Trinidad resulted from a single introduction from Jamaica in 1942, which has not hitherto been reported upon.

I was fortunate in being able to visit Jamaica in 1942, and, with the kind co-operation of Mr. W. H. Edwards, Government entomologist in Jamaica, was able to collect adults of this predator, which were then sent to Trinidad by air express. In September 1942, on my return to Trinidad, liberations were made in banana fields heavily infested with the banana weevil borer. Subsequent recoveries of larvæ and adult beetles have been made on several occasions, the most recent in November 1945, and there can be no doubt that this beneficial predator is definitely established in Trinidad. It is uncertain how far the beetles have spread from the point where the original liberations were made, and it is too soon to predict what benefits are likely to accrue from their establishment. A fuller account of this work will be published elsewhere.

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[1] Adamson, A. M., *Trop. Agric. Trinidad*, **13**, 62 (1936).
[2] Callan, E. McC., *Bull. Ent. Res.*, **34**, 313 (1943).
[3] Edwards, W. H., Rep. Dep. Agric. Jamaica, 1939-40, 18 (1940).
[4] Edwards, W. H., Rep. Dep. Agric. Jamaica 1941-42, 9 (1942).
[5] Lever, R. J. A. W., *Agric. J. Fiji*, **11**, 108 (1940).
[6] Lever, R. J. A. W., *Agric. J. Fiji*, **15**, 45 (1944).

Nature of Time

Certain experiments carried out recently and detailed below appear more likely to be of importance in connexion with the study of time than that of mental processes.

Following the general methods of J. B. Rhine[1], 24 playing cards were taken from a normal pack, being 1 to 6 in each of four suits. These were shuffled and placed in a pile, face-down. Each one was then taken in turn and its 'value' (1–6) guessed quickly and the guess written down. After 24 guesses the actual values were recorded in the adjacent column.

Three scores were noted for each set of 24, recording the number of guesses which agreed with the card then held, the number which agreed with the card previously held and the number which agreed with the card next to come. These three scores will be referred to as *A*, *B* and *C* respectively. Those of type *C* are of particular interest.

The experiment was repeated for 114 sets of 24, and the 100 sets from Nos. 15–114 are now considered. It will be noted that there are only 23 possible cases per set for the *B* and *C* scores.

Total scores—100 sets

	n_{24}	*A*	*B*	*C*	pn_{24}	pn_{23}
	2400	385	374	452	400	383·3
Deviation		− 15	− 9·3	+ 68·7		
σ		18·26	17·87	17·87		
$\frac{\text{Deviation}}{\sigma}$		< 1	< 1	3·84	(chance, say, $\frac{1}{16{,}000}$	

Score frequencies—100 sets—*C* only.

Scores	0	1	2	3	4	5	6	7	8	9	10	11	12
Actual frequency	2	4	11	18	18	16	13	9	7	—	—	1	1
Theoretical (binomial)	1·5	6·9	15·3	21·4	21·4	16·3	9·75	4·74	1·89	0·63	0·17	0·04	0·008

	⩽ 6	⩽ 7	⩽ 8
Actual frequency	31	18	9
Theoretical frequency	17·23	7·48	2·74

In spite of low aggregates, the higher scores under *A* and *B* are as follow:

	⩽ 6	⩽ 7	⩽ 8
A	21	11	6
Theoretical	19·94	9·1	3·53
B	15	8	5
Theoretical	17·23	7·48	2·74

Theoretical frequencies are based on a binomial distribution, which occurs if the memory of the experimenter so arranges the guesses that each time there is a complete set of 4 × 6. In practice, the position falls between this extreme and the limit of the normal distribution—which would give lower theoretical values for the high scores.

As a further observation on the *C* scores, the last guess of each set was compared with the first card of the following set, giving:

Correct: 13 out of 99. $pn = 16{\cdot}5$, deviation $= -3{\cdot}5$, $\sigma = 3{\cdot}71$.

Compare these figures with blocks of 115 from the full experiment (*C* scores only) $pn = 19{\cdot}17$, $\sigma = 4$.

Sets	*C*	Sets	*C*	Sets	*C*	Sets	*C*
15–19	20	40–44	20	65–69	20	90– 94	27
20–24	29	45–49	20	70–74	18	95– 99	19
25–29	26	50–54	23	75–79	25	100–104	20
30–34	19	55–59	24	80–84	27	105–109	27
35–39	19	60–64	20	85–89	26	110–114	23

Set No. 65 is included as *A* 2, *B* 3, *C* 0, but it was found that if guesses were compared with the next-card-but-two the score was 9 ($pn = 3{\cdot}83$, $\sigma = 1{\cdot}79$).

The experiments were carried out between August 1 and November 7, 1945, in 14 separate batches. They were not witnessed. Arrangements could be made for inspection of original data in London.

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[1] Rhine, J. B., "Extra-Sensory Perception" (London, 1935).

Planck's Radiation Formula: a Correction

Since passing the proofs of my letter which appeared in *Nature* of April 20* I have found, as a result of a conversation with Dr. G. J. Whitrow, that the conditions prescribed for the radiation formula admit of a more general solution than that of Planck. Any function of $\xi = \nu\psi^{n-3/4}$ which satisfies the equation

$$\int_0^\infty g(\xi)d\xi = 1$$

meets those conditions. It may be that dimensional or other considerations will be found to impose somewhat severe limits on the form of the function, and it would be possible to invoke the (purely macroscopic and continuous) Wien's law to limit n to the value 1/2, but at the present stage the possibilities appear to be too numerous to justify the statement that Planck's law has been deduced on macroscopic grounds. This statement must therefore be withdrawn.

What the work does is (1) to impose a new condition on the form of the radiation formula which rules out the Rayleigh–Jeans law ($g(\xi) = G\nu^2$, $n = \frac{3}{4}$) and with it the classical atomic theory of which it is a necessary consequence; (2) to admit the Planck formula as a possibility on grounds of continuity and so to invalidate the remark of Poincaré's referred to in the former letter; (3) to support the postulate of the thermal relativity theory that the 'frequency' of light is subject to the transformation law for thermal time. The penultimate paragraph of the former letter concerning microscopic and macroscopic laws, though I believe it to be true, can no longer find strong confirmation in the present work, but the last paragraph remains unaffected.

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April 16.

* *Nature*, [**157**, 515 (1946)].

Meteorological Forecasts for Special Purposes

In the article in *Nature* of February 23 on "Weather and Electric Power Systems", Mr. J. S. Forrest raises a matter which, I believe, merits close attention. I refer to the general application of his particular suggestion that electricity supply authorities should employ meteorologists with special knowledge of the requirements of the organisation concerned.

Since arriving back in Britain at the beginning of the year, I have heard many complaints of the official daily weather forecasts. Six years of war-time experience with the Naval Meteorological Service leads me to believe that the basis for these complaints is similar to that commonly encountered in the Services. The essence of this is that it is far easier to make accurate forecasts for specific purposes and for specific users than to make general statements for a wide variety of users. One found extreme instances of this during the War, when senior officers, for security reasons, were unwilling to take their meteorological officers fully into their confidence. The general information vouchsafed to the latter, in these cases, as to the venue of an operation was so misleading that their forecasts were to the recipients even more misleading. It was repeatedly shown, on the other hand, that a meteorological officer serving in a ship, and having full mutual confidence with his colleagues and understanding of their problems, could give general satisfaction whereas wide statements issued from a base meteorological office inspired little confidence.

I suspect, however, that it is not only the nature of the problem itself, but also that of personnel. In war-time, there was employed a large band of 'amateur' meteorologists. These were of varying calibre, but it is safe to say that a high proportion were successful forecasters who were 'amateur' only in the sense that they would not have cared to accept employment in a State meteorological service. At the same time, they were often men of wide interests who, it must be said, sometimes had a better understanding of the users' requirements than had many 'professional' meteorologists. In fairness to the 'professional' meteorologists (that is, those permanently employed by the State service), it must be said that there were standards below which they rarely fell, whereas there were inevitably a number of misfit 'amateurs' who proved themselves quite untrustworthy.

Since practical meteorology inevitably involves a large-scale observing and communications organisation which is normally suited only to Government sponsoring, there is a latent danger in every country of initiative being tempered too much by over-attention to routine and 'red tape'; promising meteorologists have been lost to the science for this very reason.

Mr. Forrest's suggestion for co-operation between 'private' meteorologists and the State service would be of great value to both, and to the science itself. It would encourage attention to aspects which have received relatively little attention, and it would probably lead to the development of a type of talent which does not take kindly to the general State service. Not only electrical authorities but also agricultural organisations, harbour boards, transport concerns, building contractors, etc., could well consider the value of instituting special meteorological departments, and in particular of tapping the sources of demobilized talent which will soon become available.

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