do me the favour of reading my original note again, he will find that the object of my remarks was simply to test the truth of a definite assertion by Mr. Spencer that "the Second Law of Motion is an immediate corollary of the preconception of the exact quantitative relation between cause and effect." It was entirely beside my purpose to discuss the general psychological question of the formation of conceptions or preconceptions farther than as it is involved in the truth or otherwise of this particular assertion. Mr. Collier's note is therefore, as far as regards my remarks, entirely irrelevant and needs no other reply than to invite him, as Mr. Spencer declines to do so, to answer the simple and definite questions proposed by me as difficulties which Mr. Spencer is bound to answer, unless he is prepared to admit that he was wrong in the assertion on which I commented.

I have assumed throughout that Mr. Spencer means to assert that the Second Law of Motion is *involved in*, not merely that it *involves*, a particular preconception. And yet this latter is all that Mr. Collier asserts in the summing up of Mr. Spencer's argument, with which he concludes his note. If Mr. Collier truly represents Mr. Spencer, I can only say that, while the assertion may be admitted to be true, it certainly appears to me to be so trite as to be hardly worth formulating. The whole question turns on the distinction between "involving" and "being involved in," which I suppose Mr. Spencer and Mr. Collier would regard as an important one, though it is difficult in some cases to make out distinctly from their language and their line of argument which they mean to imply.

Passing in conclusion beyond the particular issue to which I have hitherto confined myself, I would remark that to my mind all that Mr. Spencer's and Mr. Collier's illustrations prove is that, while unconscious experiences (whether individual or inherited) may give rise to certain general, but (except in the very simplest cases) vague, preconceptions, it is only when these preconceptions are wedded to consciously-made observation or experiment that they cease to be barren generalities and give birth to the fruitful laws of Physical Science. To a mathematician, at any rate, it is almost ridiculous to observe how little either Mr. Spencer or Mr. Collier seem to realise the great gap between the indefinite observation that two things always increase and decrease simultaneously, and the definite conclusion that they are proportional to one another. For example, it is hardly a parody of Mr. Collier's remarks to say:—"A child discovers that the greater the angle between his legs the greater the distance between his feet, an experience which implicates the notion of proportionality between the angle of a triangle and its opposite side;" a preconception, as it appears to me, with just as good a basis as that whose formation Mr. Collier illustrates, but one which, as I need hardly add, is soon corrected by a conscious study of geometry or by actual measurement.

Harrow, May 25 ROBT. B. HAYWARD

MR. COLLIER'S letter, NATURE, vol. x. p. 43, is even more astonishing than anything that Mr. Spencer has written. A mathematician who reads it feels something like Alice behind the looking-glass; and perhaps behind the looking-glass it may be "a question pertaining to the psychological basis of inductive logic," with which mathematicians, as such, have nothing to do. But in this world, this side the looking-glass, in which forces are measured and effects are measured, Mr. Collier's letter is very perplexing.

For example, after giving several instances in which a greater force produces a greater effect, Mr. Collier proceeds: "The experiences these propositions record all implicate the same consciousness—the notion of proportionality between force applied and result produced: and it is out of this latent consciousness that the axiom of the perfect quantitative equivalence of the relations between cause and effect is evolved."

Does Mr. Collier know what proportionality means? Does any one of the experiments indicated prove that where effort is doubled the result is *doubled?* The child pulls his boat by a string through the water; if he pulls twice as hard does he pull it *twice* as fast?

It seems to me that the people on the other side of the looking-glass think perfect quantitative equivalence (however measured) means the same as proportionality; and are willing to raise first the general result of experience, that greater forces produce greater effects, into an axiom of exact quantitative equivalence (without troubling themselves to consider how quantity is to be estimated), and then to accept Newton's Second Law as an instance of this quantitative equivalence, without showing any connection between quantitative equivalence and direct proportionality in that instance or in any other.

A SENIOR WRANGLER

Ocean Circulation

MR. CROLL will doubtless be of opinion that as my "theories" show such an utter ignorance of "even the elements of physics and mechanics," I can employ my time much better in acquiring some knowledge of those sciences, than in continuing to discuss the subject with him.

I shall be glad to be allowed to state to the readers of NATURE, as I have to those of the *Philosophical Magazine* (May), other grounds on which I must decline to prolong this discussion.

1. Mr. Croll has charged me (Phil. Mag. for March, p. 177, note) with a serious misstatement in regard to the mean annual rate of the Gulf Stream, which he affirms to be *nearly double* what I have represented it. Now *my* statement was avowedly based on the *average of the whole year's observed rates;* whilst Mr. Croll has taken as the basis of *his* the arithmetical *mean* between the maximum and the minimum. It has been said in disparagement of statistics that "anything can be proved by figures;" and Mr. Croll, who is nothing if not a statistician, seems to me to justify the imputation, for the adoption of his method would make the *average* number of children of a marriage to be at least *ten!*

2. Mr. Croll, in asserting that I have left out of consideration "the fact that the sea is salter in intertropical than in polar regions, and that this circumstance, so far as it goes, must tend to neutralise the difference of temperature," has only exhibited his own ignorance of a very important fact of Ocean Physics—the *low* salinity of equatorial surface-water; which was ascertained in Kotzebue's voyage fifty years ago, has been confirmed by many later series of observations, has been repeatedly cited in text-books, and has been adduced by myself as an indication that polar water is continually ascending from the bottom to the surface under the equator. But further, not only has this fact been confirmed by the *Challenger* observations, but so remarkable an accordance has been shown by them to exist between the low specific gravity of equatorial *surface*-water and that of equatorial *bottom*-water, as strongly to indicate that, as the latter is certainly polar, the former is so also. It suited Mr. Croll's purpose, however, with these observations before him, completely to ignore them, and to state as fact what is the precise contrary of facts.

3. According to Mr. Croll and his anonymous authority, the Astronomer Royal must be unfamiliar with even "the elements of physics and mechanics;" for, speaking from the chair of the Royal Society in 1872, he explicitly expressed his acceptance of the doctrine I advocate, as "certain in theory and supported by observation." The eminent meteorologist, Prof. Mohn, of Christiania, also, who expressed to me in writing last year his acceptance of it, must be equally ill-informed; as, too, must be Dr. Meyer of Kiel, who has been engaged for four or five years past in the investigation of the physics of the Baltic, the North Sea, and their connecting channels, and who has satisfied himself so completely of the power of small differences of specific gravity to put large bodies of water in motion. I have *nowhere said* that no eminent physicist shares Mr. Croll's objections; though I have not myself met with such a one.

I regret to have been forced, by the personal attacks in which Mr. Croll has latterly thought fit to indulge, thus to retort upon him. Henceforth I shall not consider myself called upon to take any notice of assertions and arguments which I do not find to exert the least influence on the opinions of the eminent scientific men with whom it is my privilege to associate.

WILLIAM B. CARPENTER

Glacial Period

IN answering Mr. Bonney's letter in NATURE, vol. x. p. 44, I shall confine myself to the consideration of his second objection to my theory, as the precise southern limit of the glacial action is not of present importance, and the height of the Scandinavian sea-beaches is irrelevant to the inquiry.

Mr. Tiddemann, in an admirable paper On the glaciation of North Lancashire (Quart. Journ. Geol. Soc., vol. xxviii. p. 471), has mapped out the course of the ice as shown by scratched rocks, lines of transported boulders, carriage southwards of local