusions in 1962/3. As will be seen from fig. 3 to this Appendix, Mr. Maskell concluded that icing of the considerable amount which he assumed would have little or no effect on lift at angles of incidence less than 6½°. At 8½° Mr. Maskell's curve indicates that lift-off speed would be increased by 2 to 3 knots.

Examination of the Scientific Evidence

119. The experimental data upon slush drag obtained from the RAE Bedford tests accorded well with the theoretical data previously used. We entertain no doubt that the drag effect of slush of known depth and density on an Ambassador aircraft can now be accurately assessed within a narrow margin of error and that such results as Mr. Illingworth's performance curves in differing depths of slush are firm guides to the behaviour of Zulu Uniform on the slush covered runway at Munich.

120. One cannot however reach such certainty as regards the effect of wing icing of the kind we have to consider, because in this case no experimental data for the Ambassador are available. For empirical information one has to rely on the broad effect of the flight tests, in which the icing was of a totally different kind. But the calculations made by Professor Owen, Professor Young and Mr. Maltby upon theoretical data achieve substantial unanimity and correspond substantially with Mr. Szukiewicz's deductions from flight test data. We do not think that there is much room for doubt that their calculations as to drag produce a reliable result. With lift there is perhaps more room for doubt because here we are making assumptions in the difficult field of boundary layer development and separation as regards an unusual wing section. This aerofoil section (NACA-65_2-415) was designed as a laminar flow wing and if it functioned as such it would be particularly sensitive to surface roughness. However having inspected an Ambassador we appreciate from the position and multiplicity of rivet-heads upon the wing surface that it is unlikely so to function. The witnesses

agreed that it would be valuable to calibrate their forecasts against experimental results; Mr. Maskell did not think his error would be found to exceed 10 per cent and said he would be concerned if it was as much as 30 per cent.

121. The witnesses reached their conclusions as to the effect on lift by different paths and upon different assumptions. The assumptions and conclusions are tabulated below:

EFFECT OF ICING ON TAKE-OFF SPEED

	Upper wing surface area iced	Ice thickness	Roughness height	Increase in minimum take-off speed
	%	*mm*	*mm*	*knots*
Mr. Maskell	40	5.0	3.0	3
Professor Owen	45	2.0	0.5	2
Professor Young	45	3.0	0.4	1

122. These results are very different from those arrived at by Professor Schlichting and Professor Gersten, the authors of the "Technical Report" (paragraph 97 above): the latter made the assumptions which were followed by Mr. Maskell but arrived at an increase in minimum take-off speed of 15 knots. The reasoning of the German experts was criticised by both Professor Young and Professor Owen, and we thought it right that Professors Schlichting and Gersten should be made aware of the divergence of views and the criticisms. We accordingly caused a summary to be sent to them, with an invitation to comment. The invitation was accepted, and Professor Schlichting and Professor Gersten sent us a paper setting out their answers. This paper was shown to Professors Owen and Young, and also to Mr. Maskell, and each of these three witnesses furnished us with their written comments. All these documents are reproduced in Appendix 7.

123. It will be seen that the unfortunate disagreement between the two bodies of expert opinion remains unresolved. It is essential for us to form our own view on the matter because the causes of the accident cannot be satisfactorily examined without postulating the effect upon drag and lift of a given quantity of wing icing. After studying the arguments we have reached the view that we ought to accept the conclusions reached by Mr. Maskell, Professor Young and Professor Owen. We have however been particularly impressed by two of the points made by Dr. Schlichting and Dr. Gersten, and we must explain why these points do not affect our acceptance of the British witnesses' conclusions.

124. The first of these points is that Dr. Schlichting and Dr. Gersten have assumed the presence of ice on the lower as well as the upper surfaces of the wings and that this assumption produces a conservative estimate of the loss of lift. In simple physical terms, a deposit on the rear upper part of a wing would have an effect like that of an upgoing aileron: it would decrease the lift. Conversely, a deposit underneath could increase the lift; together, a smaller reduction of lift would result than when the lower surface is free of ice. Mr. Maskell concedes this argument qualitatively. Quantitatively, however, Mr. Maskell agrees with Professors Owen and Young that the actual loss of lift is trivial compared with that deduced by Professors Schlichting and Gersten; all three agree that the minimum unstick speed would be increased by only from 1 to 3 knots.

125. The second point is made by Professors Schlichting and Gersten in paragraph 10 of their paper in Appendix 7, namely that no wind tunnel tests had been made to determine the effect of icing on the aerofoil section of the Ambassador. They suggested that such tests ought to be carried out and that test results would form an acceptable basis for re-examination of the assumptions underlying their calculations.

126. We have given long and careful consideration to this suggestion. We have had in mind from the outset of the Inquiry the possibility of requiring icing tests to be made either on models in a wind tunnel or on a specimen Ambassador in flight. But in his paper in Appendix 7 Professor Owen points out the difficulty of defining datum conditions to be tested, and Mr. Maskell in his paper considers that no technical case for further tests has been made out. Our view is that, while tests would undoubtedly yield valuable data, there is not a sufficient degree of uncertainty in the calculations presented to us to warrant this course being taken. We think it unlikely that such experiments would in fact show anything at variance with other experiments and calculations. There is also the practical consideration that suitable wind tunnels are at present fully employed and that by the time a test programme could be set up, conducted and evaluated very considerable time would elapse and our Report would be accordingly delayed. Therefore, attractive though the suggestion of tests may be, we have decided not to ask that they be carried out.

127. There is one further matter to be mentioned before parting with the scientific evidence. Mr. Szukiewicz gave evidence at the second German Inquiry. In their Report the Commission said (pages 12 - 13):

> "Mr. Szukiewicz's comment to the effect that the Elizabethan NACA-65_2-415 aerofoil section, in particular, is less sensitive to icing than the sections of older aircraft types doubtless applies

to wing leading edge ice but not to the kind of ice which formed on G-ALZU.

The effect of icing on the flow round a section, and hence on drag and lift, is governed by the section shape and the corresponding pressure distribution. Owing to the special shape of the NACA-65_2-415 section it is probable that ice or any other protuberance at the leading edge has a small effect while ice accretion in the central region of the upper surface has an effect on drag and a particularly large effect on lift."

Professor Young, Mr. Szukiewicz and Mr. Maskell all disagreed with the view there expressed. They all asserted that ice had a greater adverse effect on lift if present at the leading edge than if present further back. We share this view. It is also to be observed that on one of the test flights the de-icing equipment was switched on and cleared the leading edge; the water ran back and froze on the after part. Lift data obtained on this occasion supported Mr. Szukiewicz's findings.

128. Our considered view on the difficult question of the effect of ice on lift is that we find the evidence of the three British expert witnesses acceptable and we are able to use as data their conclusions as tabulated in paragraph 121 above, subject to this, that in the absence of experimental data a considerable margin of error ought to be allowed.

An Ambassador Incident Examined

129. This is an appropriate place to mention an incident with another Ambassador which came to our notice. We had evidence from Captain K. J. Moody, Ambassador Flight Manager with Dan-Air Ltd. In the winter of 1963/4 he was first officer on an Ambassador which took off from Gatwick in freezing conditions after being de-iced. As the aircraft reached a height of 400 feet the port wing dropped and the aircraft went into a diving turn to port. It required the strength of both pilots to control this movement, as they were fortunately able to do after losing some 200 feet height. The crew felt that there had been a control surface malfunction but no defect was found. A few weeks later a similar incident occurred and in consequence the company had the aircraft tested by De Havillands. Captain Moody received a message that it was thought that the incident had been caused by rime-ice inboard of the engine on the port side. Unfortunately it has proved impossible after the lapse of time to trace any report on this incident and nothing further can be ascertained about it. We inquired whether BEA had any knowledge of any similar occurrence with an Ambassador and were informed that they had not. There this matter must be left.

130. We can now state our findings and views, firstly upon what happened on the final run, and secondly upon what were the immediate or physical causes of the disaster.

The Final Run

131. We are satisfied that the aircraft left the apron with some wet snow on its outer wing surfaces. Whether this was all swept away by wind pressure and vibration, or whether any of it adhered or turned to ice during the final run we cannot say with any certainty. By "ice" we include not only sheet ice but also any slush frozen and adherent to the wing. We accept Herr Reinhardt Meyer's evidence (paragraph 65), supported by the less cogent evidence of other immediately post-accident witnesses (paragraphs 63 and 64), as showing that there was no noticeable ice after the accident, and we do not find ourselves assisted by evidence of ice found six hours later (paragraphs 60 - 62). The post-accident condition does not determine the matter, however, because of the possibility that there was ice and that it was dislodged by the shock and vibration of the accident or melted by the fire extinguishing powder (paragraph 75). If there was ice, we are satisfied that it occurred only on the outer areas of the wings.

132. We find that the aircraft accelerated up to an indicated air speed of 117 knots, or V_1. The evidence of Mr. Rodgers, the radio operator, as well as that of Captain Thain, leaves no doubt as to this. The time and distance taken to reach V_1 was longer than normal owing to the two factors of slush drag and of the measures taken to avoid and to correct boost surging. We cannot say whether the nose was raised at the normal speed of about 80 knots, or whether raising was deferred (paragraph 42), but we are satisfied that it was raised before V_1 was reached.

133. It is impossible to say how much runway had been used when V_1 was reached. Mr. Illingworth's curve (Appendix 4) shows a run of 4,900 feet to V_1; this assumes 8 mm water equivalent of slush and all wheels on the ground. Less slush, or the raising of the nose wheel, will lessen the distance: Professor Young's reconstruction puts it at 3,700 feet,

assuming 7.6 mm of water equivalent of slush and the nose wheel raised at 90 knots. The runway was 6,260 feet long and we think it clear that V_1 was reached somewhere between 3,000 and 4,000 feet.

134. We are satisfied that at or after reaching V_1 the aircraft's nose-wheel re-entered the slush. We say this because we accept the evidence of the witnesses who say they saw the nose descend somewhere after the half-way point and because, had it not happened, the aircraft must have flown off. We are equally satisfied that the descent of the nose-wheel was not the consequence of a voluntary act by Captain Rayment, but that it was caused by increased drag exerted through the main wheels, in other words by the aircraft entering a tract of deeper and/or denser slush. Apart from conscious rotation by the pilot, which we discard, there can be no other possible cause than deeper or denser slush. Once the nose re-enters the slush, the drag is enhanced by operating on six wheels instead of the four wheels of the main undercarriage.

135. This increase in the slush drag, occurring at the point when the aircraft should have commenced to lift off, is the crux of the matter and is in our view the prime cause of the accident. It is consistent with and supported by Captain Thain's evidence that at V_1 acceleration ceased and deceleration started.

136. We are satisfied that thereafter the aircraft ran with six wheels in the slush until towards the end of the runway it rotated to the point where the tail wheel made contact with the ground. We think that rotation in these circumstances would cause some compression of the tail wheel assembly and that the aircraft went to an incidence somewhere between the 8.3° at which the tail wheel touches the ground and the maximum of 9.5°, at which the tail assembly is fully compressed. What the exact incidence was we cannot determine, although we think it unlikely that the tail assembly reached full compression and we therefore think it unlikely that the full incidence of 9.5° was reached. It is impossible to say where on the runway rotation took place, save that it must have been in the last third of the runway and more than 300 feet from the end of the concrete. The important fact is that the aircraft was fully rotated but did not fly off. This means either that its speed had fallen below the minimum for take-off, or that some aerodynamic cause had inhibited lift-off despite the requisite speed and angle of incidence having been obtained. Because we cannot pinpoint the angle of incidence attained we cannot determine exactly what was the aircraft's minimum lift-off speed (without icing). This speed lay between $105\frac{1}{2}$ knots at 9.5° and 112 knots at 8.8° and because we think it unlikely that an angle as great as 9.5° was attained it follows that we think it unlikely that a minimum lift-off speed as low as $105\frac{1}{2}$ knots was attained.

137. We can be sure that the period between V_1 and rotation was one of deceleration, and we can be sure that at its commencement the speed was approximately 117 knots. We cannot reach such certainty as to the speed at the end of the period. Captain Thain speaks of the air speed indicator showing 105 knots before Captain Rayment's ejaculation, but he does not know when the aircraft rotated. The speed at rotation may have been 105 knots, or it may have been a little higher or a little lower. If at or below 105 knots, it was below absolute minimum take-off speed with a clean wing. If at or above 105½ knots it may have been in the narrow band between minimum take-off speed with a clean wing and minimum take-off speed with an iced wing at the particular attitude. There is no means of determining which of these alternatives is correct.

138. Once the aircraft had rotated without lifting off, the accident was inevitable. There was not enough runway left to attempt to regain sufficient speed. Thereafter the following occurred, though the sequence in which they occurred is not clear. The undercarriage was selected up; the brakes were applied for a distance of 265 feet and then released; the throttles were closed; the aircraft banked to starboard. These emergency actions occurred towards the runway end and on the grass slipway and played no part in causing the accident. Whether they affected its severity is problematical.

Causes of the Disaster

139. We have no doubt that slush decelerated Zulu Uniform. In the light of the expert evidence we find nothing unlikely in the view that by the time the aircraft could be rotated slush drag alone had reduced speed below the minimum take-off speed. We do not, of course, know with any precision the point on the runway where V_1 was attained or the point at which the aircraft was finally rotated. If the distance between the two points was 1,000 feet, a reduction in speed from 117 to 110 knots would be accounted for by the slush equivalent of 17 mm of water. If the distance were greater the depth of slush to be postulated would be less; if the distance less, the depth greater. 17 mm of water equivalent equals 3.4 cm of slush of specific gravity 0.5 or 2.3 cm of slush of specific gravity 0.75. In view of the lack of aquaplaning on the one hand and the evidence of the watery nature of the slush on the other, we think the specific gravity was higher than 0.5 and certainly not less than that figure (see paragraphs 111 - 112 above). We find nothing unlikely in there being 3.4 cm (1⅓ inches) or more of actual slush. No one knows whether or not the aircraft was then running down the centre of the runway, where the slush would have been traversed and displaced by previous aircraft landing and taxying to the perimeter path. In the last 400 feet Zulu Uniform had been well over to the left where there would be fewer tracks, and more slush deposited by being thrown aside by the wheels of previous aircraft. The fact that it was to the left may be significant because in this position the slight camber of the runway would, other things being

equal, involve a slightly greater slush depth, and a slightly greater drag, to the port main wheels than to the starboard main wheels and would tend to slew the aircraft to the left. Moreover the aircraft may have been to the left of centre ever since the port boost surging had been corrected by throttling back the port engine to 54 inches of boost. This would have produced slightly asymmetric power and tended to pull the aircraft to the left. But it is profitless to follow such speculation because we are convinced that other things were not equal: we feel satisfied that owing to the factors mentioned in paragraphs 94 and 95 above, the slush depth must have varied irregularly along the runway. No one can now say what the depth was along the aircraft's precise track, but we can say that the depth to be postulated in order to account for the reduction in the aircraft's speed is well within the likelihood of the situation.

140. The next question is whether wing icing contributed to the disaster. We have already said that we do not know whether the deposit on the wings adhered during the final run. On the hypothesis that it did adhere, the question is whether it contributed to the failure to unstick.

141. Accepting as we do the expert evidence of Professor Young, Professor Owen and Mr. Maskell, we accept that the effect of the degree of icing which they postulated would have been to reduce speed by not more than 1 knot and to increase minimum lift off speed by not more than 3 knots. These figures mean that if the aircraft, iced to the maximum degree they postulate, were travelling at, say, 105 knots and rotated to extreme incidence it can be said that had icing not been present it would have been travelling at 106 knots and that its minimum lift-off speed would have been 105½ knots instead of 108½ knots. In this hypothetical example icing would have been a contributory cause of the accident.

142. Here it is important to bear in mind the area of icing assumed for the purposes of the experts' calculations. Their assumptions differ from one another and are set out in the Table given in paragraph 121 above. But we think that if there was icing, its area was less than the least assumed. Having regard to the evidence of the eye-witnesses as to the amount of snow or slush lying on the wing at departure we do not think it can have exceeded one-third of the wing area. It follows that the effect on speed achieved and on minimum take-off speed of any ice which may possibly have been present was less than that produced by the expert witnesses' calculations. On the other hand one must allow a considerable margin of error in such theoretical calculations (paragraph 128 above). Bearing in mind these factors we feel satisfied that were ice present its effect would not have decreased speed by more than 1 knot or increased minimum lift-off speed by more than 2 knots.

143. This means that it is conceivable that when the aircraft rotated it would have taken off had it not been for icing. It may be that the minimum unstick speed was raised

fractionally by the increased download resulting from the amount of up elevator needed to rotate (as suggested by Professor Young: paragraph 106 above) but whatever the minimum take-off speed with a clean wing there would be a narrow band of speed above it before minimum take-off speed with partially iced wings was reached.

144. We have scrutinised the evidence relating to the aircraft's last rotation in order to see whether it indicates that the aircraft was all but airborne. If it was within the narrow band of speed mentioned above the greater part of its weight would be carried on the wings and the download on the wheels would be small. Zulu Uniform's main wheels left an impression on the grass slipway visible for many days, as were the scuff marks caused by braking on the concrete at the end of the runway. We find it impossible however to make any deduction from the marks save that there was at all times some download on the starboard main wheels. The like applies to the port wheels save where they were off the ground. They were off the ground because the aircraft was banking (paragraph 38 above). The aircraft had sufficient speed to enable it to bank but this does not mean that it had generated sufficient lift for take-off. We find it impossible to draw any useful deduction from the marks.

145. We are left, with this situation. There may have been ice on part of the upper surface of the wings. If present, the ice would have decreased actual speed by a maximum of 1 knot and would have increased minimum take-off speed by an amount which would not exceed 2 knots and may indeed have been no more than 1 knot. The aircraft may have attained both an angle of incidence and a speed at which that margin of speeds made all the difference between take-off and failure. Ice may have been a contributory cause of the accident. We do not know whether either of the conditioning factors (the presence of ice and the requisite speed at rotation) were present. That both were present we think unlikely.

146. If ice were present it might conceivably have caused the accident in another way. Its effect of decreasing speed by a maximum of 1 knot would mean that the aircraft would travel further before attaining V_1, 117 knots. Mr. Szukiewicz, assuming more ice than we think possible, estimated the increased length of run at 100 feet (paragraph 114 above). This means that up to 100 feet more of runway would have been available to the aircraft with clean wings than to the aircraft with iced wings, for the purpose of rotating and taking off, before reaching the slush condition which brought the nose down. Would this extra distance have made all the difference? At 117 knots the aircraft would traverse 100 feet in just over half a second. In that space of time the aircraft would have accelerated by a little over 1 knot and would not have reached V_2, 119 knots. We think it unlikely that an initiation of rotation earlier by one-half second would have avoided the entrapment of the aircraft by slush drag.

147. Our considered view, therefore, is that the cause of the accident was slush on the runway. Whether wing icing was also a cause we cannot say. It is possible but unlikely.

148. Before parting with the question of causation we think it important to say that our findings do not rest primarily on the evidence of Captain Thain. Since the subject matter of this Inquiry is whether he was to blame it is right to scrutinise his evidence with care and it is also fair to him to consider how far his evidence supports or differs from that of other witnesses.

149. We think that Captain Thain's evidence is reliable. Having seen him at two Inquiries we have no hesitation whatever in accepting that he was relating the truth as he recollected it. But the human recollection is a fallible recording device and peculiarly susceptible to subjective influence. Captain Thain underwent both the physical trauma of the accident and the psychological trauma of its effect upon his life. We have borne these matters in mind. We have also borne in mind the fact that his written statement, quoted in paragraph 26 above, was made on 6th March 1958, only one month after the disaster, and that where his evidence can be checked against other evidence, it is confirmed. We think his account is acceptable.

150. But Captain Thain's evidence is not crucial to our findings as to causation. He had no recollection of changes in angle of incidence, and our important finding that the nose came down during the run rests primarily on the eye-witnesses' evidence. His principal contribution to the story is his account of changes in indicated air speed, but none of our findings rests upon this evidence alone. That the aircraft reached V_1 we know from Mr. Rodgers, and that it decelerated when entering slush sufficient to bring the nose down is demonstrated by the expert evidence. His evidence does not help on the unsolved question of speed at time of final rotation because he does not know when rotation took place. The value of his evidence is that it is consistent with the other evidence. (The only apparent departure from consistency is that Mr. Rodgers cannot remember Captain Rayment's ejaculation "we won't make it"; but no one familiar with the effects of shock on participants in a catastrophe need be surprised that an incident impressing itself on one human mind is not retained by another). We think the consistency of Captain Thain's evidence with the other evidence both supports that other evidence and gives weight to his own.

PART VIII THE COMMAND SITUATION AND PILOT BEHAVIOUR

151. The command situation in Zulu Uniform at the material time was unusual. Captain Thain was in command, and Captain Rayment was acting as co-pilot. Ordinarily a First Officer is co-pilot, but the exigencies of service sometimes require two captains to be rostered together. Where this happens one would expect the senior Captain to be in command: indeed in the course of evidence to us Captain Baillie, the General Manager, Flight Operations, of BEA, described this as "the invariable rule". Nevertheless Captain Thain was junior to Captain Rayment and on previous occasions when the two had flown together, Captain Rayment had been in command. On this occasion Captain Thain had flown the aircraft out to Belgrade and the two captains had agreed that Captain Rayment should fly it back to London. He had flown it from Belgrade to Munich and was flying it at the time of the disaster. On this return journey he was occupying the left hand seat.

152. The left hand seat is the Captain's seat. There is ordinarily no reason why a co-pilot should not fly the aircraft, but his occupying the left hand seat was a breach of a BEA rule. Their Flying Staff Standing Instructions provided "When two Captains fly together for any reason whatsoever the Captain in command will always occupy the left hand seat". Captain Baillie told us that this rule was introduced in 1956 in consequence of the Report of the Court Investigation into the accident in which a BEA Dakota crashed at Mill Hill in 1950. In that accident the First Officer co-pilot had been in the Captain's or left hand seat, and the Court recommended that study should be given to establishing a code "so drafted and so enforced as to eliminate any possibility of uncertainty as to who is to take executive decisions in an emergency" (MCAP 93, paragraph 52).

153. Thus Zulu Uniform was being flown by the Captain senior in rank but on this occasion junior in status, and the Captain in command was occupying the co-pilot's seat. There have been suggestions that this state of affairs opened the way for confusion or misunderstanding (see the first German Report, p.33, and the second German Report, p.20) and it is clearly necessary for us to consider whether it played a causative part in the accident.

Why Captain Thain was in Command

154. It has not been easy to ascertain why Captain Rayment was not in command. At our 1960 Inquiry Captain Thain thought that the First Officer rostered for the flight had not got a Yugoslav visa and that Captain Rayment, who had such a visa, took his place. At the present Inquiry BEA advanced the view that Captain Rayment was being rehabilitated after a period of sickness and that such rehabilitation was to take the form of one flight as supernumerary (previously accomplished) followed by this flight as co-pilot. This view was based on human recollection, unaided by contemporary documents (which were then thought not to be available after the lapse of time), and turned out to be completely erroneous. It led us to investigate the state of Captain Rayment's health and training, and further research eventually produced evidence from which we can with confidence reconstruct his history prior to the accident.

155. Captain Rayment was operated on for a bilateral inguinal hernia on 17th November 1957. His last flight prior to the operation had been on the 12th November 1957. After the operation he experienced a small pulmonary embolism. He was discharged on 14th December 1957 and on 9th January 1958 he was passed fit for flying duties by Dr. F. S. Preston, BEA's regional Medical Officer. By reg. 195 of the Air Navigation (General) Regulations, 1954, the holder of an air transport pilot's licence who had been incapacitated by illness for 20 days or more was required to notify the Ministry of Transport and Civil Aviation and to submit if required to an official medical examination. Captain Rayment's licence had in fact expired on 12th November 1957. He duly applied for renewal, was medically examined on 16th January 1958, and on the same date his licence was renewed.

156. So far as the law was concerned he could then resume his flying duties. BEA's Flying Staff Standing Instructions, however, included the following rules concerning rehabilitation after sick leave:

> "After an absence of more than one month but less than three months all pilots must carry out at least one supernumerary flight before again operating in the capacity for which they were certified. During the course of such flight they will re-familiarise themselves with their respective duties.
>
> "After an absence of more than three months from flying duties Captains and First/Second Officers must undergo suitable flying and technical refresher training before again operating in the capacity for which they were certified."

Captain Rayment had, of course, been away more than one month but less than three months. It is as to his rehabilitation that some confusion arose, but a document subsequently found indicates that he took a survival course on

21st January 1958 and flew to Hanover as a supernumerary pilot on 22nd January. It has also now been ascertained that he flew Elizabethan aircraft to Jersey and back on 29th January and to Brussels on 31st January and back on 1st February. On these two round trips he was Captain in command. In verification of these facts we have inspected the log books of the two officers who were co-pilots on these flights. (Captain Rayment's log book cannot now be found). It is now quite clear that Captain Rayment was regarded by BEA as fully rehabilitated by 29th January 1958, when they rostered him in command to Jersey, and that the flight in Zulu Uniform was no part of any rehabilitation programme. How, then, did he, the senior Captain of the two, come to be rostered as co-pilot on the flight to Belgrade?

157. The history of the pilot selection appears to be as follows. When the Belgrade flight was planned Captain Rayment was selected as Captain. When he went sick Captain Thain was substituted. When Captain Rayment returned he expressed a wish to get back on the flight but was told that Captain Thain could not now be displaced. Meanwhile another pilot, First Officer Hughes, had been rostered as co-pilot. He, however, was delayed on the Continent by weather conditions and although returning to London Airport on 1st February could not have had his appropriate time off before the Belgrade flight was due to depart on 2nd February. A second First Officer had been rostered as stand-by co-pilot but his services were not called upon and Captain Rayment went instead, no doubt because of his expressed wish to do so. We were informed that where a Captain comes in as a last-minute substitute for a co-pilot it is the practice for him to be the co-pilot notwithstanding that he may be senior to the Captain. Thus it came about that Captain Thain had under his command a co-pilot who was senior to him as a captain and had greater experience of flying the Ambassador.

158. We accept the medical evidence that Captain Rayment was fit to return to full flying duties and we are satisfied that he was exceptionally well qualified to fly Zulu Uniform. He had flown 8,463 hours, had converted to Ambassadors in March 1953 and since then flown 3,143 hours in Ambassadors. For comparison Captain Thain had converted to Ambassadors in March 1955 and flown 1722 hours in that type. The evidence has convinced us that Captain Rayment was regarded both by airline officials and by his fellow pilots as an outstanding pilot. Captain Thain informed us that on the outward flight to Belgrade instrument approaches had been necessary at Manchester, Munich and Belgrade and, following the BEA "monitored approach" system, the instrument approach had been conducted by Captain Rayment from the right hand seat with great precision. Captain Thain added that Captain Rayment had flown "exceedingly well" on the return from Belgrade.

159. Captain Thain's decision to allow Captain Rayment to occupy the left hand seat was wrong, in that it was a breach of an explicit rule of the airline. It was suggested to us that

this rule was sometimes honoured in the breach but we have no evidence of this. Both captains should have known the reason for the rule and should have adhered to it. Having said that, however, one must add that a Captain would clearly be happier flying from the seat to which he was accustomed. On the fatal take-off run Captain Rayment was in the position in which he had no doubt made many hundreds of take-off runs since being promoted Captain. The breach of the rule cannot have decreased his efficiency: on the contrary it must have increased it. The danger, which the rule was designed to prevent, was that a Captain in the Captain's seat might in an emergency think of himself as in command and act accordingly and conversely that a Captain in charge who was in the co-pilot's seat might hesitate to exert his authority where it needed to be exercised. In performing our task of deciding whether Captain Thain was to blame for the accident we have to consider whether his undoubted fault in putting his co-pilot in the left hand seat had any bearing upon what happened. We must therefore consider in detail what took place on the flight deck during the attempts to take-off from Munich.

Events on the Flight Deck

160. Captain Thain's account is given at Paragraph 26 above, and the evidence of the radio operator, Mr. G. W. Rodgers, at paragraph 28 above. According to Captain Thain the decision to break off the first run because of boost fluctuation was taken by Captain Rayment and the decision to break off the second for the same reason by Captain Thain (paragraph 16 above). It has been suggested that the pilots were at fault in not at once realising that the boost fluctuation was due to the altitude and should have corrected it without aborting take-off. We do not agree with this suggestion, and in any event, while the two abortive runs are part of the history, they are not the main subject of our Inquiry. When the aircraft returned to the tarmac Mr. Rodgers noticed that one of Captain Thain's hands was bleeding from a nick in the skin. Captain Thain said this had been caused on the first abandonment: when Captain Rayment pulled the throttles back Captain Thain's hand was behind them and got caught on the console. We mention this incident because the suggestion has been made that this slight injury was caused during the final stage of the fatal run and that it pointed to conflict between the pilots, Captain Thain trying to keep the throttles forward when Captain Rayment was trying to close them. Mr. Rodgers' evidence made it clear that this suggestion is without foundation.

161. The evidence as to the final run is basically Captain Thain's statement, supplemented by his evidence and that of Mr. Rodgers. In addition inference as to pilot behaviour can be drawn from the external evidence detailed in Part III above. The run can be divided into three stages, first commencement to attaining V_1, second thence to Captain Rayment's ejaculation "Christ, we won't make it", third thereafter.

The Run: Stage I

162. In the first stage boost surging was encountered and corrected. According to Captain Thain it commenced at 85 knots and ceased at 105 knots. According to the Peravia recorder it commenced at 25 seconds from start and ceased at 34 seconds. These two sets of figures correspond satisfactorily. Correcting boost surge caused the aircraft to accelerate a little more slowly, and to use a little more runway, than otherwise. The magnitude of these factors can be assessed by comparison with the first run, when the throttles were opened normally. The Peravia recorder showed that on that occasion throttles were closed for break off after about 32 seconds, and Captain Thain stated that 105 knots had been reached. So, on the final run the aircraft took 2 seconds longer to reach 105 knots. From this it can be calculated that it used about 300 feet more runway to reach that speed.

163. During the first stage the nose wheel should have been raised. The operations manual provided that "the nose wheel should be eased off the ground at about 80 knots". Captain Thain has no recollection of attitude changes, and we have indicated in Part III above the difficulty of interpreting the external evidence as to when this raising took place but we think it clear that it was raised by the time the aircraft approached its V_1 of 117 knots.

164. Mr. Rodgers confirmed Captain Thain's account of dealing with boost fluctuation and calling V_1. The picture presented by the evidence is that of two pilots co-operating normally and satisfactorily and dealing adequately at this stage with the only perceptible abnormality, the fluctuation. The acceleration to V_1 would have taken longer than normal because of both slush drag and coping with fluctuation. This crew did not, as some pilots do, check time to V_1 with a stopwatch, but if they had sensed (Captain Thain has no recollection of this) that the time was longer than normal they could in the circumstances attribute it to the throttle manipulation, which they were aware of, and would not be alerted to the possibility of excessive slush drag. Captain Thain pointed out that in the snowy conditions there was no visual point of reference from which the length of runway used on this stage could be evaluated.

The Run: Stage 2

165. We have given our reasons in paragraph 41 above for finding that the nose-wheel came down at or after the half-way point along the runway. We are satisfied that Captain Rayment would not have deliberately put the nose down. He should have been doing the opposite: the manual states "Commence to unstick when within 5 knots of the safety speed (V_2)." This change of attitude must have happened at or immediately after the time V_1 was called. Ordinarily V_2, only two knots higher, would be attained in one second, i.e., by the time

that V_1 had been called. At this stage Captain Rayment must have appreciated both the altered attitude and the failure of further acceleration. There would be no visual indication at this stage that he would be likely to run out of runway and his duty, once the point at which V_1 was called had passed, was to maintain full throttle and get the nose up. We know that he maintained full throttle and we have no doubt that he tried to lift the nose. Captain Thain stated that he saw him "adjusting the trim".

166. Meanwhile the runway is slipping away and Captain Rayment appreciates that there is an emergency. His ejaculation "Christ, we won't make it" indicates that he had till then been trying to "make it" and must have come at about the time when the nose was lifted. There was no direct evidence whether he was then aware how his speed had decayed, but after non-receipt of the expected V_2 call we would have expected him to look at his Air Speed Indicator. The fact that the aircraft's course had got well to left of centre of the runway by the end of the concrete may indicate that Captain Rayment was not giving undivided attention to visual reference at this stage.

167. During this stage Captain Thain gave no orders and passed no information. He was conscious of lack of acceleration and saw the Air Speed Indicator hover at 117 knots, drop 4 or 5 knots, and drop again to about 105 knots. His reaction, he said, was "complete and utter surprise". Then he thought there might have been an instrument failure. He did not look out to see if their position on the runway could be assessed. He estimated that some six seconds elapsed between calling V_1 and Captain Rayment's cry.

<u>The Run: Stage 3</u>

168. At or after - probably after - his cry, Captain Rayment got the nose up and the aircraft went to extreme attitude, as shown by the tail wheel mark (paragraph 31 above). At this time the throttles were at full power, as Captain Thain ascertained by banging the levers. Immediately thereafter Captain Rayment ordered "undercarriage up" and Captain Thain operated the override and selected "up". There is no doubt that he did this: see paragraph 37 above. We have indicated in that paragraph the difficulty of deciding whether retracting the undercarriage was, as we originally thought, part of an attempt to decelerate, or was part of an attempt to become airborne. At and after the end of the runway the aircraft banked to the right, we think to avert a head-on collision with the house ahead.

169. At some stage in the sequence the wheels were locked for approximately 265 feet. This must have been caused by braking and as Captain Thain did not apply the brakes Captain Rayment must have done so. At about the same time power was cut off, and again this must have been by Captain Rayment. We think it impossible to ascertain the exact sequence of these events. The absence of reasonable

certainty in this regard, however, does not affect our task because by now the accident was inevitable: its causes lay further back in time and distance, and if blame for the accident is to be attributed to an individual it must be blame in respect of earlier actions. The relevance of events in this third stage is in the light it sheds upon the relationship of the two pilots and whether there was confusion between them as to their respective functions.

Examination of Pilot Behaviour

170. We can find no material for criticism in Captain Rayment's actions. It was suggested to us that after failure to reach V_2 he might have been hesitating, awaiting a decision from his colleague, who was in turn expecting him to take the decisions. We quote from the evidence of Captain Baillie:

> "A. On the third take-off it could very well be that one Captain was saying 'If the other one is satisfied, I suppose it is all right.'
>
> Q. You mean Captain Rayment might have broken off but for the fact that he was expecting Captain Thain to take that decision? A. And vice versa.
>
> Q. Each might expect the other to decide to break off, not because of boost surging this time, but because of running out of runway? A. Yes.
>
> Q. Once that point had been passed, V_1 had been passed, and no decision to break off had been taken, was it the duty of the pilot flying the aircraft to try and fly it off? A. Yes.
>
> Q. In the evidence you have heard is there any indication that Captain Rayment did not take the correct steps to fly off the aircraft? A. No sir. I think once the deceleration was experienced, having passed V_1 they were in a very difficult situation indeed and I will not even begin to attempt to criticise either of them for any action after that stage, because they were in a situation beyond which there was nothing they could sensibly do."

171. It is clear that Captain Thain did leave the decisions to Captain Rayment. We do not think this can be criticised. A thoroughly competent man was flying the aircraft: in a situation where seconds were important it was better to let him carry on than to take over or to issue orders which might possibly confuse and would lose precious time. It may well be that Captain Thain would have behaved differently if he had had a First Officer flying the aircraft, but any inhibition upon asserting his authority as Captain in command would be due rather to his being rostered with a more experienced and senior colleage than to the fact that that colleague was in the left hand seat.

172. Moreover we do not think that events would have been different if, immediately upon it becoming apparent that V_2 was not being normally attained, Captain Thain had taken over the controls, or had commanded an immediate fly-off. Having passed V_1, the pilots were committed to take-off and could not reasonably be expected to abandon it. There is no reason why Captain Thain or any other pilot would have rotated the aircraft into take-off attitude any more quickly than Captain Rayment did. See the reconstructions by Professor Young and Mr. Szukiewicz at paragraphs 105 - 107 and 117 above.

173. The main allegation of confusion between the pilots derives from the braking of the aircraft wheels at a time when the throttles may have been fully open; it is set out in the passage from the first German Report reproduced in paragraph 23 of our 1960 Report. If Captain Rayment braked for deceleration he would earlier or simultaneously have closed the throttles or called on his co-pilot to do so. At some stage he did close the throttles. There is no evidence that his braking or his closing of the throttles coincided with Captain Thain's banging the throttles and finding them fully forward. If there had been some kind of physical contest between the pilots over the throttle levers it is inconceivable that no word would have been spoken, and Mr. Rodgers does not recollect hearing anything. We have already disposed of the suggestion that the scratch to Captain Thain's hand was sustained at this time (paragraph 160 above).

174. Our terms of reference do not directly require us to evaluate Captain Rayment's conduct. But having had to examine his behaviour because of its interaction with Captain Thain's conduct, it would be wrong for us not to express the view we have formed. It is that Captain Rayment's piloting of the aircraft was competent and efficient throughout.

175. We have said that Captain Thain was in breach of his employer's rule that he should occupy the left hand seat. We do not wish it to be thought that we regard this breach lightly: it was a clear rule imposed for a valid reason. But we have to ascertain whether this wrong action by Captain Thain played any part in causing the accident. We think it did not. We find no evidence of confusion or misunderstanding between the pilots and we think Captain Thain acted properly in allowing Captain Rayment to cope with the emergency without any interference from himself. We think that had the seating been reversed and Captain Rayment had been flying the aircraft the only difference would have been that Captain Rayment would have been less "at home" with his controls and might conceivably have reacted less swiftly than he did. Had Captain Thain himself been flying the aircraft, from the left hand seat, there is no reason whatever for thinking that he would have acted differently from Captain Rayment, and none for thinking that this would have averted the accident.

176. There is one aspect of Captain Thain's behaviour, however, unconnected with breach of the seating rule, which has caused us some concern. This is his failure to pass to Captain Rayment information about decaying speed. As co-pilot, monitoring the instruments, he should have called attention to the unusual cessation of acceleration and onset of deceleration. We have said (paragraphs 165 and 166 above) that Captain Rayment must have appreciated from the failure to call V_2 that acceleration had ceased and probably thereafter kept an eye on the air speed indicator. But he should have been relieved of this distraction. It is possible that had the declining speed been called he would have had a fractionally earlier appreciation of the nature of the emergency and possible that he would have got the nose up earlier, perhaps calling for Captain Thain's assistance in overcoming control column resistance.

177. We think Captain Thain's failure to act is explicable by his previous pre-occupation with boost surging. Having prepared himself to deal with one kind of abnormal aircraft behaviour he was the less able to react to another and unexpected abnormality; hence the time he took to overcome his "complete and utter surprise". Nevertheless he should have reacted more quickly. We cannot, however, find affirmatively that this fact renders him to blame for the accident. It is possible that had the nose come up earlier the aircraft could have become airborne; but on the whole we think the probabilities are against attainment of the requisite angle of incidence before speed dropped below the minimum for take-off. We find therefore that no causal connection is proved between this lack of action and the disaster.

Part IX THE QUESTION OF BLAME

178. We are required to report whether in our opinion blame for the accident is to be imputed to Captain Thain. Blame implies fault plus causation. In the material respects we have to consider whether Captain Thain was at fault and if so whether that fault caused or contributed to the accident. In Part VIII we have examined his behaviour as commander in permitting Captain Rayment to fly the aircraft and during the final run. We have found fault in his breach of the BEA seating rule, and we have criticised his inaction when monitoring the Air Speed Indicator. We find affirmatively that the former did not adversely affect the flying of the aircraft and had no connection with the accident. We find that no causal connection is established between the latter and the accident. In these respects therefore no blame attaches to Captain Thain for the accident.

179. There remain the questions of wing icing and runway slush. Whether Captain Thain was at fault in these respects formed two of the three specific questions the subject of our 1960 Inquiry (paragraph 7 above). We then found him to be at fault in regard to icing and not at fault as regards slush. We dealt at length with the steps taken by Captain Thain as regards wing icing in paragraphs 62 - 70 of our 1960 Report, and discussed them in paragraphs 94 and 95. We similarly dealt with his actions as regards the runway in paragraphs 76, 89 and 90 and examined them in paragraph 96. At our present Inquiry no further evidence was given as regards Captain Thain's consideration of runway slush and no suggestion was made that our previous findings needed re-examination. On the other hand his action as to wing icing was re-considered in evidence and Mr. May submitted that we should revise our view in this matter.

Responsibility for Wing Icing

180. We can summarise the relevant evidence as follows.

(1) Article 17(2) of the Air Navigation Order 1954, (in force at the date of the accident) provided: "Before the aircraft flies or attempts to fly the person in command of the aircraft shall satisfy himself ... (vi) ... that the wings and control surfaces are free from ice and hoar frost."

Paragraph 7.33.2.2 of vol. 5 of BEA's Flying Staff Standing Instructions required that Captains should make absolutely certain immediately before take-off that the lift and control surfaces of their aircraft were clear of snow. A Ministry circular (No. 150/1954), *The Effect of Frost Ice and Snow on Aircraft Performance, Precautions before Take-off* reminded those concerned that snow "will also be liable to freeze to the surface if the temperature has fallen from just above freezing point during the snow. It is, therefore, never safe to assume that snow, though apparently of the dry variety, will be blown off during take-off. Snow should therefore be completely removed before taxi-ing out to take-off, and a careful inspection made to ensure that no underlying deposit of ice remains. Such underlying deposits will necessitate the use of de-icing fluid. Particular care is necessary when the temperature is in the neighbourhood of freezing point and delay occurs between the removal of the snow and take-off. It is important in such conditions to remove any fresh snow which has fallen since the previous removal." Captain Thain was familiar with these provisions.

(2) Before the first departure from the apron at 1519 hours Captain Thain from his own observations and in conjunction with Captain Rayment and the Station Engineer, Mr. Black, assessed the situation and decided not to have the wings swept or de-iced (1960 Report, paragraphs 64, 65).

(3) Before the second and last departure at 1556 hours Captain Thain's only additional action was to study the small portion of the wing surface which he could see from the flight deck and to discuss what he saw with Captain Rayment; his evidence is summarised at paragraph 52 above.

(4) The information upon which Captain Thain acted was (i) his observation from the ground of water running from the wing and of the wings bearing "a thin film of partially melted snow"; (ii) information from Mr. Black, who had found the wings clean when he had been upon them for re-fuelling up to about 1440 hours; (iii) information from Captain Rayment; who told him that he had looked at the wings and that in his opinion they did not need sweeping; (iv) his observation of the snowfall, which he said had virtually ceased by the time of the second departure; and (v) his opinion that the temperature was above zero. His evidence on temperature is given at paragraphs 67 and 68 of the 1960 Report. He had assessed the temperature by observation and experience and could not recall having ascertained the ground temperature although he had visited the met. office.

181. The screen temperature had been zero at 1500 hours and -0.2°C at 1600 hours. The aircraft had landed at 1417 hours, cleared to taxi on the first departure at 1519 hours, returned to the apron at 1539 hours and made its final departure at 1556 hours. It had been standing and taxying in falling snow and had made two abortive runs in slush.

We thought in 1960 that in these circumstances there was a clear duty on the captain to make sure that the wings were free of both ice and snow and that he had not done so.

182. At the present Inquiry Captain Kelly, who had been an Ambassador pilot when with BEA (paragraph 37 above), expressed the view that he would not himself have had the wings swept or de-iced. He thought that if the captain were satisfied at the time of the first departure, no further investigation of the situation was needed in the circumstances before the second departure. He would know from the temperature gauge in the cockpit that there had been no sudden fall in temperature. He said that if the gauge "was still at the same temperature as it was in the first place, and there was no obvious change of circumstances outside, there was no reason to reverse the decision made 40 minutes earlier." Freezing at or fractionally below zero would be an extremely slow process.

183. The air temperature gauge is not regarded as a particularly accurate instrument. Indeed Captain Thain did not rely on it: he was asked (in 1960) "Were you in a position inside the cockpit to check the outside temperature?" and answered "Not accurately, no." Captain Kelly did not support Captain Thain's action in assessing temperature by observation of the melting snow, but we understood him to be saying that if Captain Thain had had reasonably accurate temperature information it should have made no difference to his action.

184. We are unable to accept this as an accurate interpretation of the duty of a captain in command either under the Air Navigation Orders and BEA's rules, or in common sense. Once the temperature approaches zero a captain must assume that it may drop below zero. Indeed he ought to assume that it may already be below zero at parts of the taxiway and runway: local variation in temperature of several tenths of a degree are not uncommon. We remain of the view that Captain Thain ought to have appreciated how critical was the prevailing air temperature and ought to have made, or caused to be made, an inspection of the whole of his wing surfaces followed by any necessary action to deal with what was found. At the end of the day we cannot say whether ice would have been found nor whether the deposit which we are satisfied would have been found would have frozen to the wing during taxying and the take-off run. These uncertainties emphasise the imperative nature of the duty to inspect and take action. These are safety precautions and, as with all safety precautions, a margin for error must be allowed.

185. Fault therefore we reaffirm. Was there blame? We have said that ice may possibly have contributed to the accident but that we think it unlikely to have done so. In these circumstances we cannot and do not find blame. It would of course be wrong to assert that blame exists unless its existence is proved. Our finding that icing was unlikely to have been a contributory cause of the accident means that as

a consequence we find that the fault with regard to inspection is unlikely to have been a cause of the accident. We therefore do not find that blame in this respect for the accident is to be imputed to Captain Thain.

Responsibility for Runway Slush

186. Captain Thain had not noticed any retardation during the landing by Captain Rayment, nor had it occurred to him that the slush on the runway presented any problem. He did not think it would retard the aircraft to any large extent. He accepted that his responsibility for the safety of his aircraft involved considering, in appropriate circumstances, whether the runway surface was safe for take-off. He had never himself, so far as he could recollect, made a personal inspection of a runway on foot or in a vehicle, but he knew that pilots had done this on isolated occasions. Before he took such a step, he said, he "would have to be awfully concerned about the state of the runway ... for this reason, that there must be a group of airport staff whose job it is to service and look after the airfield ... one expects it to be up to a certain standard."

187. Captain Thain did in fact address his mind to one aspect of taking off in slush. This was the question whether, if throttling back to deal with boost surging produced a swing, it would be corrected by steering the nosewheel without slipping. Should he have thought of excessive retardation, or uneven retardation? We must bear in mind the date of the accident. In 1958 no experimental investigation of the effect of slush drag had been conducted and the theoretical calculations of drag then accepted were found in the light of subsequently gathered experimental data to be incorrect. Our present extensive knowledge of these matters is dervied almost wholly from subsequent tests such as those referred to in paragraphs 108 - 111 above - tests partly occasioned by this accident. But in 1958 there was no general appreciation by pilots of the magnitude of the drag due to slush of the depths with which we are concerned in this case.

188. Captain Thain was entitled to have regard to the fact that the runway was being used without comment by other aircraft and appeared to be regarded as safe by the airport authorities. Captain Wright, who told us in 1960 that he had sometimes inspected runways before take-off, did not think inspection necessary at Munich at 1720 hours, although he had experienced retardation on landing at 1555 hours (paragraph 90 above). Captain Thain faced a longer take-off run than Captain Wright with his lightly loaded Viscount but, as the former said, Munich was not a marginal airport. We reaffirm the view we reached in 1960 that Captain Thain would have had no reason to suspect that he had not enough runway unless he had possessed a knowledge of the drag effects of slush which was not then available. We do not think he was at fault in failing to investigate the runway personally or in failing in any other way to recognise and deal with the problem which we now know was presented by the presence of slush on the runway.

Part X CONCLUSION

189. We can summarise our conclusions as follows:

(1) The cause of the accident was slush on the runway *(paragraphs 135, 147)*.

(2) It is possible but unlikely that wing icing was a contributory cause *(paragraph 147)*.

(3) Captain Thain was not at fault with regard to runway slush *(paragraph 188)*.

(4) Captain Thain was at fault with regard to wing icing *(paragraphs 184, 185)*, but because wing icing is unlikely to have been a contributory cause of the accident, blame for the accident cannot in this respect be imputed to him *(paragraph 185)*.

(5) Captain Thain was at fault in permitting Captain Rayment to occupy the Captain's seat, but this played no part in causing the accident *(paragraphs 175, 178)*.

190. In accordance with our terms of reference we therefore report that in our opinion blame for the accident is not to be imputed to Captain Thain.

191. The burden of re-examining in detail the circumstances of an accident after a lapse of ten years has been lightened by the able and energetic assistance of all concerned. We owe a particular debt of gratitude to our Secretary, Mr. S. Wignall, whose assistance has been invaluable and whose management of our sittings (including a visit to Germany arranged at short notice) has been excellent. We are indebted to the Treasury Solicitor and his officers for the smooth marshalling of the evidence. Our hearings were facilitated not only by the admirable arrangements made for accommodation but also by the services of the shorthand writers and by the provision of simultaneous translation to and from the German language and we warmly thank those who thus assisted us.

S. WIGNALL *Secretary*

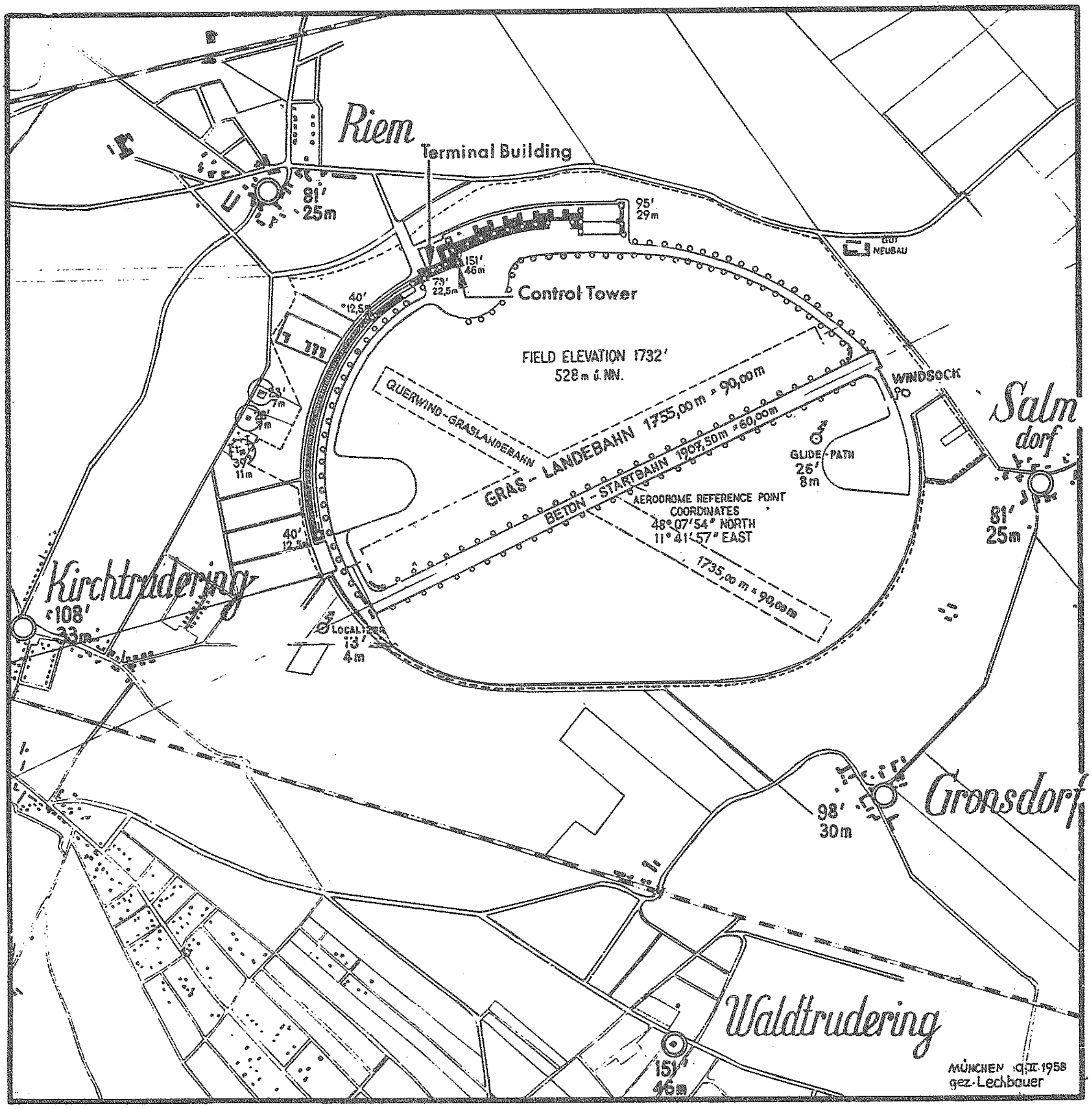
18th March 1969

E. S. FAY
A. R. COLLAR
J. R. JEFFREY

APPENDIX 1

PLAN OF MUNICH AIRPORT
February 1958

MUNICH - RIEM



1 NAUTICAL MILE

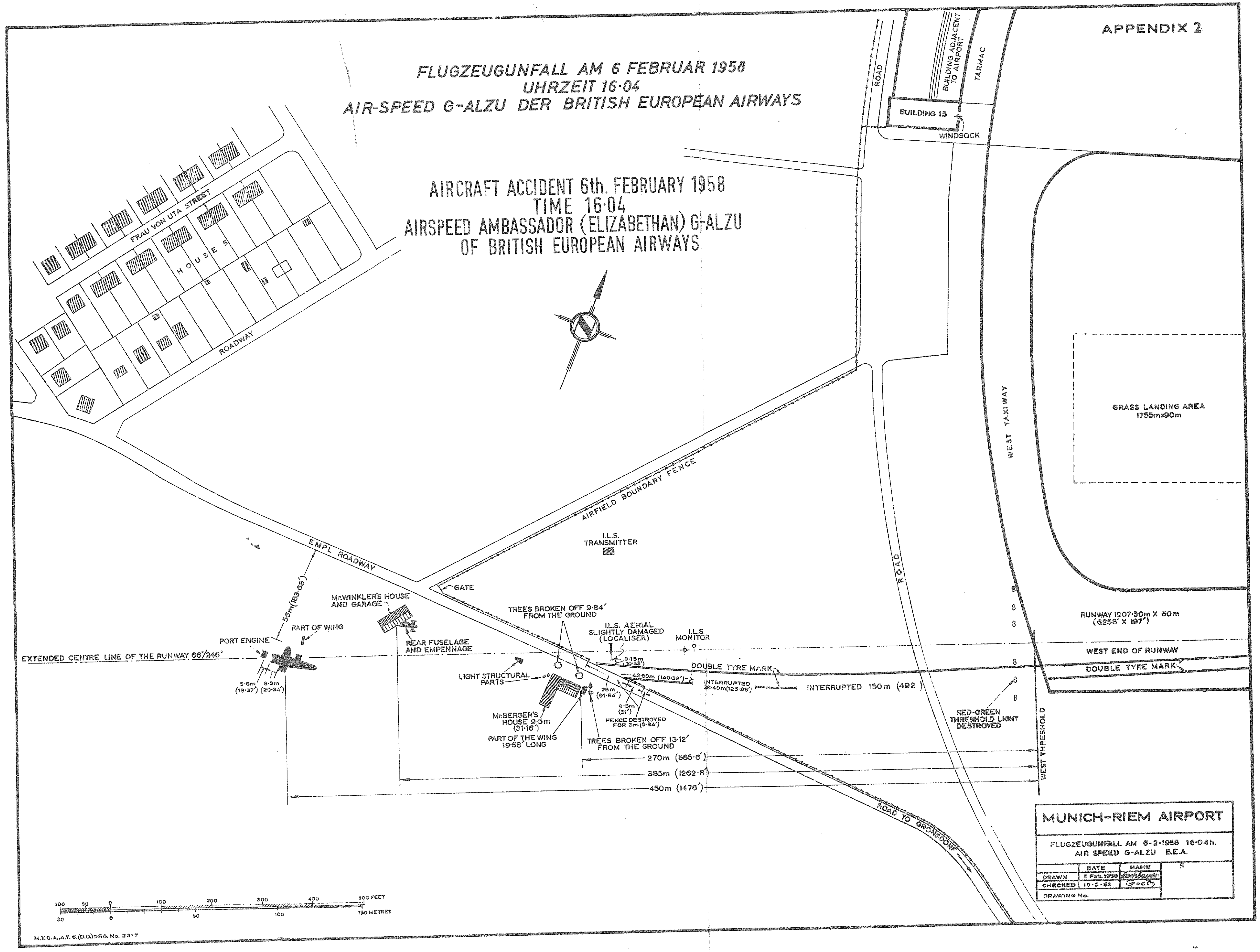
1 KILOMETER

SCALE 1:20000

LOCAL MAGN. VAR. JAN 1953 2° 54' WEST

- ◎ OBSTRUCTION, ILLUMINATED
- ○ OBSTRUCTION, NOT ILLUMINATED
- ∘ TAXIWAY - LIGHTS OUTSIDE-RIM = BLUE
- ∘ TAXIWAY - LIGHTS INSIDE-RIM = YELLOW
- ▬ OBSTRUCTION LIGHTS ON AIRPORT-BUILDINGS
- WINDSOCK WITH OBSTRUCTION LIGHT

APPENDIX 2



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APPENDIX 3

Statement by Professor P. R. Owen

1. My present appointment is Zaharoff Professor of Aviation at Imperial College in the University of London. My special field of interest is aerodynamics.

2. During the latter part of 1962 and beginning of 1963, I acted as aerodynamics expert for James Chapman & Co, Manchester, solicitors for the Manchester United Football Club in their proposed action against BEA.

3. The conclusions I formed about the accident at Munich were reached in two stages. The first was based on a study of the Report of the German Enquiry (HMSO, CAP 153) and of that of the British Reviewing Body (HMSO, CAP 167). The second was based on a study of the Report by Schlichting and Gersten, which came into my hands only at a late stage in my own investigation.

4. Conclusions: <u>first stage</u>. (Report to James Chapman & Co dated 2.11.62).

   (a) The action of slush in resisting the passage of the aircraft along the runway must be accepted.

   (b) In that case, icing must be rejected as the sole cause of the accident.

   (c) It remains to examine whether icing is admissible as a contributory cause.

   Among the points considered under (c) was that concerning evaporative cooling to which the German Enquiry attached importance. I made detailed calculations of the effect and found it to be negligibly small.

   The main arguments supporting the retention of (c) as a serious exercise arose from Professor Collar's remarkable reconstruction of the accident given in the British Reviewing Body's Report. The reconstruction which was based on the Schlichting and Gersten aerodynamic calculations possessed two features supporting the argument that icing could have been a contributory cause. They were:

(i) that the drag increment due to the ice so prolonged the take-off as to expose the aeroplane to the region of increased slush thickness or density,

(ii) that, having reached a speed of 117 knots, and if at that speed Capt. Rayment had, as the German Enquiry suggested, attempted to lift off the wings were so impaired by the ice that they could not develop sufficient lift to support the weight of the aeroplane.

It therefore appeared that an investigation of the calculations made by Gersten and Schlichting with regard to the effect of ice deposits on drag and lift was crucial.

5. Conclusions: <u>second stage</u>.

The report by Gersten and Schlichting was made available to me around mid-December 1962.

A number of features of that Report caused me concern. They were as follows:

(i) The thickness of the ice-layer was taken to be 5 mm whereas the total precipitation could have led only to a thickness of 2 mm.

(ii) The entire upper surface of the wing was assumed to be coated with ice, making no allowance for the observations of witnesses that the inboard portions as well as the leading edge of the outboard portions were free from ice.

(iii) Most significant of all, I could not agree with the Gersten-Schlichting estimates of the increments in profile drag for a wing whose upper surface is entirely covered by ice. My own calculations gave values only about one-third or one-quarter as large.

6. Independent aerodynamic calculations.

(a) <u>Profile drag increment</u>. Comparison with the experimental data given by Jones and Williams in R&M 1708 supports the Gersten-Schlichting argument that the ratio of the drag of a roughened aerofoil to that of the smooth aerofoil is approximately in proportion to the corresponding drags of a flat plate. I assumed that the same rule would apply when only one surface of the aerofoil and one surface of the flat plate were roughened.

(b) <u>Decrease in lift curve slope</u>. The Hoerner data given in fig.9 of the Gersten-Schlichting Report provide an overall guide to the effect of an increase in profile drag on lift curve slope but, appealing to the data of Abbott and von Doenhoff in their book *"Theory of Wing Sections"*, it appears that significant departures in the behaviour of individual aerofoils from that predicted by Hoerner's curve can occur.

As an alternative, I analysed the data of Abbott and von Doenhoff, which were obtained by roughening the first 5% - 10% of the upper and lower surfaces of an aerofoil, the roughness corresponding to a height of about 1.5 mm on the Elizabethan. The method I adopted was to read off from their experimental curves the fall in lift coefficient due to roughness that occurs when the lift coefficient of the smooth aerofoil with flaps up is unity. I concentrated on sections of 15% thickness/chord ratio.

The result was that the drop in C_L was around 0.06, but plotting the individual values of ΔC_L against ΔC_{Do}, the profile drag increment, one could obtain a curve as plausible as that of Hoerner's and possibly of equally doubtful reliability; figure 1.

Two features of the analysis are worth mentioning.

(i) low-drag sections, like the NACA 65_2 - 415 used on the Elizabethan appear to be no more sensitive to leading edge roughness, so far as C_L is concerned, than conventional sections (see Collar's remark in Appendix 2 of the British Reviewing Body's Report); e.g. figure 2.

(ii) No marked effect on flap performance is discernible (see the remark on page 10 of the Schlichting-Gersten Report).

(c) Take-off speed. Using my own estimates of profile drag increment, together with Hoerner's curve for the decrement in lift, I found that, even if the entire upper surface of the wing were iced, lift-off would have been possible at a speed between 112 and 113 knots with a roughness height of 2 mm; figure 3.

With a roughness height of 0.5 mm, the corresponding speed range was 110.5 to 111.0 knots: figure 3.

Similar results were obtained on the basis of my own analysis of the effect of roughness on lift, according to the Abbott and von Doenhoff data; figure 3.

(d) State of the wing free from ice. In practice, it is unlikely that a low-drag wing on an aeroplane in ordinary service fulfills the design promise so far as drag is concerned. Accordingly, I made calculations for profile drag coefficient of 0.0065 and 0.0080 for the 'smooth' wing. But experience suggests that, even when free from ice, the wing of the Elizabethan was probably not smooth in the aerodynamic sense.

7. Overall conclusions and summary

(i) I disagree with the Gersten-Schlichting estimate of profile drag increment due to icing of the wing upper surface.

(ii) On the basis of my estimates, if Capt. Rayment had attempted to lift-off at 117 knots he would have been successful. Accordingly, the argument put forward by the German Enquiry that Captains Rayment and Thain acted in opposition when 117 knots was reached, does not appear to me to be tenable.

(iii) When allowance is made for the areas of the wing suggested by witnesses to have been free from ice or snow, even if the remaining parts were iced to the extent of producing a 2 mm roughness, the lift-off speed would have been increased by about 2 knots. The only way, therefore, that icing could have impaired take-off was, by the barest chance, for the pilot to have attempted to lift the aeroplane off the ground when the speed lay between 108.6 knots and 110.5 knots. However, at that stage, the aeroplane must have been under the influence of slush which would give rise to a nose-down pitching moment. The down-load on the tailplane required to trim that moment - which the evidence suggests that Capt. Rayment was trying to do - would itself contribute appreciably to the diminution in lift available for take-off.)

(iv) If the wings were iced the effect on the take-off run would have been to decrease the aeroplane speed towards the end by about 1 knot, compared with the clean aeroplane; thus demolishing the argument that, if the wings had been clean, the aeroplane would have reached its take-off speed before encountering whatever it was that caused the retardation at 117 knots.

(v) On the basis of my calculations, I wrote to James Chapman & Co on 3.1.63 giving the following opinion.

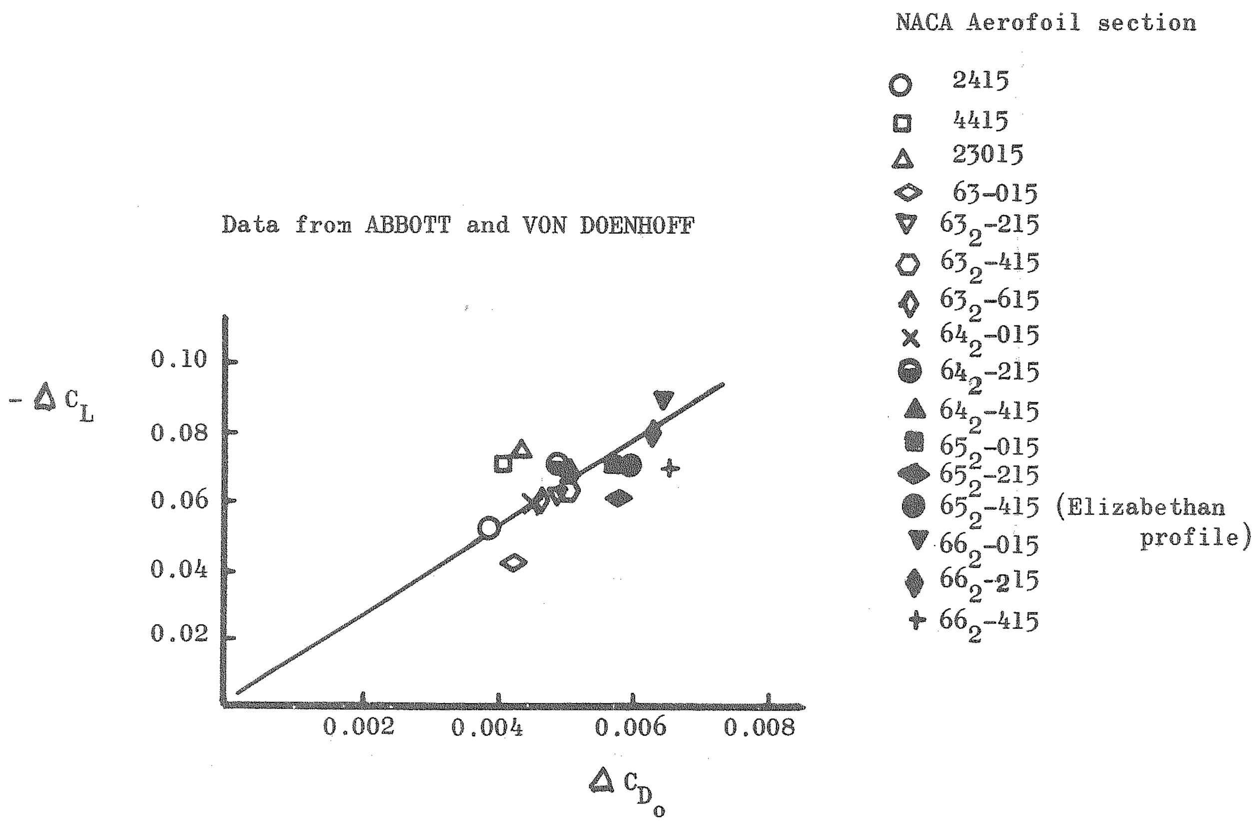
(a) The effect of ice on the take-off run was negligible.

(b) The failure to reach V_2, the take-off speed, can be accounted for in a convincing manner by slush on the runway. In this respect the reconstruction suggested in the Report of the British Reviewing Body is highly plausible, and it is extremely likely that the behaviour of the aeroplane near the end of the take-off run was due to an encounter with either denser or deeper slush.

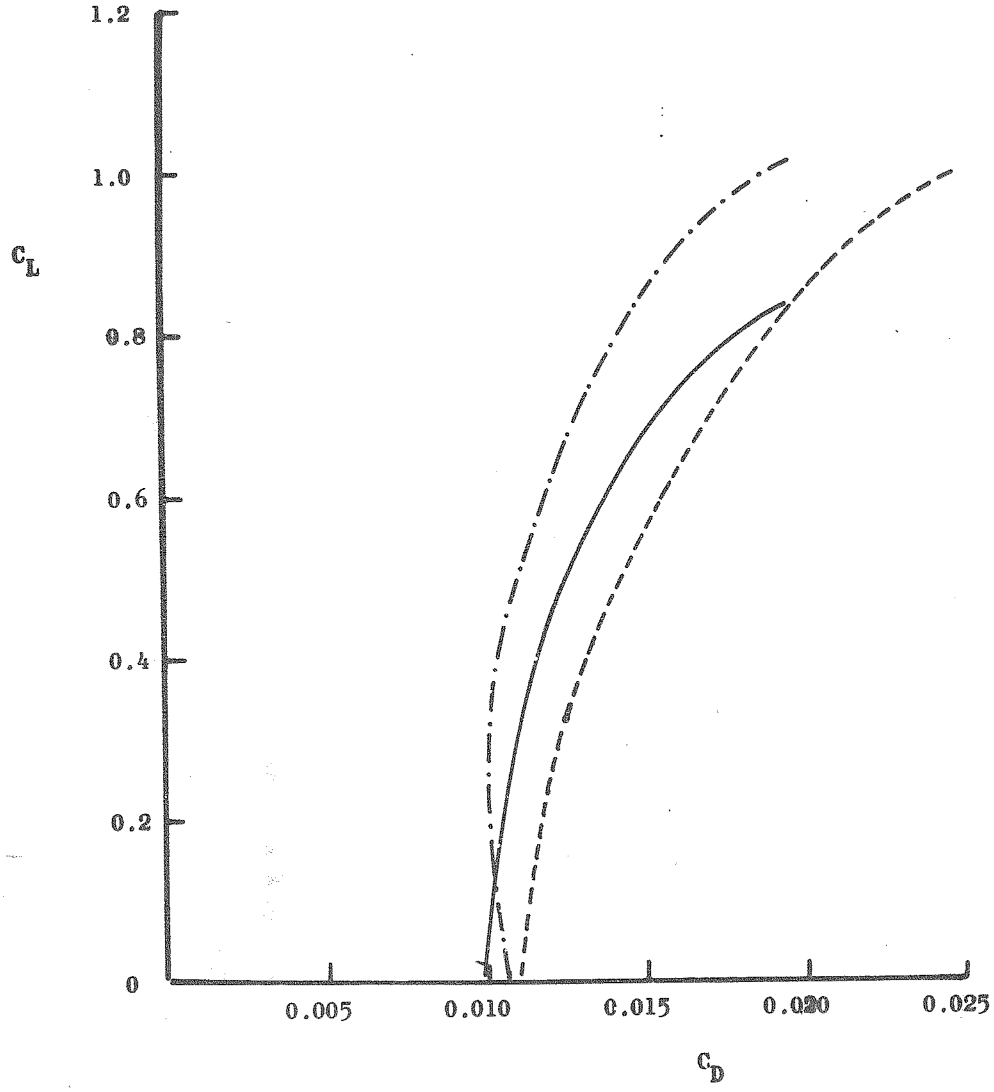
(c) The effect of ice on the take-off speed was only 1 or 2 knots. While it is possible that this could have accounted for failure to take-off, it seems to me that the chances of the pilot having selected just the moment when the speed lay between what was needed for take-off with an iced wing and what was possible with a clean wing are slender indeed.

(d) It is highly probable that the accident can be attributed entirely to slush on the runway.

June 1968 P. R. OWEN



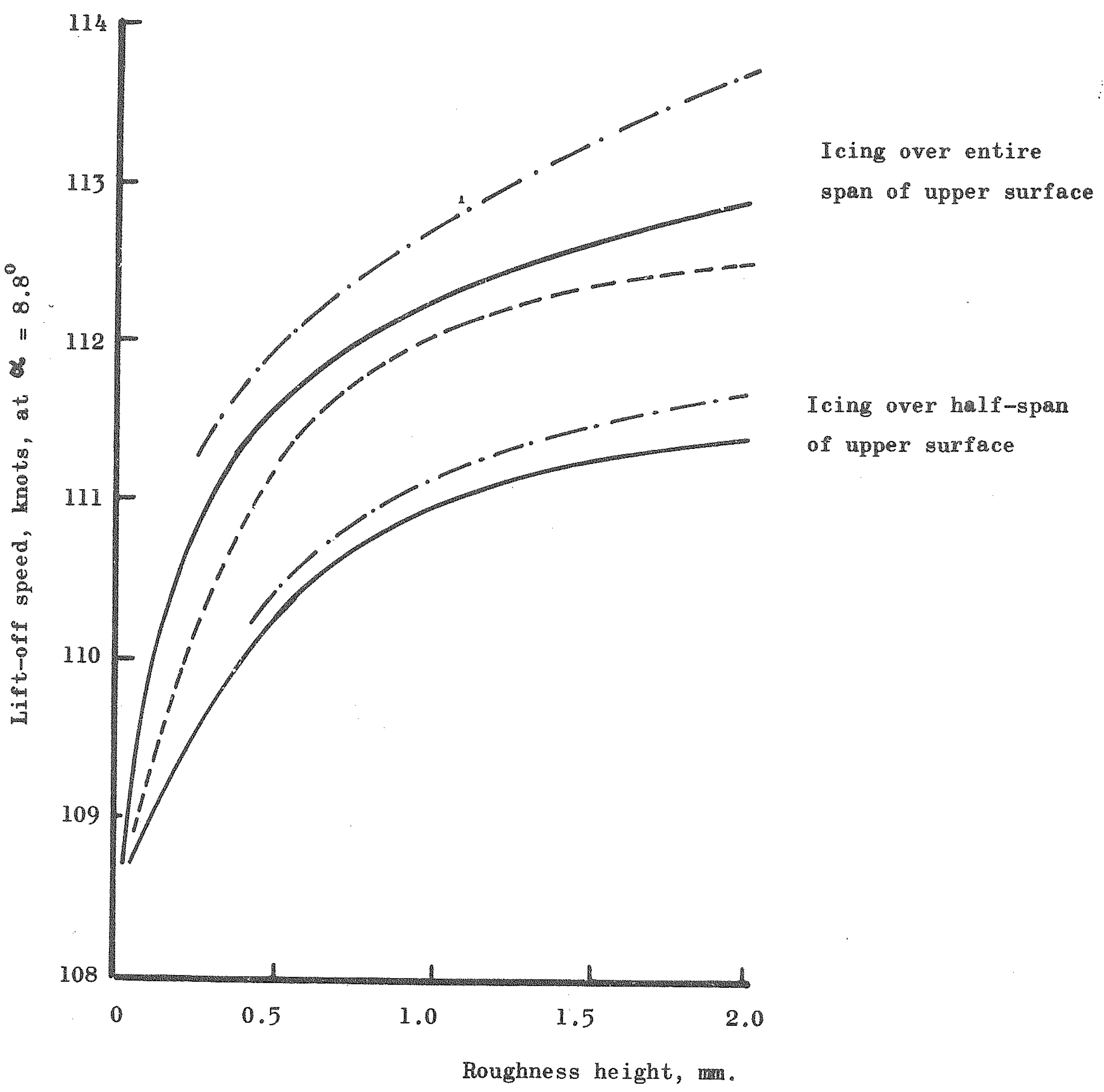
The fall in C_L from a C_L of 1.0 due to roughening the leading edge with grains corresponding to $^{k}s/c = 4.6 \times 10^{-4}$ (approximately 1.5 mm on the Elizabethan).



—·—·— NACA 65_2-415 with roughness $k_s/c = 4.6 \times 10^{-4}$ distributed over leading edge ($R = 6 \times 10^6$)

——— NACA 0012 with roughness $k_s/c = 4.6 \times 10^{-4}$ distributed over the leading edge ($R = 6 \times 10^6$)

- - - - NACA 0012 with roughness $k_s/c = 1.2 \times 10^{-4}$ distributed over the whole surface ($R = 5 \times 10^6$)



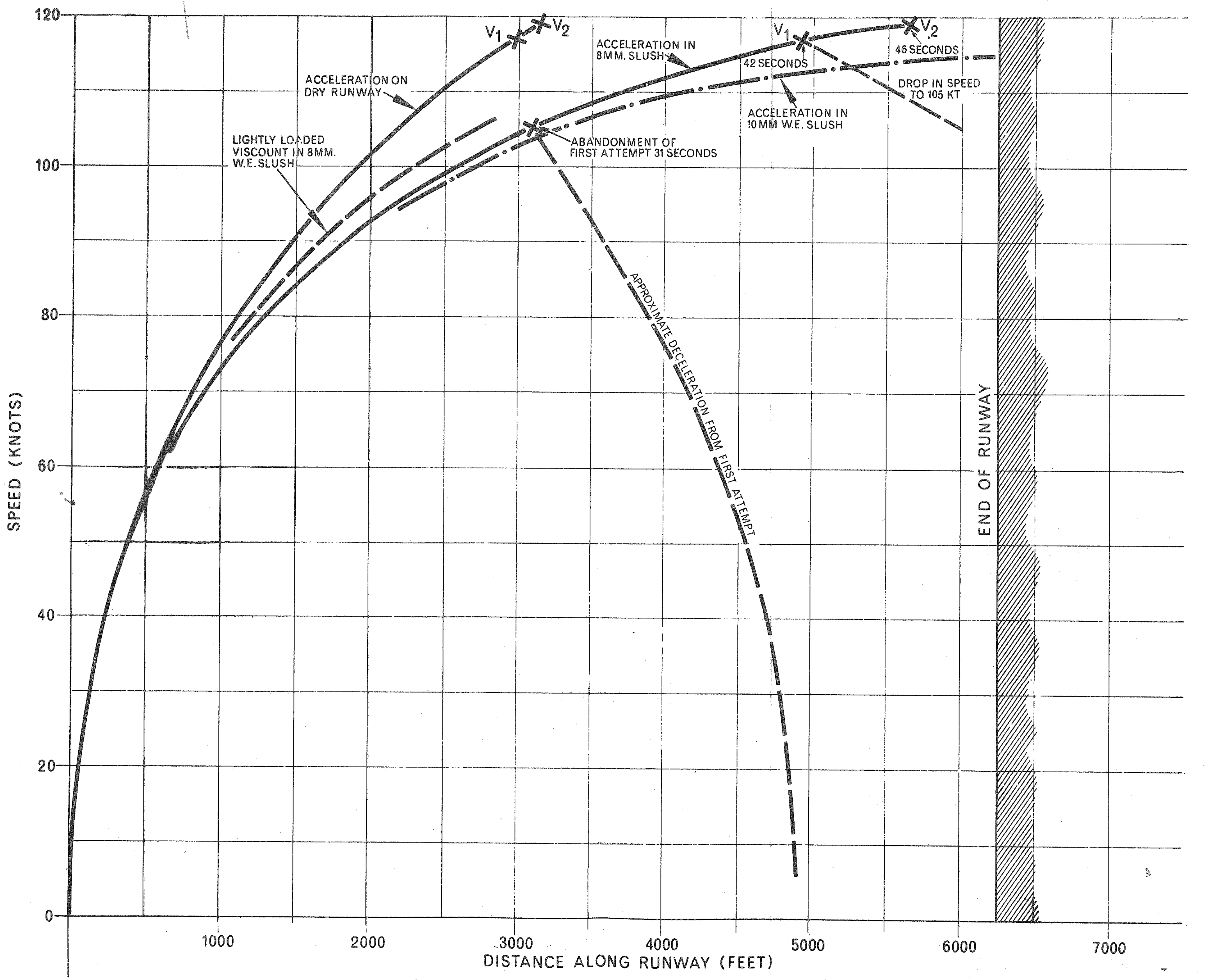
Lift-off speed at W = 54620 lb., based on independent estimate of drag increment due to roughness, but on the Schlichting-Gersten curve of $1/a_o$ against C_{Do} (fig. 9 of Report 58/21);

——— C_{Do} = 0.0065 for the clean wing.

- - - - as above, but with C_{Do} = 0.008 for the clean wing.

—·—·— based on analysis of the Abbott and von Doenhoff data. C_{Do} = 0.0065 for the clean wing.

PERFORMANCE ESTIMATES FOR G-ALZU AT MUNICH - 6th FEB. 1958

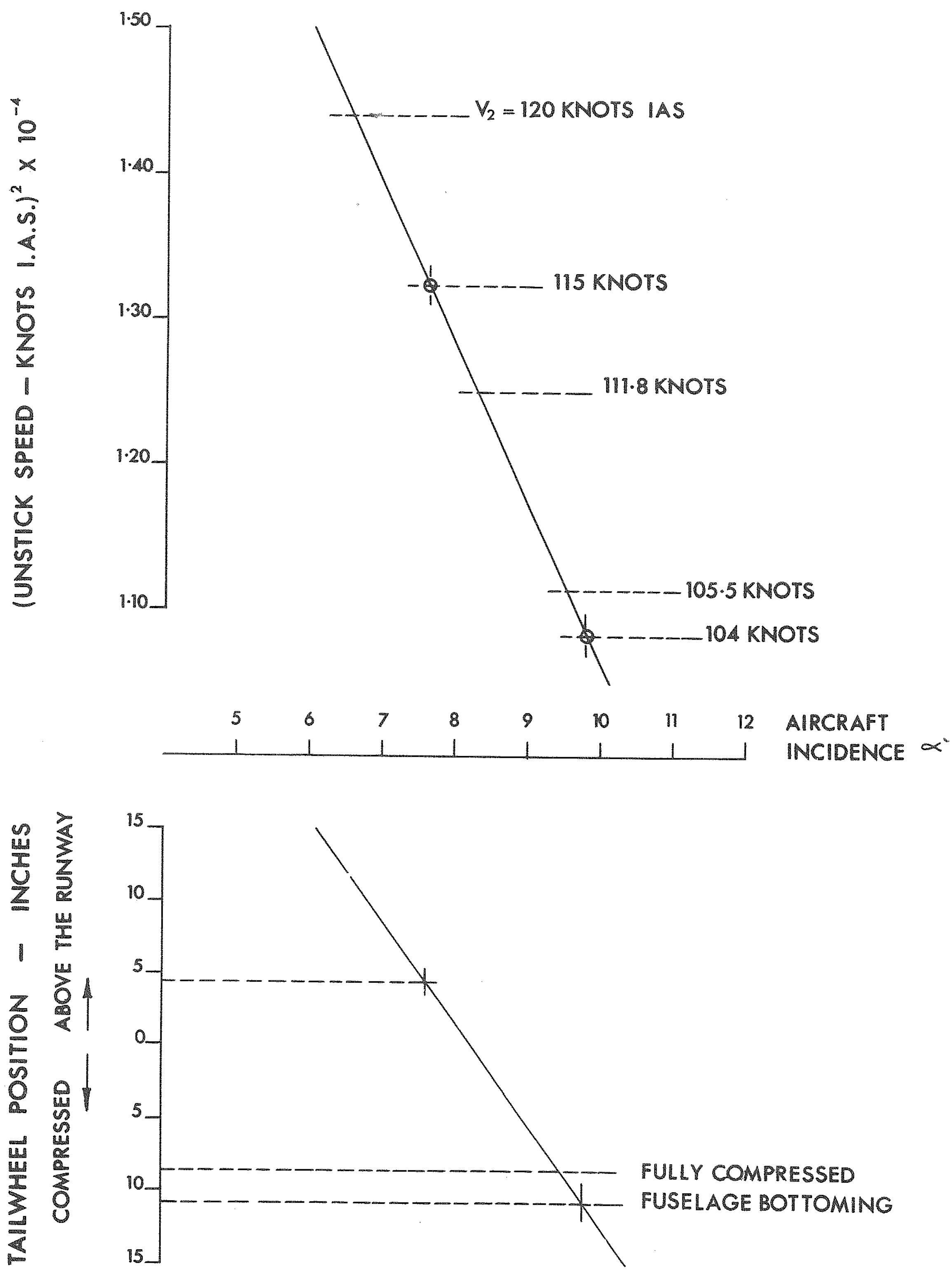


AMBASSADOR

VARIATION OF TAKE-OFF UNSTICK SPEED WITH TAILWHEEL RUNWAY CLEARANCE.

A.U.Wt. 55,000 lb.

FLAPS 0°



APPENDIX 6

Royal Aircraft Establishment, Farnborough: *Effect of Ice on the Ambassador Wing*

1. The conclusions reached by the German courts of inquiry into the accident to the BEA Ambassador aircraft G-ALZU, at Munich Airport on 6th February, 1958, depend crucially on the assumption that sufficient ice had accumulated on the outer wings (excluding the first 15-20% chord) to have prevented take-off at the maximum incidence attainable on the ground (ca 8.8°) at the known maximum speed reached during the take-off run (117 kts).

2. The question arises as to what thickness of ice deposit would be thought necessary to give rise to the assumed lift loss. This is a question that can be answered only by inference from more or less relevant data. It is vital to the argument, therefore, for any such inference to be based on sound physical principles.

3. My own qualitative assessment of the problem is as follows:

   (a) A thin ice layer amounts to an effective surface roughness, which influences boundary layer development which, in turn, modifies the lift.

   (b) Evidence of scale effect provides some indication of the possible magnitude of the boundary layer effect on lift.

   (c) The mass of data presented by Abbott and Von Doenhoff[1] suggests that scale effect on lift is very small over the essentially linear parts of lift-incidence curves. It shows up primarily in the incidence at which non-linearity begins, and in the subsequent non-linear development with incidence up to, and beyond, C_{Lmax}. The precise nature of the effect is therefore very much dependent on the manner in which the aerofoil stalls.

   (d) The aerofoil section of the Ambassador is NACA 65_2 -415, for which smooth-wing data is given at three Reynolds numbers by Abbott and Von Doenhoff, - note that there is a small difference in datum incidence between Abbott and Von Doenhoff and Schlichting[2], but that the clean-wing lift curve used by Schlichting is otherwise essentially the same as the highest Reynolds number curve given by Abbott and Von Doenhoff, at least over the material range of C_L.

A 15% thick aerofoil of this kind would be expected to rear-stall in the relevant range of Reynolds numbers - i.e. to stall as a result of the progressive forward movement of turbulent boundary layer separation near the trailing edge of the aerofoil. This expectation is consistent with the observed shapes of the lift-incidence curves which exhibit well-rounded maxima at all three Reynolds numbers.

(e) Differences in boundary layer thickness have a very small effect on lift, so long as the boundary layer remains attached to the whole of the upper surface of the aerofoil. It follows that, when separation occurs, variations in boundary layer thickness forward of the separation point have an equally trivial effect.

(f) Hence, once rear separation is present, any loss of lift must be related primarily to the position of the separation point and the subsequent development of the near wake. But the structure of the near wake itself would be expected to depend primarily on the position of the separation point. And so, for a given rear-stalling aerofoil at a given incidence, we would expect the lift to correlate primarily with the position of the rear-separation point, and to be essentially independent of the detailed nature of the boundary layer development up to separation - in particular, whether the separation is brought about naturally or artificially by roughness.

4. The foregoing assessment of the problem suggests that Hoerner's correlation[3] is over-simplified and, in consequence, that Schlichting's extrapolation from it and transfer to another aerofoil is not soundly based. Moreover, it appears to me that Hoerner's method of correlation - the drawing of straight and parallel constant-incidence lines across the lift-drag polars - is inconsistent with the mass of data now available (see Abbott and Von Doenhoff).

5. It seems to me that the only consistent procedure open to us is:

(a) to regard as relevant only data that has been obtained for the given aerofoil - and, in addition to the three clean-wing lift-incidence curves already mentioned, Abbott and Von Doenhoff give a fourth curve for their so-called standard roughness at $R = 6 \times 10^6$;

(b) to correlate the measured lift at given geometric incidence against estimated position of the rear-separation point;

(c) to use the correlation thus obtained as a calibration, whence roughness effects on boundary layer growth can be converted into corresponding effects on lift.

6. In order to carry this procedure through, it is necessary to calculate boundary layer development, with and without roughness, up to separation. For this purpose the most suitable method, in my view, is that due to M. R. Head[4], because

(a) it is not restricted, in principle, to smooth walls,

(b) the adaptation from smooth to rough wall is effected simply by a change in the wall-friction formula,

(c) the method has been well tested over a wide range of pressure distributions in recent years, and is generally regarded as the most reliable of the better known techniques.

7. However, the adaptation of Head's method to rough wall conditions has been newly developed, specifically for the present investigation, and has not been tested directly. Nevertheless, it would appear to be the best that can be done in the present state of knowledge, in part because of the inherent properties of the method outlined in 6, and in part because the actual wall-friction formulae that I have used are close approximations to relations recommended by Rotta[5] in a most exhaustive review of turbulent boundary layer theory and experiment, and shown graphically in his figures 19.1 and 19.2.

The formulae that I have used are

(a) for the smooth wall

$$C_f = 0.246 \quad 10^{-0.678H} \; (R_\theta)^{-0.268}$$

which is the well-known Ludwieg-Tillman formula, and

(b) for the rough wall

$$C_f = 0.0962 \quad 10^{-0.701H}\left(\frac{K_r}{\theta}\right)^{0.306}$$

where K_r is the so-called equivalent sand roughness.

Following Rotta, I have also assumed that for $R_\theta < (R_\theta)_s$ the surface is to be considered hydraulically smooth. This means, simply, that the rough-wall formula holds only so long as it gives higher C_f than the smooth-wall formula.

8. Head's method, adapted as described above, has been programmed and applied to the procedure outlined in 5. The results obtained are shown in the attached figures.

9. The accuracy of the procedure is not easily estimated. I would not place much reliance on the estimated positions of the separation points, in an absolute sense, but I would expect the trends to be correctly predicted. I would hope, therefore, that the order of magnitude of the roughness required to effect a given loss of lift would be correctly predicted. I certainly do not believe that a better estimate is possible at the present time.

10. Note that in my predictions I have assumed roughness to begin at 20% chord.

11. The suggestion, in the report of the second court of inquiry, that with the aerofoil section NACA 65_2-415 icing at the leading edge probably has little effect on lift, whereas icing over the central region probably has a particularly large effect, seems to me wholly without foundation. Abbott and Von Doenhoff conclude, quite generally, that roughness is less and less effective the further aft it is placed. I know of no contrary evidence.

12. In estimating the effect of ice on the drag of the wing, the Rupprecht Commission appears to have misunderstood the Gray-Glahn results[6]. These were concerned - as was the original work[7] from which the relevant figure is derived - with an increase in profile drag of an aerofoil section, for which the clean-wing profile drag was roughly 0.01. In the Rupprecht Report the percentage increase in profile drag observed in the Jacobs experiments[7] has been applied to the total drag of the aircraft.

From the data given by Abbott and Von Doenhoff[1] the contribution to the total drag from the wing profile drag would appear to be about 0.01. Even if we assume the sectional profile drag to be doubled by the icing, the direct effect on the total drag (assuming 50% of the wing to be affected) would amount only to an increment of 0.005.

Aero.L/ECM
Aerodynamics Department
27th February 1967

E. C. MASKELL

REFERENCES

No.	*Author*	*Title, etc.*
1.	I. H. Abbott A. E. Von Doenhoff	Theory of Wing Sections. Dover, 1959.
2.	H. Schlichting K. Gersten	Investigation into the take-off run of the crashed aircraft G-ALZU "Elizabethan". Inst. for Stream-Mechs. Tech. Univ., Brunswick, Rep. 58/21.
3.	Hoerner	Einfluss der Oberflachenrauhigkeit auf die aerodynamischen Eigenschaften der Luftfahrzeuge. Ringbuch der Luftfahrttechnik IA9 (1937).
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5.	J. C. Rotta	Turbulent Boundary Layers in Incompressible Flow. Progress in Aeronautical Sciences Vol.2. Pergamon Press,1962.
6.	V. H. Gray U. H. Von Glahn	Effect of ice and frost formations on drag of NACA 65_1-212 airfoil for various modes of thermal ice protection. NACA TN 2962 (1953).
7.	E. N. Jacobs	Airfoil section characteristics as affected by protuberances. NACA Rept. No.446 (1932).

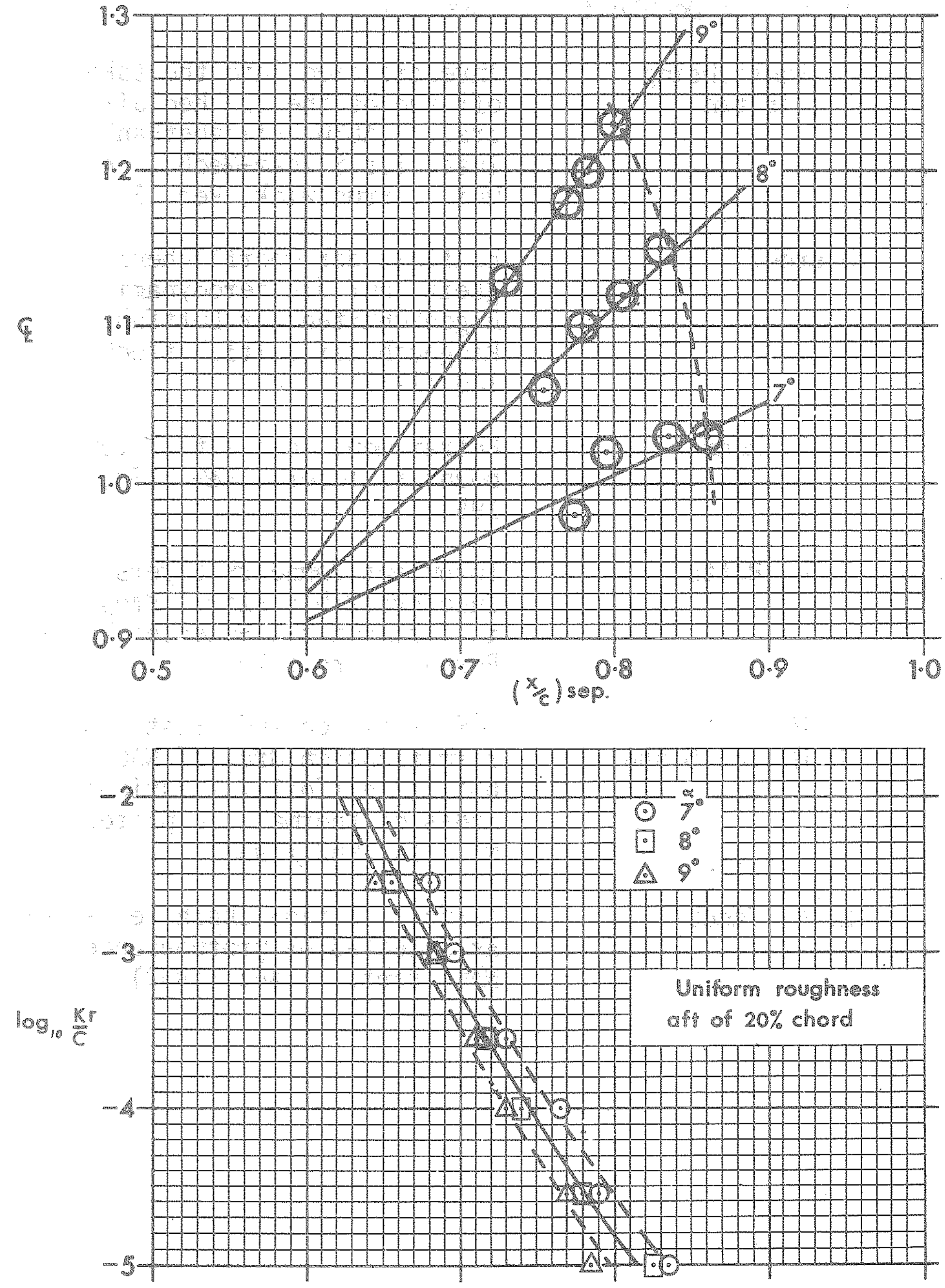

FIG.1 Correlation of calculated separation point against observed C_L and roughness height.
(Aerofoil section NACA 65_2-415)

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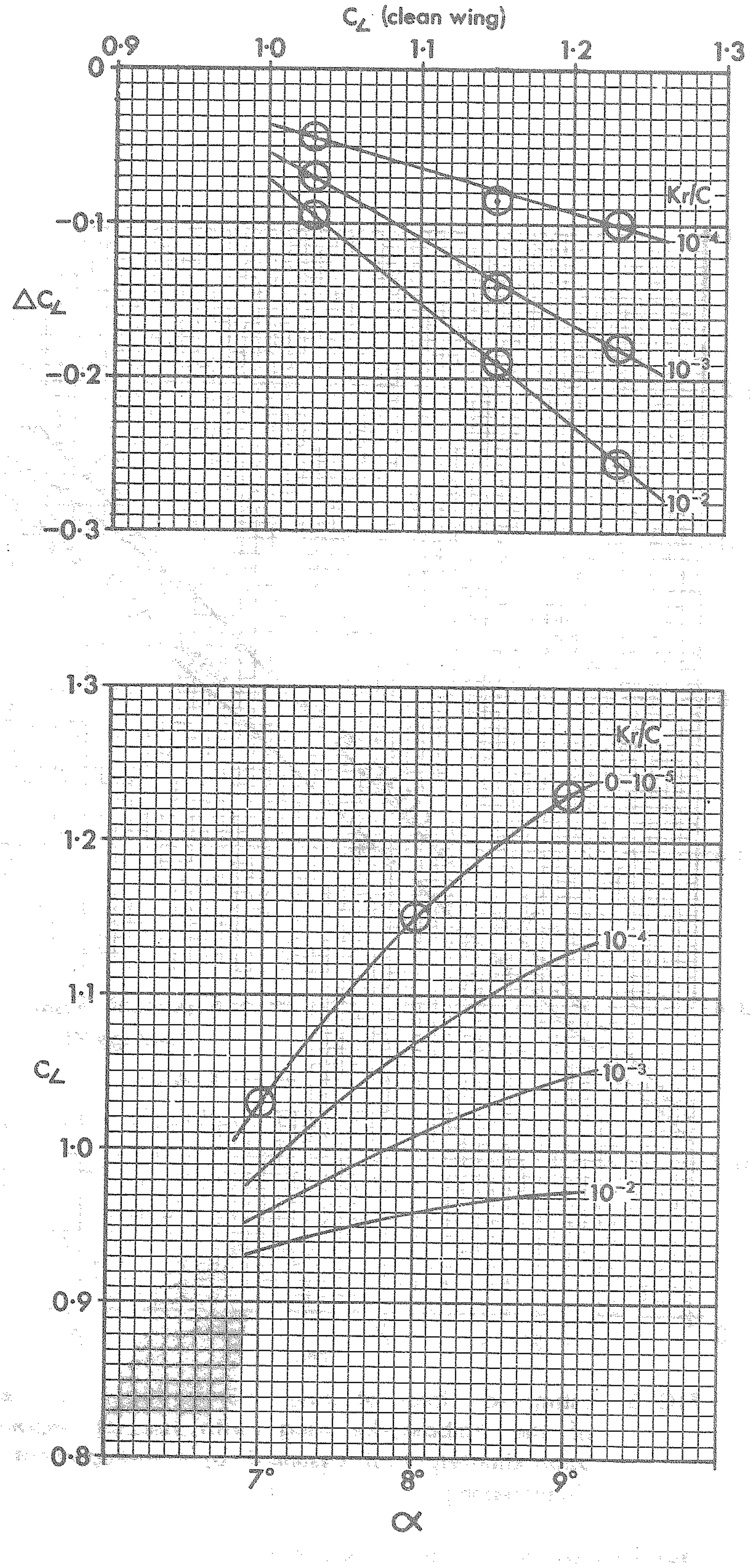

FIG. 2 The estimated effect of roughness (aft of 20% chord) on the lift of aerofoil section NACA 65_2-415

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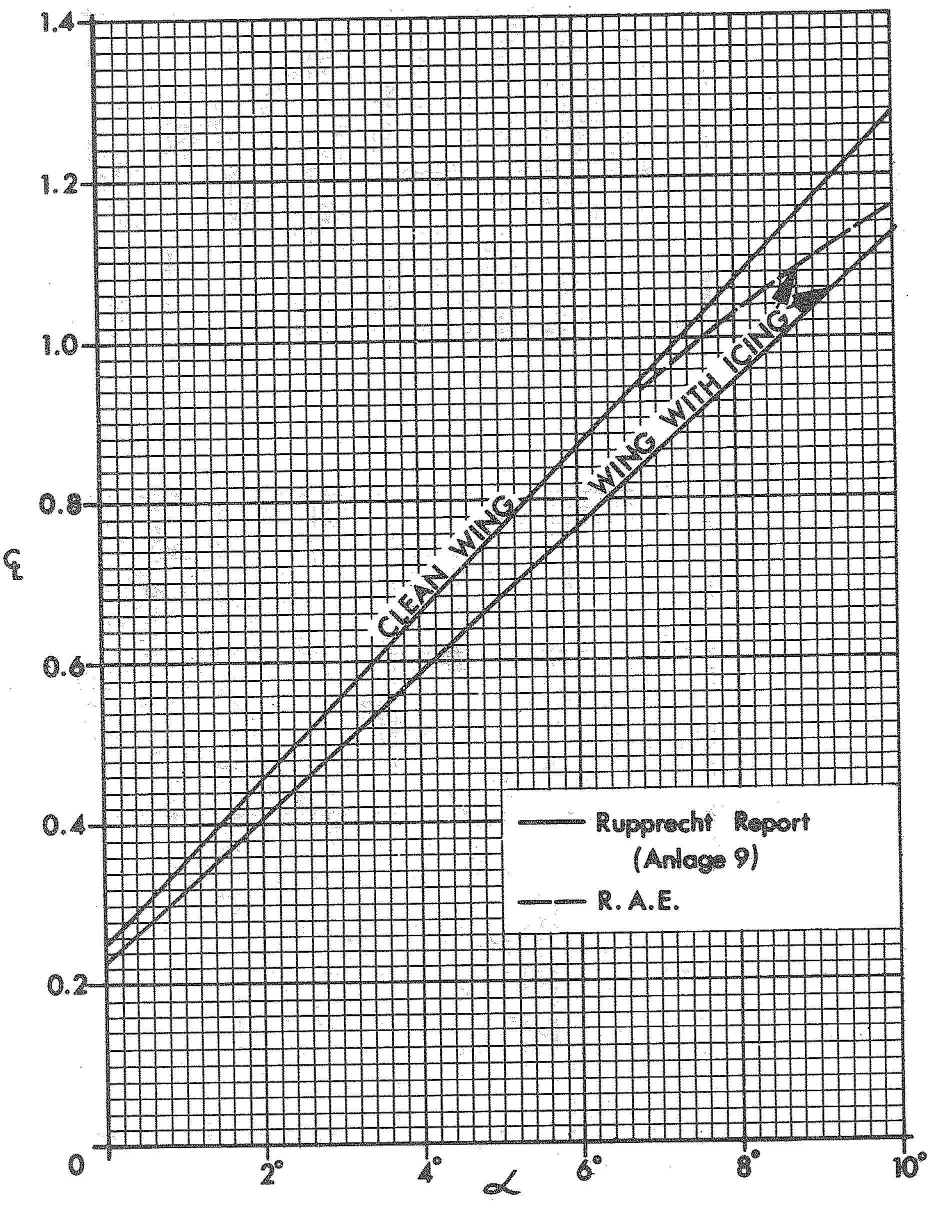

FIG. 3 Estimated effect of icing (5mm. thick) on the lift of the Ambassador wing (with the ice deposit approximately that assumed by the Rupprecht Commission)

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APPENDIX 7(a)

Criticism by Professors P. R. Owen and A. D. Young of an Argument set out in Report 58/21 (Investigation into the Take-off Run of the Crashed Aircraft G-AZLU 'Elizabethan') by Professors Dr. H. Schlichting and Dr.-Ing. K. Gersten of the Institut für Strömungsmechanik, Technische Hochschule, Braunschweig

The criticism has been concerned with the estimation of the increment of profile drag due to roughness in the above document, and the consequential estimate of the effect on lift.

It has been pointed out that in the formula

$$C_{WP} = 2.25\ C_f.$$

on which the solid line in figure 13 is based, the skin-friction coefficient Cf gives the skin-friction drag of one side of a flat-plate according to the formula

$$C_f = (1.89 - 1.62 \log \frac{Ks}{l})^{-2.5}$$

and that the factor 2.25 must be interpreted as a factor of 2 to account for upper and lower surfaces, and a further 0.25 to allow for the fact that the upper surface velocities are rather higher than on a flat plate. That is to say that the solid line in figure 13 corresponds to an aerofoil completely roughened on both surfaces. Provided that it is interpreted in this way the figure is accepted as fairly reliable.

However, the straight line marked "glatt" in figure 13 is drawn at a profile drag coefficient of $7X10^{-3}$, and if the effect of roughness height 10^{-4} is considered the corresponding profile drag coefficient is roughly $11.5X10^{-3}$.

It follows that a difference of 0.0045 is ascribable to roughness of that height distributed over the whole wing surface, both upper and lower. This figure is to be compared with a value of 0.01 for ΔC_D. taken on page 10 of the paper.

In a similar way, it may be inferred from figure 13 that the increment of profile drag for a surface roughness of 0.001 is 0.011 rather than the value of 0.02 taken on page 10.

In consequence it has been suggested that your assumed profile drag increments are some 2 to 2½ times those indicated by Hoerner, rather than the 'slightly greater' claimed in the footnote to page 10.

It has been argued, further, that since the lower surface of the wing was smooth your estimated drag increments must have been a further factor of 2 too large, and since only half of the wings were iced-up, there is yet another factor of 2 to be taken into account. According to this argument, therefore, your increments are something like 8 times too large, at least at low incidence. However, the fact that the first 10% of the wing was considered to be free of ice introduces a further error.

It is the consequences of these assumptions on the estimated loss of lift that is of greatest importance. It has been suggested that if the drag estimates are greatly in error, then there is a correspondingly large error in the calculation of the effect on lift from figure 9 of the paper. Although it might be argued that the important thing is the effect on the maximum lift coefficient, this argument has been countered by the claim that since angles of incidence up to only about 8½ degrees need be considered - an incidence range over which the lift curve can be expected to be linear - the effect on lift-curve slope is, in fact, of vital importance.

APPENDIX 7(b)

Comments by Professors Dr. H. Schlichting and Dr.-Ing. K. Gersten on the Criticism by Professors P. R. Owen and A. D. Young on our Expert Opinion (Report 58/21) on the Aircraft Accident in Munich

1. In the last sentence of the criticism it is quite correctly stated that the decisive effect of roughness on lift occurs in the linear portion of the lift curve and consists principally of a reduction in the lift coefficient $dC_L/d\alpha$.

2. According to S. Hoerner (reference 2 in our expert opinion) this reduction in $dC_L/d\alpha$ is clearly related to the profile drag C_{Dp} (Figure 9 in our expert opinion). The relation shown is based on measurements of profiles which were distinctly covered on both sides with roughness.

3. This reduction in $dC_L/d\alpha$ in consequence of roughness is based on the following physical circumstances:

   In consequence of the roughness of the upper surface a considerably thicker boundary layer is formed than with a smooth upper surface. This thickening of the boundary layer is greater on the upper side than on the lower side, which causes a reduction of the effective camber. This gives rise to a decrease in circulation round the profile and thus in lift. A previously occurring detachment of the boundary layer as a consequence of roughness may play an additional part.

   If the underside of the profile is free of roughness, this would make no difference to the decreased circulation. In fact a greater decrease in circulation might then be expected, since the modification of the effective camber would then be accentuated.

4. For purposes of fig. 9 in our expert opinion it must naturally be assumed in calculating the increase in profile drag that the wing is covered by roughness on both sides, since the diagram is only applicable in this case. For the reasons given under para. 3 the absence of roughness on the underside of the profile makes no difference to the reduction in $dC_L/d\alpha$.

5. In calculating the profile drag, the frictional resistance of a flat plate subjected to a longitudinal airflow were first determined using well-known formulae. These figures were multiplied by the factor 2.86. This factor makes allowance for both sides of the profile and also for the thickness of the profile according to Schlichting/Truckenbrodt *Aerodynamik des Flugzeuges*, Volume I, Springer-Verlag 1962, page 444. This gives the following values:

	Upper surface	*Flat plate*	*Profile* $\frac{d}{\ell} = 0.15$	*Increased drag over I*
I	Smooth $Re = 1.3 \times 10^7$	$C_f = 2.75 \times 10^{-3}$	$C_{DP} = 7.9 \times 10^{-3}$	-
II	Rough $k_S/\ell = 10^{-4}$	$C_f = 4.95 \times 10^{-3}$	$C_{DP} = 14.2 \times 10^{-3}$	$\Delta C_{DP} = 0.0063$
III	Rough $k_S/\ell = 10^{-3}$	$C_f = 8.45 \times 10^{-3}$	$C_{DP} = 24.2 \times 10^{-3}$	$\Delta C_{DP} = 0.0163$

The increases in profile drag thus ascertained are immediately applicable only to frictional resistance. Since as is shown under paragraph 3 there is a possibility of change in pressure distribution in consequence of roughness and also possibly of detachment, an additional pressure drag is to be anticipated, for which an estimated additional factor of $C_{DP} = 0.0037$ was included in the expert opinion. This gives the figures for increased profile drag taken as a basis in the expert opinion.

6. Even if the first 10 per cent of the wing profile were free of icing, this would in all probability make no difference to the results, since the flow processes at the trailing edge of the wing are decisive for the decrease in lift.

7. It is asserted in the criticism that only half of the wing was iced up. This assertion is by no means borne out by the available information. We therefore assumed in our expert opinion that the entire upper surface of the wing was iced up. If in fact parts of the wing remained free of ice, for the reasons explained under paragraph 3, calculation of the increase in profile drag should be based not on a smooth wing surface but on the span of the ice-free portion of the wing surface. This would produce only minor alterations in the final results regarding decreased lift.

8. The only uncertainty in the calculation of the decrease in $dC_L/d\alpha$ given in our expert opinion lies in the estimated value for the co-efficient of pressure drag in consequence of roughness. On the most pessimistic assumption this uncertainty could produce a maximum error of 37% in ΔC_{DP} and thus of 5% in $dC_L/d\alpha$. There can however be no question at all of a factor 8.

9. As is clearly stated in our expert opinion, we expressed the effect of icing on the aerodynamic qualities of the wing as a roughness effect. We then gave a feasible estimate of the correlation between icing and sand roughness. As long as this correlation between icing and sand roughness remains unknown, there is little point is criticising the calculations we made at that time, since the question of the correct roughness factor to be included remains open.

10. During the last ten years extensive tests have been made at considerable expense on the effect of slush on the take-off process. It is indeed remarkable that on the other hand no attempt has as yet been made to carry out simple wind tunnel tests which could be made with comparatively modest means, in order to determine the effect of icing on the aerodynamic qualities of airfoils. We therefore suggest that such wind tunnel tests should be carried out in order to obtain really new information, which might then possibly be used in a valid criticism of the assumptions underlying our calculations.

July 1968

APPENDIX 7(c)

Comments by Professor A. D. Young on Appendix 7(b)

Item 1

The ready agreement here conflicts with the footnote on p.10 of the Schlichting-Gersten Report.

Items 2, 3 and 4

Here it is argued that in order to apply fig.9 of the Schlichting-Gersten Report due to Hoerner the correct drag coefficient to use to assess the lift curve slope ($dC_L/d\alpha$ or $C'_{a\alpha}$) of a wing with roughness only on the upper surface is that of the wing with roughness on both surfaces. This is because, so the argument runs, the data for roughened wings included in fig.9 all refer to roughnesses on both surfaces and as far as lift is concerned only the roughness on the upper surface matters.

This strikes me as a most dubious argument. Hoerner produced the empirical correlation of lift curve slope and profile drag coefficient of fig.9 in 1937 on the basis of a few somewhat disparate test results available to him at the time, and this simple correlation is open to considerable criticism which I will briefly develop later. However, Hoerner claimed that it could be quite generally applied and was independent of Reynolds number, section shape and thickness and, by implication, of the roughness distribution or transition positions. If one accepts Hoerner's correlation as Schlichting and Gersten do then one must accept Hoerner's claim that the lift curve slope is determined by the actual profile drag coefficient and is not dependent on how this coefficient arises. Further, Prof. Gersten seems to have overlooked the fact that in considering lift curve slope we are considering not the lift at a given incidence but how the lift is changing with incidence, and the boundary layers on both surfaces will modify this insofar as their displacement thickness distributions will change with incidence. It is far from evident that the effects of the boundary layer on the lower surface of a wing will necessarily be either negligible or otherwise of opposite sign to the effects of that on the upper surface as far as $dC_L/d\alpha$ is concerned.

If, for example, the displacement thickness on the lower surface decreases with increase of incidence whilst that on the upper surface increases, as is likely, then both boundary layers will act to reduce the lift curve slope, and this will be true whether the surfaces are rough or smooth.

However, although Prof. Gersten says the drag coefficient was calculated on the basis of both upper and lower surfaces being rough solely for the purpose of determining $dC_L/d\alpha$ from fig.9, nevertheless it is clear from fig.16 of the Schlichting and Gersten Report showing the calculated polar curves for the aircraft that the assumed drag increment based on both surfaces rough were also used in all the take-off calculations.

I must at this stage draw attention to what appears to be a further error in the Schlichting-Gersten Report. In fig.15 the ratio of the lift curve slopes for Cases I (smooth) and II ($k_s/\ell = 10^{-4}$) is 1.20. Application of fig.9, however, with Schlichting and Gersten's assumed drag increment of 0.01, leads to the ratio 1.15 for the ratio of the lift curve slopes for infinite aspect ratio and correction of this ratio to allow for the finite aspect ratio of the Elizabethan reduces it to 1.13. It seems therefore that solely in applying fig.9 the reduction in the lift curve slope has been calculated by Schlichting and Gersten about fifty per cent too large. For Case III there is a corresponding error, the reduction in lift curve slope as calculated by Schlichting and Gersten is about thirty per cent too large. These errors are equivalent to significant under-estimates of the lift at take-off speed and incidence, namely, about 3,500 lb and 4,000 lb respectively.

On the general question of the validity of fig.9 the following points may be noted -

(a) Even amongst the few data shown there is a deviation from the curve of the order of 5% in $dC_L/d\alpha$. Under take off conditions for the aircraft this deviation is equivalent to about 3,000 lb in lift.

(b) The initial point for the curve for zero profile drag is derived from the theoretical value for thin wings ($dC_L/d\alpha = 2\pi$ with α in radians), in fact the theoretical value of $dC_L/d\alpha$ increases with thickness and is about 12% higher than 2π for a 15% thick wing. This fact alone shows that a single curve cannot hold generally.

(c) It is easy to cull from the considerable body of data presented by Abbott and Doenhoff in their book *Theory of Wing Sections* cases where the trend of the variation of $dC_L/d\alpha$ with C_D is opposite in sign to that indicated by Hoerner's curve in fig.9. The following table is an example obtained from fig.57 and 67 of that book:

Section	C_D (or C_{wp})	$1/dC_L/d\alpha$) or $1/C'_{a\infty}$
63-218	0.0046	0.148
64-218	0.0044	0.151
65-218	0.0041	0.153
66-218	0.0037	0.165

(d) A comparison of fig.64 and fig.79 of Doenhoff's book showing the effects of roughness strips at the leading edge, 0.2c and 0.3c on the lift and drag of the NACA 63(420) - 422 section demonstrates a significant effect on lift only for the roughness strip at the leading edge for which the corresponding drag coefficient increment is about 0.004; for the roughness strip in the other two positions the drag coefficient increments are less but not insignificant being about 0.0026 and 0.0015 respectively.

(e) The Royal Aeronautical Society Data Sheets for lift curve slope of wing sections emphasise the importance of such parameters as section shape, particularly trailing edge angle, Reynolds number and transition positions.

Item 5

The factor 2.86 quoted as required to multiply the skin friction coefficient for one side of flat plate to determine the corresponding drag coefficient of a wing of 15% thickness was not quoted in the Schlichting and Gersten Report where the only factor quoted was 2.25 in fig.13 of that Report. It is however more nearly correct than this latter figure, although it is based on a series of approximate calculations. It may be noted that in Schlichting and Truckenbrodt's book *Aerodynamik des Flugzeuges* Vol.I, p.444 the following formula is also quoted:

$$C_{wp} = 2C_{ft} \, (1 + cd/\ell)$$

where C_{wp} is the profile drag coefficient, C_{ft} is the flat plate skin friction coefficient, d/ℓ is the thickness-chord ratio and c is a number that is stated to vary between 2 and 2.5. This gives a range for the above factor when d/ℓ = 0.15 of 2.6 to 2.75, suggesting that the value of 2.86 is a little high.

However, I can see no justification whatever for adding a further 0.0037 to the increments for profile drag coefficient so calculated even though it brings them up to the convenient round figures of 0.01 and 0.02. The above factor is designed to yield the profile drag coefficients and hence automatically includes the form or pressure drag changes due to the roughness and no further allowance for the latter on the lines argued by Prof. Gersten on p.3 can be justified. An increment of 0.0037 in drag coefficient represents the effect of a very substantial protuberance as is evident from NACA Tech. Note 2962 by Gray and Glahn. It is about 50% of the basic wing profile drag coefficient and at small incidences would be produced by a spoiler type protuberance no less than about 0.3" high at 0.15c back from the leading edge.

Item 6

I can only say that I am astonished at this statement. To quote from Abbott and Doenhoff's book, p.143:

> 'It has long been known that surface roughness, especially near the leading edge, has large effects in the characteristics of wing sections. The maximum lift coefficient, in particular, is sensitive to leading edge roughness. ...... Fig.64 shows that roughness strips located more than about 0.20c from the leading edge have little effect on the maximum lift coefficient or lift-curve slope.'

The last sentence refers to the results already mentioned above.

The following is my translation of some sentences on p.438 and 439 in the book by Schlichting and Truckenbrodt already referred to:

> 'Apart from the Reynolds number, the profile shape particularly in the neighbourhood of the nose is of great importance in determining the flow separation at large incidences and hence the value of C_L max., since the shape of the profile nose determines the pressure distribution there.'

Then follows a reference to a paper by Prof. Nonweiler and a reproduction of a figure from that paper showing the marked dependence of the maximum lift of airfoils in the range of thicknesses from about 8% to 20% on their nose radii of curvature. This sensitivity is particularly marked for aerofoils about 15% thick. It follows that roughnesses which could distort the profile shape in the neighbourhood of the nose or considerably increase the boundary layer thickness in the strong adverse pressure gradients that develop there at large angles of incidence could have a marked adverse effect on the lifting characteristics of the wing by provoking an early separation. In the light of the evidence that I have seen, and this includes the paper by Szukiewiez on the icing tests on the Elizabethan in flight, I would conclude that provided the first fifteen per cent of the wing upper surface and a few per cent of the lower surface near the leading edge were clear only protuberances and surface distortions further aft of a most gross kind (i.e. something measurable in centimetres) would be likely to cause a major deterioration of the lifting characteristics of an aircraft wing. On the other hand relatively small distortions or roughness over the upper surface near the leading edge could have a major effect on those characteristics. Further evidence on the effects on drag is given in my comments referring to Item 10.

Item 7

Prof. Gersten appears not to have read the Report of the second German Commission of Inquiry into the accident, wherein it is clearly accepted (p.12) that some fifty-five per cent of the upper wing surface was clear of ice or snow.

I cannot follow the point that he is trying to make in the last two sentences. The span of the clear area is also roughly fifty per cent of the wing span, and it is reasonable therefore to assume that the overall wing characteristics will be mid-way between those for the whole wing smooth and the whole wing rough. This means that the calculated effects of the roughness are halved as far as the airplane is concerned, hardly a 'minor alteration'.

Item 8

From my previous comments it will be clear that I cannot accept that Prof. Gersten has met the criticism that his calculations are out by a factor of about 2 in not allowing for the lower surface being free of deposit plus a further factor of about 2 in not allowing for the accepted fact that at least half the upper surface was free of deposit. There is a further and by no means unimportant factor to be applied to allow for the fact that the first fifteen per cent of the upper surface was clear this might well reduce the calculated effects (after dividing by 4) by at least a further 25%. To be conservative, however, let us assume that this reduction is 10%. There is also the error in applying fig.9 referred to in my comments on Items 2, 3 and 4.

With regard to the extra drag increment of 0.0037 that Prof. Gersten here admits might be unjustified, he dismisses this as resulting in only a 5% change in $dC_L/d\alpha$. What he omits to note is that it is more than half the increment that he would otherwise calculate for the case II roughness ($k_s/\ell = 10^{-4}$) and the corresponding overall change in $dC_L/d\alpha$ that he calculates is about 60% too large due to this assumption alone. Indeed, at take-off speed and incidence it is equivalent to a loss in lift of about 4,000 lb.

Thus, I would maintain that even if one accepts the general approach adopted in the Schlichting-Gersten Report the effects on drag and lift quoted should be multiplied by a factor $\frac{1}{2} \times \frac{1}{2} \times 0.9 \times \frac{1}{1.5} \times \frac{1}{1.6} = 1/9.4$ for the Case II roughness, and for the Case III roughness the factor is $\frac{1}{2} \times \frac{1}{2} \times 0.9 \times \frac{3}{4} \times \frac{1}{1.2} = 1/7.1$. The precision with which these factors can be specified is open to question, what is clear is that the original estimates of Schlichting and Gersten are seriously incorrect and are about 8 times too large.

Item 9

I fully accept that there is a great measure of uncertainty in any calculations insofar as we cannot readily identify or correlate whatever roughness that there was on the wings with the sand roughness of standard wind tunnel tests. As I have argued in my report, however it seems to me reasonable to infer that the layer of yielding mixture of melting snow and water that was on the wings at the time of the accident could not have as large an effect on drag and lift as a sand roughness of much the same average particle size. Therefore, to base one's calculations on experimental data obtained with sand or carborundum roughness is likely to lead to an over-estimate rather than an under-estimate of the roughness effects.

Item 10

I would not argue against such wind tunnel tests, but we must be clear that unless what was on the wing is agreed upon and can be made the subject of test, we shall be testing a wing with various forms of artificial roughness and the difficulty referred to in Item 9 would remain. What such a test would then clarify is how far the Elizabethan wing section is sensitive or otherwise to roughness deposits away from its leading edge. I think that I know the answer but such tests would no doubt be of some value.

However, there is considerable pertinent evidence to be derived from NACA Tech. Note No. 2962 by Gray and Glahn giving the results of wind tunnel measurements on the profile drag of a wing of NACA 65 - 212 section for various incidences with a variety of ice formations. Unfortunately, no systematic measurement of the ice thickness and roughness was apparently made, but some reasonable inference can be drawn from the text and the photographs. I have attempted to summarise the main results in the attached table, for which it should be noted that the ice formation occurred on both surfaces of the wing.

It will be noted that in general where the first 12-13% of the wing was clear the increment in profile drag resulting from ice or frost did not exceed about 0.004 and in most cases the increment was considerably less. The only exception was the combination of heavy runback ice (about 5 mm thick) and heavy frost (about 3 mm particles) for which the drag increment was 0.0093. It will be recalled for comparison that Schlichting and Gersten assumed a drag increment of 0.01 for Case II i.e. a roughness height of about 0.25 mm. It will be clear from the table that to obtain the increment of 0.02 which they assumed for Case III (roughness height of about 2.5 mm) a quite unrealistic massive deposition of rough ice would be required extending backwards from the leading edge and at the leading edge it would need to have the very irregular disruptive formation of mushroom glaze ice some inches thick. These conclusions are consistent with the results of the icing tests in flight reported by Szukiewiez.

TABLE SUMMARISING SOME SALIENT DATA FROM NACA TECH. NOTE 2962

Ice type	α	ΔC_D *(ice on both surfaces)*	*Comments*
Glaze	5^o	0.0023	ice about 5 mm thick, with nodules 5-10 mm, from L.E. to 0.2c on U.S. and to 0.15c on L.S.
Mushroom glaze	8^o	0.0094	ice formation on L.E. 25-50 mm thick there, elsewhere about 12 mm thick extending to 0.08c on U.S. and L.S.
Mushroom glaze	8^o	0.0145	ice formation on L.E. 50 mm + thick there, elsewhere about 12 mm thick extending to 0.13c on U.S. and L.S.
Runback icing	2^o	0.0025	ice extends backwards on U.S. from 0.13c (occasionally at 0.04c) and on L.S. from 0.13c Ice thickness 3-5 mm
Runback icing	5^o	0.0037	ice extends backwards on U.S. from 0.08c and on L.S. from 0.13c
Runback icing	8^o	0.0025	ice extends backwards on U.S. from 0.13c and on L.S. from 0.13c
Runback icing	8^o	0.0088	ice extends backwards on U.S. from 0.01c and on L.S. from 0.13c
Heavy glaze with runback	2^o	0.008	ice extends backwards on U.S. from 0.02c and on L.S. from 0.02c. Thickness about 12 mm and with nodules of about 12 mm in size
Heavy glaze with runback	8^o	0.011	ice extends on both surfaces from L.E., with nodules of order 12-25 mm and thickness about 12 mm
Runback	8^o	0.0005	ice (about 2-3 mm thick) extends from 0.13 c on U.S. and L.S.

Ice type	α	ΔC_D *(ice on both surfaces)*	*Comments*
Glaze ice plus frost	5^o	0.013	25-50 mm on L.E. followed by rough nodules about 5-10 mm in size and frost extending back from 0.12c with particles about 1-3 mm in size
Runback ice plus frost	2^o	0.0093	ice extends from 0.12c on U.S. and L.S. (about 5 mm thick plus frost with particles about 3 mm in size)
Frost	5^o	0.0041	frost extends aft of 0.13 c on both surfaces, particles about 1-3 mm in size

APPENDIX 7(d)

Comments by Professor P. R. Owen on Appendix 7(b)

Professor Gersten's comments fall into three groups: (i) the extent to which the aeroplane was iced, (ii) the effect of roughness on the lift curve slope, and (iii) the estimate of the increment in profile drag due to roughness. I shall discuss these in turn.

(i) The extent of the icing

Whatever ice or snow deposit formed on the aircraft, the evidence presented to the first Inquiry, CAP 153, and subsequently accepted by the re-opened Inquiry, CAP 292, was that it did not occupy the entire upper surface. Indeed, the re-opened Inquiry based its calculations on a roughened area amounting to 45% of that of the wing upper surface.

I therefore fail to understand why Professor Gersten clings to the "expert opinion that the entire upper surface of the wing was iced up" (c.f. his paragraph 7).

Moreover, the re-opened Inquiry accept the evidence that the first 15% of the chord was free from deposit; a fact that seems to have been only partially and grudgingly accepted by Professor Gersten in his paragraph 6.

(ii) The effect of roughness on the lift curve slope

Professor Gersten's remarks on the relation between the lift curve slope of a wing and its profile drag coefficient appear to me to be inconsistent.

In the first instance, his paragraph 3, he maintains that the use of Hoerner's correlation must include the assumption that both surfaces of the wing are roughened, because it was in that condition that the experiments, from the results of which Hoerner's curve was constructed, were performed. He then goes on to state, in paragraph 3, that "if the under-side of the profile is free of roughness, this would make no difference to the decreased circulation".

Whilst I am unconvinced of the validity of Hoerner's correlation, I believe that, if it is to be used at all, one has to be guided by the author's suggestion that the lift curve slope is a function only of the total profile drag coefficient, irrespective of its origin.

If one rejects the latter condition, and I should agree that there are grounds for doing so, one must automatically reject Hoerner's method.

On the question of the influence on lift curve slope of the chordwise extent of the roughness, I disagree emphatically with Professor Gersten's assertion in his paragraph 6 that "even if the first 10% of the wing profile were free of icing, this would in all probability make no difference to the results, since the flow processes at the trailing edge are decisive for the decrease in lift". (There is hardly any need for me to draw attention to the fact that it has been agreed that the first 15% of the Elizabethan profile was free from deposit).

Quite apart from the fact that there is incontrovertible experimental evidence, such as that contained in the book by Abbott and von Doenhoff, 'Theory of Wing Sections', to show that leading edge roughness exerts a powerful influence on the lift of a wing, I fail to follow Professor Gersten's implied argument that the state of the boundary layer at the trailing edge is uninfluenced by conditions near the leading edge. The development of the boundary layer on the upper surface of a wing is dependent on the history of the flow over that entire surface, and is especially sensitive to any imperfections near the leading edge where, under conditions of non-zero lift, (and even at zero lift, in the case of the NACA 65_2 - 415 section) the largest velocities and chordwise velocity gradients are to be found.*

I cannot therefore understand the reasoning underlying Professor Gersten's claim that the state of the boundary layer at the trailing edge can be considered independently of conditions near the leading edge, and I adhere to the conviction that a wing free from roughness over the forward 15% of its upper surface must experience a larger lift and a lower drag than one having that surface completely roughened.

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* An illustration of the way in which a leading edge protuberance can communicate an effect to the whole of the boundary layer is given in the theoretical work of J. F. Nash(NPL Aero Note 1036, October 1965) who calculated the magnification of the drag due to the protuberance by the downstream development of the boundary layer.

Hence, the evidence that the leading edge of the Elizabethan was unblemished by ice or snow, as accepted by both German Commissions of Inquiry, cannot be dismissed as insignificant. Indeed, if the Hoerner method is employed to estimate lift curve slope from the value of the profile drag, that drag must logically include an allowance for a comparatively smooth surface over the front 15% of the wing.

Furthermore, in applying the lift curve slope thus deduced to the Elizabethan wing, one must take proper account of both the finite aspect ratio of the wing and the fact, referred to in (i) above, that only 45% of the upper surface of the wing could have had a snow or ice deposit on it. In view of the latter, any change in lift curve slope must be about one-half that appropriate to a fully-roughened upper surface; alternatively, if Hoerner's method is used, the profile drag is approximately halved.

In relation to the above argument, I confess to being mystified by the third and fourth sentences of Professor Gersten's paragraph 7. I argue that if approximately half the wing upper surface is roughened, its lift curve slope cannot be far from the mean of those of a completely smooth and a completely roughened wing. If I interpret Professor Gersten's remarks correctly, he maintains that, starting from a completely roughened wing, any allowance for the more lift-productive smooth part must be based on the smaller span, hence reduced aspect ratio, of that part. Besides the fact that one could equally well use the argument in reverse and start from a wholly smooth wing, in which case the lift decrement from the rough part is reduced by its smaller aspect ratio, the whole concept appears to me to defy the principles of finite wing theory.

(iii) Estimate of profile drag

Professor Gersten describes in his paragraph 5 how the profile drag coefficient for the Elizabethan aerofoil section was obtained from accepted values of the drag coefficient for a roughened flat plate. He applied a factor 2.86 to the flat plate value, and then added a profile drag coefficient of 0.0037 to account for "additional form drag".

In the first place, one must be clear about the interpretation of the factor 2.86. (In the original Report by Professor Schlichting and Gersten its value appears to have been 2.25; see their figure 13). It arises partly from the increase in both frictional and form drags on the aerofoil above those for the flat plate, due to the existence of larger velocities, accompanied by larger velocity gradients, on the surface of the former; and partly from the conversion of a drag on one surface of a flat plate to drags on both surfaces of the aerofoil.

When the factor 2.86, which I am prepared to accept, is used to deduce drag coefficients corresponding to those in Professor Gersten's table, starting with $C_f = 0.00275$ for a smooth flat plate with a wholly turbulent boundary layer, one obtains $C_{Do} = 0.0079$ for the smooth aerofoil, as does Professor Gersten. But, for a rough aerofoil, with $k_s/\ell = 10^{-4}$, the fact that only one surface was roughened leads to an estimate of 0.0110 for its profile drag coefficient; (i.e. $\frac{1}{2} \times 2.86 \times 0.00495 + \frac{1}{2} \times 0.0079$, where $C_f = 0.00495$ for the rough plate given in Professor Gersten's table) whereas Professor Gersten quotes 0.0142. Thus, his ΔC_{Do} of 0.0063 is reduced to 0.0032 - that is to say, just halved.

Secondly, Professor Gersten adds a further 0.0037 to the profile drag for reasons which I do not understand. He claims in his paragraph 5 that 0.0037 allows for the form drag developed on the curved surface of the aerofoil additional to its frictional drag. Surely this effect has already been included in the factor 2.86, which otherwise would have been around 2.2 if the excess velocity on the aerofoil were alone accounted for. Moreover, I am unconvinced by the further argument that the 0.0037 might cover the possibility of boundary layer separation from the aerofoil surface. Separation either occurs, or it does not; and it seems to me that a 'contingency' value of 0.0037 is purely arbitrary and completely unjustified in any self-consistent calculation scheme.

Thirdly, in paragraph 6, Professor Gersten dismisses the importance of the unroughened condition of the leading edge. I cannot agree with him, for the reasons already given in (ii) above. Since the neighbourhood of the leading edge contains, even at zero lift, the highest stream velocities of any part of the upper surface of the NACA 65_2 - 415 aerofoil section as used on the Elizabethan, any roughness there would exert a disproportionately large effect on drag. Conversely, the absence of roughness may be expected to lead to a reduction in ΔC_{Do} in a proportion greater than the ratio of the length of smooth surface to the total chord. However, to err on the conservative side, let us take the reduction to be simply in the ratio of those two quantities, namely 0.15 : 1. It follows that the estimate of drag increment must be reduced from 0.0032 to 0.0027.

We must now turn to paragraph 7 of Professor Gersten's comments. I have already referred in (i) to his insistence on treating the wing as if the entire upper surface were roughened, and have pointed to the conclusion, reached by the re-opened Inquiry, that only 45% of the area could be assumed to have been affected by deposits of snow or ice. I can find no grounds for departing from that conclusion and, in consequence, insist that the profile drag increment for the whole wing must be 0.45 times the ΔC_{Do} estimated above; that is to say 0.45×0.0027, or 0.0012. (Originally, I took half the wing to be covered, which would merely change the ΔC_{Do} to 0.0013).

It follows that, by adopting the method of estimating used by Professor Gersten, one arrives at a value for the profile drag increment about one-eighth the magnitude of his.* The corresponding proportion in the case of the larger roughness is somewhat greater than one-eighth.

I believe that the foregoing argument demonstrates that Professor Gersten's estimate of the profile drag increment can be too large by a factor of eight, contrary to the statement made in the final sentence of his paragraph 8.

If one now follows the Schlichting-Gersten procedure and estimates the sectional (two-dimensional) lift curve slope by Hoerner's method, and then applies the usual correction for finite aspect ratio, the following values are obtained for the ratio of the lift curve slope of the Elizabethan wing, with roughness equivalent to $k_s/\ell = 10^{-4}$, to that of the smooth wing;/ in the absence of ground effect:

$$\Delta C_{Do} = 0.0013; \quad \frac{dC_L}{d\alpha}\text{ ROUGH}/\frac{dC_L}{d\alpha}\text{ SMOOTH} = 0.98$$

$$\Delta C_{Do} = 0.01; \quad \frac{dC_L}{d\alpha}\text{ ROUGH}/\frac{dC_L}{d\alpha}\text{ SMOOTH} = 0.885$$

Even if one questions the accuracy of estimating the effect of the profile drag increment on lift curve slope according to the Hoerner method, there is no question at all that the lower value of ΔC_{Do} is the one appropriate to the drag estimate required for a take-off calculation.

To consider now Professor Gersten's paragraph 9, I readily admit that a calculation of the effect of ice or snow in terms of an equivalent sand roughness must contain an uncertainty. Having said that, I am impelled to point out that my criticism of Professor Gersten's calculations was not concerned with the estimate of the roughness height of

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*In my evidence submitted to the second British Reviewing Body, I stated that my estimates of ΔC_{Do} lay between one-eighth and one-quarter of the Schlichting-Gersten values. The larger figure arose from an attempt to set an upper limit to the profile drag increment and involved considerations which Prof. Gersten does not refer to in his comments.

/If figure 15 of the Schlichting-Gersten Report is examined, it appears that, for the estimated ΔC_{Do} of 0.01 the ratio $(dC_L/d\alpha)$rough/$(dC_L/d\alpha)$ smooth corresponding to their Curve II (rough wing) and Curve I (smooth wing) is 0.83. I am unable to account for the entire discrepancy between that value and the 0.885 quoted above. If ground effect - is taken into consideration, the ratio 0.885 is merely re reduced to about 0.88. If the Hoerner curve is used purely as a correction for the effect of roughness to an experimentally determined lift curve slope for the smooth wing, the above ratio, including ground effect, becomes 0.86.

any deposit that might have been present on the wing of the Elizabethan during its final take-off run (although I believe that such estimate is open to a separate question), but with the consequences to profile drag and lift curve slope of the roughness heights assumed by him.

Finally, I wish to draw attention to what seems to me to be a point that is quite crucial to any estimate of the drag increment, and one to which I can find no reference in any of the official reports published so far; it is also pertinent to the suggestion made by Professor Gersten in his paragraph 10. The datum condition, that of the Elizabethan free from ice, has been described as 'smooth'. In all my experience, no aeroplane in service can be assumed to approach closely such a condition. In practice, imperfections, such as protuberances, asperities and waviness in the metal skin, are invariably present, and the actual profile drag is appreciably larger than that estimated on the basis of supposing the surface to be smooth. For example, the experimental data on the NACA 64_2 - 415 section, contained in the book *Theory of Wing Sections* by Abbott and von Doenhoff, indicate a minimum drag coefficient of about 0.004 for the smooth aerofoil. Yet, none of the experts concerned with estimating the drag coefficient for the Elizabethan aeroplane have suggested so small a value; and with good reason, amounting to an acknowledgement that the surface of the aeroplane, even when unimpaired by ice or snow deposits, could not have been without blemish. In fact, implicit in all the estimates is the assumption that surface imperfections were just sufficient to cause transition from laminar to turbulent boundary layer flow to occur near the leading edge, and any further effect on the boundary layer - such as would lead to an increase in profile drag - has been ignored. Hence, any drag additional to that experienced by a smooth wing with transition at the leading edge has been attributed entirely to the ice deposit. Whilst I should agree that it is difficult to proceed in any other way, in the absence of data on the particular aeroplane in question, I consider that it is important to recognise the calculation procedure that has been adopted and to be fully aware of the imponderables it contains.

In view of the difficulty of defining a 'datum' condition for the aeroplane, I have to treat with caution the suggestion put forward by Professor Gersten in his paragraph 10. At first sight, the proposition that wind tunnel experiments would serve to settle, once and for all, the arguments about the effect of roughness on the drag and lift of a NACA 65_2 - 415 wing appear to be eminently reasonable and would be very acceptable as an academic exercise, but we must ask ourselves what relevance such data would possess with regard to the Elizabethan accident. For it must be recognised that we do not know, or ever hope to know, the datum condition of the aeroplane; nor are we sure about the thickness and effective roughness of any ice or snow that was present on its wing.

However, if general agreement could be reached on the geometrical features of the ice deposit (and they must be specified with precision, including a definition of the areas thus affected, as well as a careful description of the height and density of the roughness as well as the effective height of the ridge formed at the leading edge of the deposit) I should accept that experiments at the appropriate Reynolds number would at least inform us of the effects of that particular roughness on lift and drag, and illuminate such questions as the validity of the Hoerner method.

On the other hand, if the experiments were to generate arguments like those presented by Professor Gersten in his paragraphs 6, 7 and 9, I can see no value in performing them.

October 1968

APPENDIX 7(e)

Comments by Mr. E. C. Maskell on Appendix 7(b)

In paragraph 2 of his reply, Professor Gersten draws attention to the fact that the relation (fig.9 of Report 58/21) between lift-curve slope and profile drag derived by Hoerner refers to profiles roughened on both surfaces. Following a brief appreciation of the physical nature of the roughness effect on lift he then goes on to argue, in paragraph 3, that the boundary-layer development on the lower surface of an aerofoil makes little contribution to the reduction of circulation and that, if anything, a smooth lower surface would result in a further decrease of lift. He therefore concluded (paragraph 4) that if Hoerner's correlation is to be applied to a wing with a smooth lower surface, the appropriate profile drag coefficient is not the true value, but that corresponding to a roughened lower surface.

Up to this point Gersten's argument has some merit. The boundary-layer effect on lift is related to the difference between the boundary-layer development on the two surfaces of the aerofoil, whereas the profile drag is related to their sum. A correlation of lift curve slope against profile drag (i.e. in effect, against the sum of the upper and lower surface momentum thicknesses at the trailing edge) makes sense only if the asymmetry in the boundary-layer development (in the main, the difference between the upper and lower surface displacement thicknesses at the trailing edge) is itself a unique function of the profile drag. I am inclined to agree that the implication of this argument is that the profile drag employed in the use of Hoerner's correlation must include some allowance for roughness on the lower surface of the aerofoil, whether it is actually present or not. In my view, however, Gersten's remaining comments are all without substance.

In paragraph 5 he outlines the procedure adopted for the estimation of the profile drag of a fully roughened wing, which was not previously in dispute. But with the more detailed description now available it seems to me that even this procedure is open to criticism. It appears to involve two important misconceptions. Firstly, he supposes that the factor by which the flat-plate drag must be multiplied is independent of roughness height, whereas one would expect the effect of pressure gradients to become proportionately less significant as the influence of roughness become progressively more dominant. Secondly, he supposes the factor to relate solely to the skin-friction contribution to the drag, whereas such a factor can be plausibly constructed only for the profile drag as a whole. The flat plate is a special case for which the profile drag is wholly due to skin friction. In general, it includes contributions due to skin-friction and form drag (what Gersten calls pressure drag), but both are included in the drag deduced from the boundary-layer state at the trailing edge of an aerofoil, and the two components are not readily separated one from the other. In consequence, it is the total profile drag that is the more fundamental quantity in the theory of boundary-layer effects on drag, and therefore, the total profile drag that must be supposed given by an amplification factor of the kind used by Gersten.

In my view, therefore, the amplification factor of 2.86 is unreasonably large for a rough aerofoil, and there is no justification whatever for the addition of a further form drag coefficient of magnitude 0.0037.

Paragraph 6 amounts to a contradiction of everything that is known about roughness effects on boundary-layer growth. It is the state of the boundary layer at the trailing edge of the wing that is decisive in relation to the decrease in lift. The effect of roughness on the boundary-layer growth depends on the ratio of roughness height to local boundary-layer thickness, and therefore is most decisive near the leading edge where the boundary layer is thinnest. So the first 10 per cent of the wing profile is all-important.

The logic of the argument in paragraph 7 escapes me. In so far as I can follow it, it seems to imply that if half the span were free from ice the lift reduction would be halved also - hardly 'a minor alteration regarding decreased lift'. The reference to paragraph 3, which was concerned only with whether the lower surface should be considered rough or smooth, is irrelevant.

The argument in paragraph 8 has been largely dealt with above. In my view there is no justification for any additional allowance for pressure drag due to roughness. Moreover, since this unwarranted additional drag largely balances the allowance for some roughness on the lower surface (accepted in consequence of the argument in paragraph 3) and since, in my view, the remaining criticisms are indeed reinforced by Gersten's own reply (paragraphs 5, 6, 7, 8) it seems to me that an error approaching a factor of 8, at low incidence, still holds.

The argument in paragraph 9 misses the point of the criticism, which was that the Schlichting-Gersten calculations were grossly in error, even accepting their assumption concerning the equivalent roughness of the icing. It is true, of course, that this correlation is unknown. But since the true correlation must depend on the conditions under which the icing forms, there is no way of resolving the uncertainty by experiment. In any event, all the calculations of the icing effect have used essentially the same estimate of the equivalent sand roughness, which is biassed - probably heavily - in favour of the roughness effect, precisely because of the admitted uncertainty. So if this particular assumption is to be called in question, it seems to me that the only reasonable conclusion to be drawn is that all the estimate of lift loss - including those of Young, Owen and myself - are most probably too large. For example, if the ice were relatively smooth, and wavy rather than rough, it could even result in an increase, rather than a decrease, of lift.

The contrast drawn in paragraph 10 between the extensive slush experiments and the lack of corresponding work on icing effects is easily explained. British technical opinion at the time of the Munich accident, and ever since, has held that the data already available was sufficient to allow the probable order of magnitude of the icing effect to be estimated - provided that it would be represented as a roughness effect on boundary-layer development. Moreover, the estimates that were made - notably by Professor Young - did not suggest that icing might have been a prime cause of the accident. On the other hand, the information available on slush effects was considered to need amplification - an assessment that has been amply justified by the success of the experimental programme.

There is no doubt, to my mind, that if the Schlichting-Gersten calculations could be defended as a technically valid interpretation of the available data, a strong case could be made for further research on roughness effects on lift. But four independent attempts to estimate icing effects on the Ambassador are now known to exist, three of them - by Gersten, Young and Owen - performed shortly after the accident, and based on essentially the same basic assumptions and essentially the same background data. Of these three, two - those by Young and Owen - are in close agreement. The third - Gersten's - has been criticised more on matters of fact than assumption (the experimental evidence leaves little freedom of choice concerning the questions discussed by Gersten in his paragraphs 5, 6, 7 and 8) and, especially in the light of paragraphs 5-8 of Gersten's reply to the criticism, is demonstrably wrong. It therefore seems to me to have been established beyond reasonable doubt that, given the basic premises adopted by Schlichting and Gersten, the correct estimate of the effect of the supposed distribution of icing is that determined separately by Young and Owen, and that there are no grounds for believing that these estimates could be varied substantially without serious misinterpretation of the available data.

The fourth estimate - my own - is based on the contention that the available data can be interpreted differently, in particular, that only data obtained for the aerofoil section of the Ambassador need be used. But this, too, tends to confirm the Young-Owen conclusion as to the probable magnitude of the icing effect.

It seems to me, therefore, that a purely technical case for further study of roughness effects on lift cannot be made on the strength of the technical evidence of uncertainty presented to the second British Inquiry into the Munich Accident. Moreover, if such a study were to be undertaken, it would be neither simple nor modest in effort and cost since, to be convincing, it would almost certainly have to be assigned to the 8 ft x 8 ft wind tunnel at Bedford; new, accurately made, model aerofoils would have to be constructed; and extreme care, and much repetition, would be needed before reliable measurements of the relatively small force increments could be claimed.

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