WE must all agree with Sir Edward Fry's desire to obtain a clear and exact definition of an "acquired character," as this term has been used in the discussions upon hereditary transmissibility. I do not think, however, that those who have taken part in the various controversies and discussions which have raged intermittently during the last seven years, have been misled by the lack of a sufficiently exact definition or the multiplicity of inexact ones. I believe that both sides have known well enough the kind of character which was called acquired, even though no sufficiently clear definition was forthcoming. And it may be that this mutual understanding has tended to obscure the demand for a definition.

An acquired character has generally been briefly defined as "the result of the operation of some external force upon an organism," and I still think that this is as satisfactory as any definition of equal brevity can be. But some want of clearness follows from the elasticity of the word "result." Everything that follows the operation of some external force may be called its "result"; but the definition interpreted in this way would include much that is not within the meaning of the word "acquired." Some increased precision may be added by using the words "direct result"; but a perfectly satisfactory definition should, I think, imply the admission that the result (in its wide sense) of an external force on an organism must always contain elements which are not due to the force—which are not acquired—as well as those which are due to the force and which are acquired. I think that the following definition will meet the case: "Whenever an organism reacts under an external force, that part of the reaction which is directly due to the force is an acquired character."

In many cases the external force acts only as a shock, with the *starting* of reaction as its only direct result. In such a case the occurrence of the reaction, as contrasted with the sequence of events which make up the reaction itself, is the acquired character. In examples such as these, those who maintain the transmission of acquired characters would be required to prove that the reaction which could only be started by an external force in the parent, started without this stimulus in the offspring.

I believe the definition suggested above meets all Sir Edward Fry's conditions—viz., that it includes all "acquired" characters, and excludes all that are not acquired; that it is physical and not metaphysical; that it is not "stated in terms derived from hereditability or the contrary, or in terms of any hypothesis or theory"; and that it admits of ascertainment and verification.

That a reaction under an external force is compounded of two parts, due respectively to the body which reacts, and to the force which causes the reaction, is a fact and not a theory or hypothesis. It may be urged, however, that the separation of the two constituents does not admit of "ascertainment and verification." This may be true, in the present state of our knowledge, for certain cases; and if so, these cases would be unsuitable for the purposes of an inquiry into the transmissibility of acquired characters. But I do not admit that it is proved that the two constituents of the reaction cannot be separated in every case by a sufficiently careful investigation. For the purposes of this inquiry it is sufficient, however, if we can prove beyond doubt that some part of a reaction is the direct result of an external force, even if we have not thereby exhausted the whole of the direct results contained in the reaction. For if this can be done in a vast number of cases, an immense body of evidence will be provided, and we may expect that, if acquired characters are transmissible, some proof will be forthcoming.

I propose to test the efficiency of the definition given above, by showing how it can be applied to some of the examples given in Sir Edward Fry's letter.

In the case of the "exercierknochen" it is clear that the *occurrence* of the reaction—the existence of the bony growth—is the direct result of the external force. Here then is an acquired character which will be admitted by everyone, which can be witnessed in a vast number of examples, and which can be conveniently applied to test the transmissibility of such characters. There may, or may not, be other direct results contained in the reaction: some of the processes of osseous growth may have followed directly from continuous or intermittent pressure. But in the first place the verification is much more difficult, although not, I believe, beyond the reach of scientific inquiry, and, in the second place, such proof, if obtained, would yield evidence which would be far more difficult to obtain in very large quantity.

It is clear that when Prof. Weismann admits that "the periodical change of leaf in temperate climates has been produced *in relation to* the recurring alternation of summer and winter," he is referring to the selection of inherent characters, and not the production of acquired characters. The sentences which follow the one quoted (p. 406), leave no doubt upon this point. Sir Edward Fry may feel assured that when any direct results of heat, cold, air, food, moisture, gravity, or light upon the organism are proved to be heritable, the controversy is at an end.

The case of geotropism logically resembles that of the "exercierknochen." The *occurrence* of the reaction is certainly a direct result of the external force—an acquired character; and here too we have an immense body of evidence to which to appeal, and which points only in one direction. In spite of the innumerable generations during which plants have assumed certain relative proportions under the influence of gravity, this influence is just as necessary to-day as it has ever been, and the youngest generation starts unbiassed by the direct result of external forces upon its ancestors.

As regards the "extra fingers or toes, patches of grey hair, moles, &c.," the question is raised as to whether external forces are not involved as direct causes. If this can be proved the question at issue is settled, for such characters are known to be transmissible. If not, the observation merely shows us that certain characters, not proved to be acquired, are transmissible. But if the non-transmissibility of those proved to be acquired has been established on a sufficiently large scale, then the observation in question, accompanied by the continued absence of proof that the characters in question are acquired, may be fairly held to indicate the existence of two contrasted classes of characters, which we may call spontaneous or inherent, and acquired.

We are asked if we have any scientific knowledge of the organic world independently of any external influence. This method of eliciting an answer must not be allowed to disguise, as it appears to do, the very positive knowledge we possess of the separate effects of the several external influences. This is a legitimate province of scientific investigation, and a large amount of research at the present day is devoted to such questions.

In handwriting the two constituents of the reaction are somewhat difficult, but by no means impossible, to distinguish. The external influence of training operates upon the most complex part of the organism, the nervous system, which again directs the muscular system. Is the style of handwriting due to the external force, or the organism which reacts? We can eliminate pen, ink, and paper as influences by only considering the cases in which these have been identical. There remains the influence of the teacher, and in order to prove that this has been the direct cause of style, it must be shown that the teacher had produced the same style in many pupils. If a style so produced became hereditary, evidence of transmissibility of an acquired character would be provided. Conversely, variety of style under the same conditions of teaching, &c., would favour the view that we are not dealing with an acquired character in this part of the reaction.

It is unnecessary to consider further the cases of mutilation and wounds, for I imagine that Prof. Weismann, and all who agree with him on this subject, will be willing to accept the clear statements of Sir Edward Fry's letter. "What the organism transmits is the capacity or predisposition, and not the actual result of the reaction." The latter in these cases is an acquired character, while no one has ever shown that there is any probability that the former is acquired.

I have, in this letter, avoided reference to many points raised by Sir Edward Fry, not from want of interest or inclination, but in order to keep to the main issue—the attempt to furnish a clear definition of the class of characters in question.

If acquired characters are transmissible, we must expect that sooner or later among the vast body of characters which are or will be admitted on all hands to be acquired, some valid instances of hereditary transmission will be forthcoming.

Such cases as that mentioned by Dr. Hill in NATURE of October 25, when on a sufficient scale and adequately sifted, would supply the requisite evidence. But up to the present such satisfactory evidence has not been forthcoming, although it has been sought for by many observers.

EDWARD B. POULTON.

Oxford, November 4.