pecked very slowly, and frequently raised her head and stretched her wings as if partially narcotised. This experiment was repeated on the cock, but I could not detect any indications of narcosis. The saucer was filled with hot dilute sulphuric acid, into which an ounce of powdered cyanide of potassium was thrown. The evolution of prussic acid was so violent that I considered the neighbourhood unsafe. My gardener, who was working thirty yards away, spoke to me of the "smell of almonds." For some minutes the cock turkey fed with his usual eagerness; then, suddenly, he began to stagger round the enclosure, crossing his legs and holding his beak straight up in the air. He made his way back into the pen, where he stood with head down and wings outstretched. After ten minutes he returned to the enclosure, but did not eat any more grain. His comb and wattles were deeply suffused with blood.

In all observations on the sense of smell of animals we have an obvious difficulty to face. There is no reason for supposing that an animal enjoys an odour which pleases us or dislikes one which we find disagreeable. My dog appeared to be almost indifferent to bisulphide of carbon. He showed, however, great repugnance to chloroform and prussic acid. It is difficult to think that an animal which is unable to protect itself from the injurious effects of such drugs as these can possess the sense of smell.

I shall be very grateful to any of your readers who will give me information on this subject. Especially should I be glad to learn something about the habits of wingless birds, the mode of life of which, more or less, resembles that of a terrestrial mammal. In them, if in any birds, it would seem likely that the sense of smell would be efficient. In his memoir on the Apteryx, Owen stated that "the relative extent and complexity of the turbinated bones and the capacity of the posterior part of the nasal cavity exceed those of any other bird; and the sense of smell must be proportionately acute and important in its economy."

Downing College Lodge, January 26. ALEX. HILL.

The Origin of Radium.

IN the issue of NATURE for January 26, Mr. Soddy describes the present position of his experiments on the production of radium from compounds of uranium, and announces a positive result.

Since I wrote on May 5, 1904, pointing out that, on the theory of Rutherford and Soddy, the quantity of radium developed by a few hundred grams of uranium should be measurable in a few months, a quantity of about 400 grams of uranium nitrate has been preserved in my laboratory.

I am not yet prepared to give definite quantitative results, but Mr. Soddy's announcement may perhaps excuse a preliminary statement that the quantity of radium emanation now evolved by my uranium salt is distinctly and appreciably greater than at first.

A rough calculation of the rate of growth of radium indicates a rate of change far slower than that suggested by the simplest theory of the process, but somewhat quicker than that given by Mr. Soddy, who finds that about 2×10^{-12} of the uranium is transformed per annum. As Mr. Soddy says, it is possible that the total amount of emanation is not secured, and the fraction obtained may depend to some extent on the particular method used by each experimenter. But another possibility should be borne in mind. If a non-radio-active product, intermediate between uranium and radium, exists, the rate of appearance of radium would be slower at first, and quicker as the experiment proceeds. My uranium salt was not purified so successfully as that used by Mr. Soddy, and, when the first measurement was made a month or so after preparation, the yield of radium emanation was appreciable. It may be that Mr. Soddy is tracing the process from its inception, and that I have started at a later stage, where the rate of formation is somewhat greater. Further observation may be expected to elucidate these and other questions.

W. C. D. WHETHAM.

Cambridge, January 30.

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Fact in Sociology.

I ADDRESSED a letter to the editor of NATURE replying to what I allege to be misrepresentations and misstatements in a review of three of my books by "F. W. H." (December 29, 1904, p. 193). After a delay of some weeks due to the absence of "F. W. H." abroad, the editor of NATURE has written to ask me to modify and shorten my protest.

"F. W. H." told the readers of NATURE that my "Food of the Gods" "claimed to forecast the future." This was untrue, and I said so.

"F. W. H." mixed up my discussion of probabilities in "Anticipations" with my general review of educational influences in "Mankind in the Making," and presented this as my ideals. I pointed out that this was an unsound method of criticism.

"F. W. H." presented the following as my opinions:—"Germany will be cowed by the combined English and American Navies, and Anglo-Saxonism will eventually triumph. There remain the Yellow Races. Their star, too, will pale before that of the Anglo-Saxons." I repudiated this balderdash with some asperity. It is violently unlike my views.

He wrote of me, "he seems unaware of the part in the national life that is played by the lower stratum of society, the 'stagnant' masses as he would call them." I denied that I should, and pointed out that no one does know what part is played by any stratum of society in national reproduction. It is a field of unrecorded facts. I commented on "F. W. H.'s" assumption that he was in possession of special knowledge.

He wrote of "the fact that this stratum is an absolute necessity." This is *not* a fact. It may or may not be true. I commented on this use of the word "fact" in view of "F. W. H.'s" professorial sneer at my "imagination unclogged by knowledge."

He declared that I want to "get rid of the reckless classes, and depend solely on the careful classes," a statement which has not an atom of justification. He not only "guys" my suggestions, but foists an absolutely uncongenial phraseology upon me.

Finally, he wrote, "we are to introduce careful parentage, *that is*, put a stop to natural selection." I quoted this in view of his statement that I had "no very thorough grasp of the principles of evolution." I discussed what appeared to be his ideas about evolution. They appeared to me to be crude and dull, and I regret I cannot condense my criticisms to my present limits.

I expressed some irritation at his method of misstatement followed by reply, and hinted a doubt whether my own style of inquiry—in spite of the fact that romances blacken my reputation—was not really more scientific than his.

H. G. WELLS.

The Fertilisation of *Jasminum nudiflorum*.

THIS well known plant, in accordance with its usual habit, has been flowering in my garden at Stonehaven, Kincardineshire, since the third week in December, 1904, and amidst frost and snow and cold winds. There are no leaves, but there are thousands of bright yellow flowers. It is a puzzle to me how fertilisation is effected. The two stamens are situated about half-way down the tube of the corolla, and about four or five millimetres below the style, which is, in many cases, two millimetres longer than the tube of the corolla. It seems to me to be a plant requiring the aid of insects in its fertilisation, but there are no insects to be seen at this time of the year. On January 22, as there was some sunshine, I watched the plant for about four hours, but no insect paid it a visit. At the same time I found the oblong anthers had split and pollen grains were sticking to the stigma in many flowers. The brilliantly coloured flowers, although destitute of scent, are fitted to attract insects, and the form of the flower seems adapted for their visits. But there are no insects! Can anyone offer an explanation? The plant is beautifully figured in the *Botanical Magazine*, lxxviii., tab. 4649.

JOHN G. MCKENDRICK.

University of Glasgow, January 24.