Crampton's ('94) observation that in a sinistral gasteropod, Physa, there is a reversal of the usual directions of spiral cleavage, is particularly interesting in this connection. If the initial asymmetry is caused in Physa, as in Crepidula, by the asymmetry of the cells 5C and 5D, then it is easy to see how this reversal of the cleavage stands in a causal relation to the reversed asymmetry of the adult. Both Crampton and Kofoid ('94) call attention to the fact that in Rabl's ('79) figures of the embryology of Planorbis there is a reversal of the usual direction of cleavage, and also that in Haddon's ('82) figures of Janthina a similar reversal is indicated. In Planorbis the asymmetry of the adult is reversed, though this does not seem to be the case in Janthina (*cf.* Kofoid ('95), p. 69).

If it should prove on further investigation that reversed cleavage always leads to reversed asymmetry of the adult, there would be good reason for believing that Crepidula exhibits the usual and perhaps the ancestral method in the establishment of the asymmetry of the gasteropods.

D. GENERAL CONSIDERATIONS.

1. *The Forms of Cleavage.*

Several different kinds of cleavage are commonly recognized: (1) with reference to its extent, cleavage is total (holoblastic) or partial (meroblastic); (2) regarding the relative size of the cell products it is equal or unequal; (3) in the distribution of yolk it is telolecithal or centrolecithal; (4) with reference to the constancy of form, it is regular or irregular; (5) with reference to symmetry it is radial or bilateral. To these five classes I think a sixth should be added, at least for the present, *viz.*, one with reference to the destiny of the cleavage cells and axes, and for which I propose the names determinate and indeterminate. It may be that future work will show that this distinction is not necessary, but in the present state of knowledge such a distinction exists and it is very useful to have a name for it. Attention is directed in this place only to the two

last-named classes, *viz.*, that having reference to the symmetry of cleavage and that having reference to its prospective value in the developing organism.

(a) *The Radial Type.*

(1) Orthoradial Cleavage. — The purely radial form of cleavage, in which a series of "meridional" furrows alternate with a series of "equatorial" ones, and in which the cleavage cells as well as furrows form zones and meridians which are perpendicular the one to the other, has long been regarded as the typical form of all total cleavage. Thus, in the earlier works on the cleavage in the frog, Amphioxus, mollusks, echinoderms, in fact most animals, it is usually stated that there is a regular alternation of meridional and equatorial furrows, and that the number of cells is doubled at every stage; accordingly it was once supposed that the entire number of cells could be accurately determined by getting the approximate number of nuclei present on one side of the egg. And even to this day there are those who lightly speak of 64-, 128-, 256-, and 512-cell stages in a way that causes one who has ever attempted a detailed study of cleavage, especially in these later stages, to stand aghast. Rauber ('82) has rendered great service by pointing out the fact that in the frog's egg all such representations are pure diagrams which have no counterpart in nature. Nevertheless these ideas of cleavage have found secure and undisputed lodgment in many of the text-books, and so the error is propagated from year to year, and from generation to generation. All such ideas of a purely radial, or orthoradial,[1] type of cleavage with the cells regularly increasing in geometrical ratio are misleading, if not absolutely erroneous. Instead of being the usual form of cleavage, this orthoradial type is exceedingly rare and is never found beyond

[1] Wilson designates as a "purely or truly radial" form of cleavage that in which "there are two systems of cleavage planes, of which one set are meridional and radially symmetrical to the egg axis, while the other set intersect the meridians at right angles." I propose for this form of cleavage the term *Orthoradial*, since the so-called "spiral" form of cleavage is just as truly and purely radial as the other.

the first few divisions. Wilson ('92 and '93) cites Amphioxus, Echinus, Synapta, Antedon, and Sycandra as typical examples of the orthoradial type of cleavage. In his work on Amphioxus, however, he says that the radial form is present in only about three-quarters of all the eggs of that animal, and that even in these the "cross furrow" may or may not be present. The presence of a polar or cross furrow is in itself sufficient evidence that the cleavage is not strictly orthoradial. Among the echinoderms I have observed that polar furrows are usually present in the eggs of Asterias and Arbacea, and I believe that some of the cases in which orthoradial cleavage is figured may be attributed to the influence of the usual teachings upon this subject. But granting that there are some eggs, as appears to be true, in which the cleavage is orthoradial up to the 8- or 16-cell stage, there is certainly no egg which preserves the orthoradial condition through any considerable part of the cleavage. This form of cleavage is not only very rare, but when found it is exceedingly evanescent and very soon gives way to spiral cleavages. Driesch ('92) has well said: "Das so oft schematisch gezeichnete Vierzellenstadium mit zwei sich in zwei Puncten schneidenden Meridianen kann man wohl getrost aus der Reihe des Existirenden streichen; vgl. hierzu die genauen Furchungsstudien von Chabry und Rauber. Das Princip der kleinsten Flächen, dessen nothwendiger Ausdruck (Plateau, Lamarle) es ist, dass stets drei Flächen in einer Kante, vier Kanten in einem Punkt zusammenstossen, scheint gerade in der Ontogenie der Thiere besonders deutlich zu Tage zu treten."

(2) Spiral Cleavage. — The word "spiral" has long been used (Selenka '81, Lang '84) to describe that form of cleavage in which there is an actual or virtual rotation of the blastomeres upon each other. Wilson ('92), however, first proposed the recognition of a *spiral type* of cleavage. "The spiral type," he says, "arises from the radial through a twisting of the radii, as it were, the blastomeres being displaced or rotated, with respect to the egg axis, either to the right, following the hands of a watch (right-handed spiral), or in the reverse direction (left-handed spiral), as the case may be.

. . . The term 'spiral' refers to the fact that the curved radii, if prolonged, would form a spiral about the egg axis." Lillie ('95), commenting on this statement, says: "In the ontogeny there is no twisting of the radii, but merely an inclination of the axis of the dividing cell from the vertical. It seems to me, therefore, that this form of cleavage would be more correctly termed *oblique*." Kofoid ('95) has also criticised the term spiral cleavage as ambiguous and misleading, and suggests as a substitute *alternating cleavage*. It seems to me, however, that the name used by Wilson is better than either of these later suggestions. Lillie's only objection to the term, apparently, is the fact that there is no twisting of the radii; in Crepidula, however, there is in the early cleavages just such a twisting of the radii as Wilson mentions, dependent upon the actual rotation of the blastomeres after they have been formed.[1] It is true that in many animals with this type of cleavage an actual rotation of the blastomeres and the consequent twisting of the radii do not occur, but in all such cases there is at least a *virtual* rotation. But the principal reason for preferring the term *spiral* to the word *oblique* is that it has long been used (Selenka, Lang, Wilson, Heymons) in one form or another to designate that kind of oblique cleavage in which the divisions are in the same direction in each quadrant, so that it has come to have in this connection a distinctly technical meaning, whereas, as commonly used, *oblique* cleavages may be in different directions in different quadrants, *i.e.*, they may be bilateral or radial or neither. The suggestion made by Kofoid merely emphasizes the alternating character of the successive cleavages, and this might be more satisfactorily accomplished by the use of the term *alternating spirals*, since alternation is as characteristic of orthoradial as of spiral cleavages. There are cases, as we shall see a little later, in which the cleavage is oblique in the same direction in each quadrant (*i.e.*, it is spiral), *but in which it does not alternate with the preceding*

[1] Almost all who have written on the cleavage of gasteropods (Fol, Rabl, Blochmann, Heymons) describe an *actual rotation* of the cells. In Crepidula this rotation is especially well shown in the formation of the first and second quartettes and in the subsequent division of the first; it is not marked in the formation of the third quartette, but is very pronounced in the separation of the fourth.

cleavage. It will not do, therefore, to lay too great emphasis on the alternation of cleavages. It must be confessed that the expression *spiral cleavage* is open to objection, since in many cases no real spiral is formed either by the spindles or the cell walls. Still, if taken apart from its colloquial meaning, the word *spiral* clearly and specifically designates a particular kind of cleavage which needs a distinctive and technical name, and it may be doubted whether any other name would not be open to as serious criticism.

On the other hand, Wilson's exposition of the spiral type of cleavage is in certain places open to objection. He says ('92, p. 378): "The cell divisions . . . show a peculiar modification of radial symmetry, which is best characterized as spiral in character, and which cannot be reduced to the bilateral type." The suggestion, here contained, that there is a third kind of symmetry, *viz.*, *spiral symmetry*, is still further borne out by the three coördinate types of cleavage which he establishes, *viz.*, the radial, the spiral, and the bilateral. That such a classification, either of symmetry or of cleavage, is unjustifiable is shown, I think, by the fact that so far from being "a peculiar modification of radial symmetry," the *spiral symmetry*, thus suggested, is one of the most common forms of radial symmetry, and likewise the "spiral type of cleavage" is by all odds the most common representative of radial cleavage. Spiral cleavages, therefore, belong entirely to the radial type, and should not be classified as coördinate with either the radial or bilateral types.

I shall limit the use of the term *spiral* to those cases in which cleavage occurs in the same direction in each quadrant, *i.e.*, it is always a purely radial cleavage. If this radial character is changed even in one out of four quadrants, it would then be better to use the term *oblique*. Oblique cleavages then might be or might not be bilateral, but they would not be radial. As we shall see later, oblique cleavages, using the term in this special sense, are transitional between spiral and bilateral cleavages.

One of the most constant and characteristic features of all radial cleavage is the alternation of direction in successive

divisions. This alternation is essentially the same in both orthoradial and spiral cleavages; in the former case, the axes of the nuclear spindles are alternately meridional and equatorial, in the latter they lie between these positions, being alternately oblique to the right and to the left.

It is a most remarkable fact that in all known cases of spiral cleavage, with the exception of a few sinistral gasteropods, the direction of the spirals is invariably the same. The full significance of this fact can only be grasped when one realizes that spiral cleavages are found in animals so far apart as turbellarians, annelids, lamellibranchs, and gasteropods.

Selenka ('81) first called attention to the spiral character of the *third* cleavage by which the first quartette of ectomeres is formed. He also observed that in the formation of the second quartette the spiral was in the opposite direction. Lang ('84) carried the spiral cleavages back to the *second* division of the egg, which he characterized as a "left-wound spiral." As a result of this spiral division, he showed that two of the macromeres lie at a higher level than the other two, and consequently two polar furrows are formed (see p. 52). These polar furrows always bear a fixed relation to the first two cleavages, because the second cleavage is constantly laeotropic.

Other investigators have recognized the spiral character of the second cleavage in many other animals, but, so far as I know, no one has suspected that the *first* cleavage also is a spiral one. This, however, is the case in Crepidula, for immediately after the first cleavage is completed, it can be seen that the first division was a *dexiotropic* one.[1] Likewise *in all animals in which the second cleavage is constantly laeotropic, it is probable that the first is virtually dexiotropic. The spiral cleavages, therefore, probably begin with the first division of the egg, and in almost every case in a dexiotropic direction, the second division is laeotropic, the third dexiotropic, the fourth laeotropic, etc.* In Crepidula these alternating spirals proceed without a break, except slight differences in the time of division in the different quadrants, from the 1- to the 44-cell

[1] See p. 42.

stage, and even after this they continue in a majority of the cells as long as the cleavage can be followed.

Kofoid ('95) has collated from the most important literature on spiral cleavage the facts as to alternation, and has presented them in a series of excellently constructed tables. In conformity with the methods used by him, there are given in the following table (pp. 180, 181) the facts as to the alternation of successive cleavages in Crepidula.

This table shows one very clear case of the reversal of an entire spiral cleavage, *viz.*, the division of the basal cells of the cross $1a^{1.2}$, etc., Gen. VII. Two other cases, not so clearly marked because the spindles are nearly meridional or equatorial, are found in the division of the cells $2a^{2.1}$, etc., Gen. VIII, and the descendents of these cells, $2a^{2.1.1}$, etc., Gen. IX.

There are many cases in which reversals are seen in one quadrant, while the usual direction is preserved in the other three. Thus, every division of the third quartette (with the possible exception of $3a^{1.2}$, etc.) shows reversal in at least one quadrant, and the same is true of certain cells of the first and second quartettes. These reversals, however, unlike those which occur in all four quadrants, have reference to the appearance of bilateral symmetry.

In Neritina there is a total reversal of the cleavage in the basal cells of the cross $1a^{1.2}$, etc., just as in Crepidula, whereas in Umbrella the cleavage of these cells follows the usual rule. I have elsewhere (p. 95) pointed out the fact that upon this reversal the continued existence of the cross as a recognizable structure in Neritina and Crepidula depends. In Neritina the second division of the third quartette ($3a^1$, etc.) is indicated in Blochmann's figures (see Diagram 12, *b*), and this shows reversals in quadrants B and D, so that the divisions are purely bilateral; in Crepidula there is a reversal in quadrant D only, so that the cleavage is bilateral in the posterior quadrants, but not in the anterior ones. Umbrella shows almost exactly the same reversals in the history of the third quartette as are exhibited by Crepidula. All three of these gasteropods show a slightly greater tendency to reversals in quadrants B and D than in quadrants A and C.

TABLE GIVING DIRECTIONS OF CLEAVAGES IN CREPIDULA.

(*Cases of reversal are in italics.*)

GENERATION.	NUMBER OF CELLS.	DESIGNATION OF CELLS.	DIRECTION OF DIVISION.	FIGURES.
I	1	Ovum		
II	2	AB, CD	Right	2–6
III	4	A, B, C, D	Left	7–11
IV	8	A, etc. < A, etc. / 1a, etc.	Right	12 & 13
V	12	A, etc. < A / 2a	Left	14 & 15
	16	1a, etc. < $1a^1$ / $1a^2$	Left	16
VI	20	A < A / 3a	Right	17
	24	2a < $2a^1$ / $2a^2$	Right	18 & 19
	29	$1a^1$ < $1a^{1.1}$ / $1a^{1.2}$	Right	22
	—	$1a^2$ < $1a^{2.1}$ / $1a^{2.2}$	Bilateral (divides in quadrants A and B only)	50
VII	25 & 52	A < A / 4a	Left	21 & 33
	34	3a < $3a^1$ / $3a^2$	Left in A, B, C; *Right in D* (not constant)	25–28
	38	$2a^1$ < $2a^{1.1}$ / $2a^{1.2}$	Left	26–28
	42	$2a^2$ < $2a^{2.1}$ / $2a^{2.2}$	Left in A, B, C; *Right in D* (not constant)	26–28
	88	$1a^{1.1}$ < $1a^{1.1.1}$ / $1a^{1.1.2}$	Left	44–47
	47 & 77	$1a^{1.2}$ < $1a^{1.2.1}$ / $1a^{1.2.2}$	*Right* (*reversed spiral*)	31 & 42
VIII	—	A < A / 5a	Bilateral	54 & 57–60
	—	4a < $4a^1$ / $4a^2$	Right (?)	58 & 59

Generation.	Number of Cells.	Designation of Cells.	Direction of Division.	Figures.
VIII	64 & 68	$3a^{1} < 3a^{1.1}, 3a^{1.2}$	Right in A, B, C; *Left in D* (almost equatorial)	36 & 38
	68 & 77	$3a^{2} < 3a^{2.1}, 3a^{2.2}$	*Left* in A, B, C; Right in D (almost equatorial)	42 & 43
	77 & 88	$2a^{1.1} < 2a^{1.1.1}, 2a^{1.1.2}$	Right in A, B, D; *Left in C* (not constant)	44–47
	58 & 64	$2a^{1.2} < 2a^{1.2.1}, 2a^{1.2.2}$	Right	35 & 38
	58 & 64	$2a^{2.1} < 2a^{2.1.1}, 2a^{2.1.2}$	*Left* (almost meridional)	35 & 38
	—	$1a^{1.1.1} < 1a^{1.1.1.1}, 1a^{1.1.1.2}$	Bilateral	53
	—	$1a^{1.1.2} < 1a^{1.1.2.1}, 1a^{1.1.2.2}$	Bilateral, *Left in B and D*	51
	111	$1a^{1.2.1} < 1a^{1.2.1.1}, 1a^{1.2.1.2}$	Right in A, B, C; no division in D (almost meridional)	46 & 47
	77	$1a^{1.2.2} < 1a^{1.2.2.1}, 1a^{1.2.2.2}$	Right in A, B, C; no division in D (almost meridional)	42
IX	111	$3a^{1.1} < 3a^{1.1.1}, 3a^{1.1.2}$	*Right in A and B;* no division in C and D	46 & 47
	111	$3a^{1.2} < 3a^{1.2.1}, 3a^{1.2.2}$	Left	44–47
	—	$2a^{1.1.1} < 2a^{1.1.1.1}, 2a^{1.1.1.2}$	Bilateral in A and C; *Right in D*	49 & 56
	—	$2a^{1.1.2} < 2a^{1.1.2.1}, 2a^{1.1.2.2}$	Bilateral in A and C; *Right in D*	49 & 56
	111	$2a^{1.2.1} < 2a^{1.2.1.1}, 2a^{1.2.1.2}$	Left in A, B, C	46 & 47
	88	$2a^{1.2.2} < 2a^{1.2.2.1}, 2a^{1.2.2.2}$	Left in A, B, C	44–47
	111	$2a^{2.1.1} < 2a^{2.1.1.1}, 2a^{2.1.1.2}$	*Right* (almost equatorial)	46 & 47
	111	$2a^{2.1.2} < 2a^{2.1.2.1}, 2a^{2.1.2.2}$	Left (almost equatorial)	46 & 47
	—	$1a^{1.2.1.1} < 1a^{1.2.1.1.1}, 1a^{1.2.1.1.2}$	Bilateral (*Right in A and C;* no division in D)	53
	—	$1a^{1.2.1.2} < 1a^{1.2.1.2.1}, 1a^{1.2.1.2.2}$	Bilateral (Left in A, B, C; no division in D)	53
	—	$1a^{1.2.2.1} < 1a^{1.2.2.1.1}, 1a^{1.2.2.1.2}$	Bilateral (*Right in A, B, C;* no division in D)	49
	—	$1a^{1.2.2.2} < 1a^{1.2.2.2.1}, 1a^{1.2.2.2.2}$	Bilateral (Left in A, B, C; no division in D)	49

In the following table the reversals in Crepidula are classified according to quadrants :

TABLE OF REVERSALS OF CLEAVAGE IN CREPIDULA.

In all four quadrants.	Quadrant A.	Quadrant B.	Quadrant C.	Quadrant D.
$1a^{1.2}$, etc.	$3a^2$ (?)	5b	$3c^2$ (?)	3d
$2a^{2.1}$, etc. (?)	$1a^2$	$3b^2$ (?)	$2c^{1.1}$	$2d^2$
$2a^{2.1.1}$, etc. (?)	$3a^{1.1}$ (?)	$1b^{1.1.2}$	$1c^{1.2.1.1}$	5d
	$1a^{1.2.1.1}$	$3b^{1.1}$	$1c^{1.2.2.1}$	$3d^1$
	$1a^{1.2.2.1}$	$1b^{1.2.2.1}$		$1d^{1.1.2}$
				$2d^{1.1.1}$
				$2d^{1.1.2}$
Total cases . . 3	5	5	4	7
Doubtful . . . 2	2	1	1	0

Dropping all doubtful cases of reversal, in which the spindles are nearly meridional or equatorial in position, there remain three cases in quadrant A, three in C, four in B, and seven in D.

To this table of reversals there should be appended a statement that *eight* divisions which occur in the anterior quadrants, A and B, are not represented in quadrant D, and likewise *two* are not represented in quadrant C. All of these omitted cleavages except two ($3c^{1.1}$ and $3d^{1.1}$) are connected with the peculiar history of the posterior turret cells and the posterior arm of the cross.

In a few cases which are classified here as reversals the nuclear spindle does not indicate that the cleavage is to be reversed, and even the daughter nuclei may occupy the same relative positions as in the quadrants in which there is no reversal, while at the same time the lobing of the cytoplasm and the subsequent rotation show that the cleavage is reversed ; the first division of 3d (Figs. 25, 26, 29) is a case in point. In such cases the conditions which influence the direction of the cleavage are not manifested until after the nuclear division is completed, whereas they are usually shown in the direction of the nuclear spindles and in the earliest stages of cleavage.

(b) *The Bilateral Type.*

In purely bilateral cleavage, such as is found among the ascidians and cephalopods, the very first division is bilaterally symmetrical. In other cases bilaterality may not be clearly marked at the beginning of the development, but still may appear at a very early stage.

Wilson ('93) has found in Amphioxus that the cleavage of normal eggs may be bilateral or spiral or radial in the earliest stages, although there is shown "a distinct tendency toward bilaterality in almost all forms of the cleavage."

In annelids, gasteropods, and lamellibranchs, on the other hand, the cleavage is typically spiral until about the time of the formation of the mesoblast (4d). In Nereis, according to Wilson, this spiral cleavage suddenly gives place to the bilateral. "The most striking feature in the cleavage," says Wilson, "and the one on which the entire discussion may be made to turn, is the sudden appearance of bilateral symmetry in the cleavage; . . . *the bilaterality does not appear at the beginning of development.* It appears only at a comparatively late stage, and by a change so abrupt and striking as to possess an absolutely dramatic interest." "The bilateral asymmetry of the early stages depends mainly upon the fact that the substance of the somatoblasts (*i.e.*, the mesoblast and the material of the ventral plate) is stored in the left posterior macromere. Bilateral symmetry is established upon the reduction of this macromere (D) to the size of its fellow (C) by the separation of the somatoblasts and their transportation to the median line. Immediately upon this event follows the appearance of bilateral cleavages throughout the embryo, except in the cells which give rise to the prototroch, a purely larval organ."

Among the mollusks conditions are very different, bilateral cleavages appearing very slowly, and creeping, as it were, from cell to cell and from quadrant to quadrant. In Crepidula they first appear at the 34-cell stage and by a slight shifting in position of a single cell, $3d^2$, immediately after its formation; so slight is this movement that it is doubtful whether it ought to be considered as indicative of bilateral cleavage. The

next bilateral cleavage occurs at the 42-cell stage, and it also consists of a slight change in the direction of division of the single cell $2d^2$. The first bilateral division in the mesoblast occurs at the 44-cell stage, and all subsequent divisions in this layer are bilateral. From this time on bilateral cleavages increase in number, but up to the stage with 111 cells perfect spiral cleavages are present, and in the very latest stages to which any group of cells could be traced spiral cleavages were found in some of the cells (usually three) of each group. (See table of Directions of Cleavage.)

That the reason assigned by Wilson for the "bilateral asymmetry" of the early stages is not applicable here is shown by the fact that in many of the gasteropods the left posterior macromere is not appreciably larger than the right and in some (*e.g.*, Umbrella) it is smaller, and also by the fact that the mesoblast (4d) is only one member of a quartette which is separated in a left spiral from the macromeres, each of the other members being quite as large as, or even larger than, the cell 4d. The following conclusions may be drawn concerning the origin of bilateral cleavages among the gasteropods :

(1) Bilateral cleavages first appear on the posterior side of the egg.

(2) They are generally due to a reversal of the direction of cleavage of one out of four cells, this reversal being most frequent in quadrant D.

(3) Certain time differences appear between the divisions on the anterior and posterior sides of the egg, the divisions on the posterior side being much slower in the first quartette, but ultimately much more rapid in the second and third.

(4) Another factor in the establishment of bilaterality, and the one which gives meaning to the three preceding ones, is the teloblastic growth of all the layers at the posterior end of the embryo and the formation in this region of the larger part of the adult body.

(5) The primitive radial symmetry is preserved in the anterior quadrants long after it has disappeared in the posterior ones, *e.g.*, the arms of the cross, the origin of larval mesoblast in quadrants A, B, and C, etc.

The conclusion, therefore, is unmistakable that bilaterality first appears in processes which lead to the formation of the trunk and the elongation of the future animal, while the primitive radial symmetry of the anterior quadrants, which is so long preserved, is correlated with the fact that these quadrants give rise largely to larval organs, most of which bear traces of radial symmetry.

(c) *Significance of the Forms of Cleavage.*

The cause of alternating cleavages in general has been very fully discussed by various writers, particularly by Sachs (*Physiology of Plants*, ch. XXVII) and by Hertwig (*Die Zelle und die Gewebe*, ch. VI). The latter author has presented, in the form of two laws, the principles upon which alternations in division are based. These laws are: (1) the nuclear spindle lies in the direction of greatest elongation of the protoplasm; (2) the division walls intersect the spindles and the previous division planes at right angles. It is probable that these principles are true in general, but they meet with many exceptions in the development of most animals. The chief objection to these laws is that they assume that protoplasm is an inert substance which behaves during and after division like so much clay. On the other hand, nothing is more certain than that protoplasm has intrinsic powers, which are, at least occasionally, capable of setting aside these mechanical principles: *e.g.*, it is able to change its shape so that it may elongate twice or a dozen times in the same direction, as is seen in most cases of teloblastic growth; or the axis of the nuclear spindle may lie in the shortest diameter of the protoplasm, and the division take place apparently in the direction of the greatest pressure (cf. McMurrich, '95[1] and '95[2]); or the division wall may intersect the spindle obliquely (as I have observed in several cases in Crepidula); or successive division walls may intersect each other at any angle from 0° to 90°. The setting aside of these as well as many other mechanical principles on the part of living matter is due to the fact that protoplasm is not soapsuds or oil emulsion, but something vastly more complex than either; and it gives evidence that in cleavage, as in

the entire development, intrinsic factors of development are more important than extrinsic ones.

Reversal of cleavage may be due, apparently, to either of the following causes: (1) it may be produced by external mechanical disturbances which compel a second division in the same direction, or (2) it may be caused by the precocious appearance of certain organs or planes of symmetry. In most cases of normal cleavage I believe it can be shown that the first cause is dependent upon the second, and that the ultimate cause of reversals is therefore an intrinsic one.

All that one can affirm concerning the so-called "law of alternating cleavage" is that in early stages successive cleavages tend to alternate in direction if uninfluenced by processes of differentiation. This law of alternation is less manifest in the later than in the early stages of development, and even in the early stages it may be violated as soon as definite cell groups, *e.g.*, the cross, begin to appear.

Apart from the general phenomenon of alternations in cleavage we may now consider the significance of the peculiar features of radial and bilateral cleavages.

(1) Significance of Orthoradial Cleavages. — The most remarkable thing concerning orthoradial cleavage is that it does not conform to the principle of minimal contact surfaces. So far as I can recall, all eggs which are said to exhibit this form of cleavage, *e.g.*, Amphioxus, echinoderms, Scyandra, do not exhibit a compact form, but consist of a number of blastomeres loosely piled together, and generally with a large segmentation cavity between them. During the early stages of cleavage these blastomeres are individualized to such an extent that they are globular and are not closely pressed against their neighbors. There are therefore no rotations of the blastomeres, and consequently no polar furrows or pressure surfaces. The compactness of the egg is sacrificed to the independence of the segment spheres. It may be worth while to remark in passing that this independence in form is generally associated with a great amount of independence in function, as experiment has demonstrated in the case of echinoderms and Amphioxus. Sooner or later these independent blastomeres

lose their rounded outlines ; they are pressed more and more closely together, rotations occur, and consequently pressure surfaces are developed, and we have the principle of surface tension and, perhaps, of mutual attraction between the cells (*Cytotropismus*, Roux '94) asserting itself in the appearance of *spiral cleavages*.

(2) Significance of Spiral Cleavages. — So far as the mere rotation of blastomeres and the consequent formation of polar furrows and pressure surfaces is concerned, I quite agree with Wilson that "the spiral type owes its peculiarities entirely to mechanical conditions, the blastomeres assuming the position of greatest economy of space, precisely like soap bubbles or other elastic bodies." This form of cleavage alone fulfills the conditions of minimal contact surfaces, and considered from the purely physical standpoint, it is a wonder that it should ever fail to occur. Spiral cleavages, then, in general, are certainly due to the general physical phenomenon of surface tension ; but the fact that they occur in definite directions is just as certainly due to something else. The absolute constancy of direction in certain cases of spiral cleavage is a thing which no merely extrinsic factors can possibly account for. The alternate directions of the spirals is but an expression of alternation in general, and each successive cleavage finds the sufficient cause of its direction in the direction of the preceding one until we reach the first cleavage. Why is the first division dexiotropic rather than laeotropic ? I cannot at present answer this question, but it is obvious that the cause of this constancy of direction must be intrinsic rather than extrinsic, and that it must be sought for, not in the mechanical conditions of surface tension, but rather in the structure of the unsegmented egg itself.

The direction of the spirals has presumably a profound influence upon the entire development. Associated with it is the formation of the mesoblast and the greater part of the adult body from the left posterior macromere in cases where the spirals are not reversed, whereas it is the *right* posterior macromere which gives rise to these structures in cases of reversed cleavage, as Crampton ('94) has shown in the case of

Physa. And the fact that the asymmetry of the adult body is reversed in those gasteropods (Physa, Planorbis) which show reversed cleavage makes it probable that the direction of the spirals influences not only the cleavage stages but also the entire development.

The general significance of radial cleavages, both orthoradial and spiral, may be considered here, since it has much to do with the interpretation of cleavage in general. The mere alternation of divisions explains many fundamental features of radial cleavage, but it by no means touches upon its most interesting and remarkable characters. These characters are particularly well shown, not only in the radial symmetry manifested in the *direction* of division, but especially in the *size* and *shape* of the blastomeres. Unequal cleavages in themselves, as I have argued elsewhere, must signify more than extrinsic forces; they can be explained only by assuming certain intrinsic causes, and when we have these unequal cleavages minutely repeated in the different quadrants, even though the mechanical environment in those quadrants may be different, we have to reckon with causes which are still more complex and obscure.

As striking illustrations of this radial symmetry in the position, size, and shape of cells may be mentioned the following: the formation of four macromeres, frequently equal in size; the formation of at least three, usually four, quartettes of micromeres; the radial symmetry manifested in the history of each quartette, unless modified by the early appearance of definitive structures; the radial symmetry of embryonic or larval structures, such as the trochoblasts, the apical cells, the terminal and basal cells of the cross, the reversed cleavage in each of the latter, and the long-continued resemblances between the right, the left, and the anterior arms of the cross; the origin of the mesoblast from the second quartette in quadrants A, B, and C, and from the fourth quartette in quadrant D.

In several cases these radial structures seem to belong to the same category as the radial structures of the trochophore larva,

and I believe that they are to be explained as a foreshadowing of larval characters, just as bilateral cleavages are usually attributed to a precocious development of adult characters.

Wilson ('93) emphasizes the fact that bilaterality in cleavage is an inherited character. This is undoubtedly true, but it is also just as true that radiality in cleavage is an inherited character. It is possible to conceive of a radiality which would be due merely to extrinsic forces and stresses, but this is not the radiality of cleavage; for so far as now known the latter is characterized by a definiteness in the directions of division and in the size and form of the resulting cells, which such extrinsic forces are wholly unable to explain.

It seems to me highly probable that all forms of cleavage are truly inherited, just as certainly as the size and shape and character of the egg or spermatozoön are inherited. The loose character of the aggregate of blastomeres in Amphioxus, the compact form of cleavage with its definite spirals in the annelid or mollusk, the bilateral arrangement of the blastomeres in the ascidian, all are ultimately due to the same thing, *viz., the structure of the germinal protoplasm.* These peculiarities could not be produced by extrinsic forces, they must come from within; and, if I understand the word at all, this is just what distinguishes *heredity.* On the other hand, certain minor features in all these forms of cleavage are due to extrinsic factors, and consequently the forms of cleavage, like all other forms of the organism, are the resultants of the intrinsic and of the extrinsic factors of development.[1]

(3) Significance of Bilateral Cleavages. — In the case of bilateral cleavages the law of alternations or rectangular intersections is violated more or less from the beginning; and likewise the principle of minimal contact surfaces is more or

[1] In a review of Wilson's work, Driesch ('95) criticises this very point in a way with which I thoroughly agree. He finds the cause of all different kinds of cleavage in the structure of the protoplasm, and hence concludes that one is as truly inherited as the other. It seems to me that this conclusion differs radically from some of his earlier views concerning cleavage; indeed, I am unable to harmonize it with other expressions in this same paper, *e.g.*, he says that there can be no phylogenetic significance in the close resemblance between the cleavage in annelids and gasteropods *because it has been mechanically produced* (see p. 195).

less completely set aside. It seems quite certain, therefore, that the cause of the bilateral form of cleavage is an intrinsic, not an extrinsic, one. If it be true that there are cases of bilateral cleavage which have no reference to the bilaterality of the adult, as Miss Clapp's ('91) observations on the toadfish and Morgan's ('93) on certain teleosts indicate, it can only be explained, so far as I can see, by supposing that the same causes which operate to produce bilaterality in the adult may operate independently on the cleavage stages, producing bilateral symmetry which has no connection with that of the adult. This probability seems to me so remote that I think it more likely, considering the extensive shiftings and rotations of blastomeres which have been observed in some animals, that the bilaterality of cleavage is only an early appearance of the final bilaterality with which it is directly continuous, *though perhaps only after extensive shiftings of cell groups, or even of entire layers, have occurred.* The further possibility remains that in some cases apparently bilateral cleavages are not really bilateral, but are radial, as is the case with the first cleavage in Crepidula.

(d) *Determinate and Indeterminate Cleavage.*

In only a comparatively small number of animals, so far, has the history of individual blastomeres been traced through the development to the organs which they ultimately form. In a few cases, however, among such widely separated groups as Annelida, Gasteropoda, Lamellibranchiata, Arthropoda, and Tunicata, this has been done in the case of a few cells, and with the constant result that, under normal conditions, definite cells in any given animal invariably give rise to definite structures in the embryo or the adult. Such cells are not only identical in origin and destiny but also in shape, size, and developmental history. Such definiteness in the origin, form, and history of blastomeres leads irresistibly to the view that the history of each cell in such ova is, under normal circumstances, predetermined and always in the same way and to the same end. For all such kinds of cleavage I propose the name *determinate cleavage.*

On the other hand, in most Echinodermata, Coelenterata, and Vertebrata, no such definiteness in the history of the blastomeres is known to exist. Of course the possibility remains that in most, if not all, of these cases the cleavage is of just as determinate a character as in the first class mentioned, and that the denial of a definite prospective value to each blastomere must rest upon the curious basis that no one has followed a single blastomere through the development. I confess that to me this possibility seems extremely probable.

Under present circumstances, however, it would be unjustifiable to classify all cleavage as equally determinate in character, merely on the grounds of analogy with such cases as the annelids and mollusks. There is some evidence that the extent of predetermination differs in different cases (see Wilson, '93 and '94); I propose, therefore, to classify all cases in which predetermination is not known to exist as *indeterminate cleavage*. Such a classification is in many respects an unsatisfactory one, and it can only be regarded as having a temporary value, but it will serve to emphasize a distinction which in our present state of knowledge we must recognize as existing.

Most of the earlier experimental work in embryology was done upon forms in which the cleavage is not known to be determinate in character, and many general conclusions were drawn which are not applicable to determinate cleavage. For example, some of Driesch's conclusions have been too sweeping; no one who has ever studied such determinate forms of cleavage as are exhibited by the annelids and the mollusks could for a moment admit the truth of his earlier conclusion ('93): "By segmentation perfectly homogeneous parts are formed capable of any fate." There is every ocular evidence that in the cases referred to, the parts separated by cleavage are not perfectly homogeneous, and under such circumstances to assert that they are would be the climax of self-stultification.

There is, I think, a fallacy in Hertwig's much-quoted dictum ('92): "In consequence of the continuity of development, every older cell group must arise from a younger cell group, and so finally definite parts of the body from definite segment cells." A true conclusion would be this: "And so finally definite parts

of the body from any cell you please." The fact that definite parts of the body come from *definite cleavage cells* means more than the mere continuity of development, and in this very fact the whole question at issue between determinism and indeterminism is contained.

Later work, particularly that of Wilson ('92) on Nereis, Driesch and Morgan ('95) on Ctenophore eggs, and Crampton ('96) on Illyonassa, have led to important modifications of these extreme views. Driesch now sees in cleavage something more than the mere sundering of perfectly homogeneous materials. He grants, what one cannot fail to observe in many cases of determinate cleavage, the existence of cytoplasmic differentiations in certain cleavage cells, and even in some cases in the unsegmented egg (v. Driesch and Morgan ('95), p. 221). He still maintains, however, that the possibility of predicting the prospective significance of single cells is simply a result of the continuity of development as Hertwig's dictum asserts.

2. *Cell and Regional Homologies.*

In looking for the earliest appearing homologies between different animals, embryologists have generally been content to stop with the germ layers. One of the first and most successful attempts to go back of germ layers was made by Professor Whitman ('78) in his classical work on the embryology of Clepsine. Since then, under the stimulus of his work and suggestion, there have appeared, chiefly from the Marine Biological Laboratory at Wood's Holl, a remarkable series of contributions on this subject of the earliest homologies (cf. Wilson '92 and '93, Lillie '93 and '95, Mead '94, Conklin '92). Owing in large part to the work of this school, there are now sufficient data at hand for making an extensive comparison of every step in the development of a number of annelids, lamellibranchs, and gasteropods.

(a) *Cell Homologies among Annelids and Mollusks.*

Until recently there has been an evident tendency to regard cleavage in different families and orders as exhibiting only general and not detailed resemblances. Thus Bobretzky ('77)

believed that mollusks had only the gastrula form in common with other animals. Wolfson ('80) and Fol ('76), who maintained that there were agreements between the early cleavage stages in gasteropods and lamellibranchs, were opposed by Rabl ('79), Hatschek ('80), and Blochmann ('81), who held that there were no detailed resemblances. Blochmann concluded that the cleavage in Chiton does not belong to the gasteropod type; and although he pointed out several resemblances between the cleavage of gasteropods and of turbellarians, no one supposed that outside the molluscan phylum any exact or long-continued resemblances to molluscan cleavage would be found. I recall with what astonishment Professor Wilson and the writer found, only a few years ago, that the cleavage of Nereis and Crepidula was so wonderfully similar in many respects. Wilson ('92) called attention to many of these resemblances, though at that time I think he did not suspect that they were as numerous nor as precise as they have since been found to be. Lillie's ('95) work added some very important points of resemblance between the cleavage stages of the annelids and the mollusks, and in this work I have been able to add still others.

Wilson ('92) emphasizes the following important resemblances between the early cleavage stages of the annelid, the polyclade, and the gasteropod: (1) the *number and direction of the cleavages* is the same in all three up to the 28-cell stage; (2) in general the cells formed are *similar in position and size, viz.*, there are four macromeres, three quartettes of micromeres, and the first quartette is surrounded by a belt composed of the second and third quartettes. The first quartette undergoes three spiral divisions in alternate directions, and the second quartette divides once. Here the resemblance with the polyclade ceases, though the annelid and gasteropod go one step further in these likenesses, *viz.* (3), the *three quartettes of micromeres are ectomeres* in the annelid and gasteropod, and (4) in both these groups *the mesoblast is formed from the cell 4d*, which gives rise to paired mesoblastic bands.

Beyond this point Wilson believed that the annelid diverged from the gasteropod. He supposed that the "cross" in the two was wholly different both in origin, position, and destiny,

and that the velum had a wholly different origin from the annelidan prototroch.

Lillie ('95) has extended all the above-mentioned resemblances between annelids and gasteropods to the lamellibranchs, and in addition has discovered the following: (5) the *first somatoblast* (*2d*), which gives rise to the ectoderm of the trunk, has exactly the same origin and position and a similar history in the annelid and lamellibranch; (6) it gives rise to a *growing-point* and a *ventral plate* in all respects essentially like those of the annelids. Lillie shows good reason for believing that in other mollusks the posterior growing-point is derived from these cells.

To this list of resemblances between the annelid and the mollusk, which I can confirm in the case of the gasteropod, I have been able to add the following: (7) the *rosette series* of the gasteropod is exactly like the *cross* of the annelid in origin, position, and probably in destiny. The *intermediate girdle cells* of the annelid are like the *cross* of the gasteropod in origin, position, and destiny (at least in part). The differences, therefore, between the annelidan and molluscan cross which Wilson emphasizes are not real ones; (8) the *trochoblasts* of the annelids are precisely similar in origin and destiny (at least in part) to the *turret cells* of the gasteropods. In some annelids (Amphitrite, Clymenella), the prototroch is completed by cells of the same origin as in Crepidula and Neritina. The differences which Wilson points out between these two structures do not therefore exist. In both annelids and mollusks the prototroch lies at the boundary between the first quartette on one side, and the second and third on the other. In both there is found a preoral, an adoral, and a post-oral band of cilia; (9) in the gasteropod the apical cells give rise to an *apical sense organ* such as is found in many annelid trochophores; (10) the *supra-oesophageal ganglia and commissure* apparently arise from the same group of cells in annelids and gasteropods; (11) the *fourth quartette* in annelids and gasteropods contains mesoblast in quadrant D, but is purely entoblastic in quadrants A, B, and C; (12) a *fifth quartette* is formed in gasteropods and some annelids (Amphitrite, etc.), and consists of entoblast only; (13) in the gasteropod *larval mesoblast* arises from the same group

of ectoblast cells as in Unio, differing, however, in this regard that it is found in quadrants A, B, and C, whereas in Unio it is found in quadrant A only; (14) to this list of accurate resemblances in the cleavage cells may be added the fact that *among annelids and mollusks the axial relations of all the blastomeres (except possibly the four macromeres) are the same.*

What a wonderful parallel is this between animals so unlike in their end stages! How can such resemblances be explained? Are they merely the result of such mechanical principles as surface tension, alternation of cleavage, etc., or do they have some common cause in the fundamental structure of the protoplasm itself? Driesch answers ('92): "The striking similarity between the types of cleavage of polyclades, gasteropods, and annelids does not appear startling; it is easy to understand this, since cleavage is of no systematic worth."[1] To this, I think, it need only be said in reply that if these minute and long-continued resemblances are of no systematic worth, and are merely the result of extrinsic causes, as is implied, then there are no resemblances between either embryos or adults that may not be so explained. And conversely, these resemblances in cleavage, however they have been produced, stand upon the same basis with adult homologies.

Within the group of the annelids Wilson ('92) says that "adult homologies are represented by accurate cell homologies in the cleavage stages." But in his general interpretation of

[1] The entire passage (Driesch ('92), p. 41) reads as follows: "Es sind also gewisse äussere Umstände, welche die Furchung beherschen, in Form *empirischer Gesetze* ganz oder nahzu bekannt. Wir können daraus immerhin Manches lernen, so wird uns die auffallende Ähnlichkeit, welche die Furchungstypen von Polycladen (Selenka, Lang), Gasteropoden (Rabl, Blochmann, Fol, etc.), und Anneliden (Wilson) darbieten, nicht so sehr frappiren; *wir haben eine leises Verständniss dafür gewonnen, wesshalb Furchungsbilder nicht systematisch verwerthbar sind.*"

In similar vein he affirms elsewhere ('95, p. 416): "Wenn auch nicht durchaus, so sind also doch in sehr wesentlichem Masse der Furchungsbilder mechanisch verständlich, wofür auch die Thatsache spricht, dass bei Nereis, bei Polyclade, und bei Gasteropoden nahezu identisch gestaltet sind; das spricht zugleich gegen ihren Werth für phylogenetische Abtheilungen." It should be noted that if cleavage is inherited, as Driesch affirms elsewhere in this same paper, and if certain forms of cleavage are characteristic of species, genera, families, and orders, as is unquestioned, cleavage does have phylogenetic significance, whether that significance can be extended to widely different types, such as the polyclades and the gasteropods, or not.

cleavage he points out some fundamental differences between these early stages in the annelids, gasteropods, and polyclades, and concludes (p. 455): "Blastomeres having precisely the same mode of origin and precisely the same spatial relations to the rest of the embryo are by no means necessarily equivalent either physiologically or morphologically, and the early cleavage stages in themselves have little morphological value."

Lillie ('95) has taken much more positive ground for the homology of blastomeres among annelids and mollusks, and he was justified in so doing because of the truly wonderful resemblances which he was able to demonstrate between the lamellibranch and the annelid.

I have attempted to show that the differences of cleavage between the annelids and the gasteropods, upon which Wilson lays emphasis, are only apparent and not real, and that therefore we cannot deny the general homology of blastomeres among annelids, gasteropods, and lamellibranchs.

Concerning the polyclade cleavage I can offer nothing new. The differences here are very great, perhaps irreconcilable, and certainly this is true of other types of cleavage, such as the bilateral, the centrolecithal, and the meroblastic. But to affirm the homology of blastomeres within certain groups is not to assert that they are everywhere homologous, nor that they are completely homologous. The mesoderm of the adult mollusk differs very considerably from that of the annelid, the trunk region in the two groups is widely different, and we need not expect to find the protoblasts of these structures completely homologous.

The fact is there are no *perfect* homologies between adult annelids and mollusks, and therefore we need not expect to find *perfect* homologies between their larvae, germ layers, or cleavage stages; but, since final homologies are invariably based upon earlier ones, we should expect to find that blastomeres in general show resemblances and differences corresponding to the resemblances and differences of the end stages, and this is just what we find in the cases mentioned.

An incidental result of these observations is to bring the annelids and mollusks more closely together than has heretofore

been done. It has been generally conceded that the trochophore larva which appears in the development of both of these groups is evidence of their former connection, but the resemblances mentioned above show that in the prelarval stages, and also in the metamorphosis following the trochophore stage, there are many resemblances between the two groups, particularly in the history of the somatoblasts, the formation of the trunk, and the establishment of bilateral symmetry.

On the other hand, the embryological history only serves to widen the gap between the cephalopods and other mollusks, for in the early development there is apparently nothing in common between the two.

The application of the word *homology* to pregastrular stages may deserve a short explanation and justification. This term as employed by Owen was used to denote morphological correspondence in the relative structure, position, and connection of adult parts; but since this morphological correspondence is characteristic of the parts of embryos as well as of adults, it is evident that to rigidly limit the word *homology* to adult characters would be to draw a wholly artificial and useless distinction between adult and embryonic structures. Accordingly, we find that the word has been very generally used to denote morphological correspondence of embryonic, as well as of adult, parts, and this correspondence was found in earlier and earlier stages of the ontogeny, until Huxley finally homologized the germinal layers of higher metazoa with the cell layers of adult coelenterates.

The chief objections which have been raised in recent years against the general homology of the germ layers arise from the fact that the layers in themselves have been regarded as organs which might be compared as if they were adult parts. They were estimated by what they were rather than by what they might become, and consequently false ideas often obtained as to what they really were; for the real structure of embryonic parts can usually be determined only by observing the entire history of those parts. I presume no one supposes that we can directly recognize and compare the fundamental structure of eggs, blastomeres, or layers; at present the only satisfactory way of

determining whether they have the same structure is to observe what they develop into. If certain embryonic parts always give rise to certain definitive structures, the conclusion is warranted that these parts themselves must be alike in structure. The homology of germinal layers, therefore, must have reference to prospective resemblances, and accordingly the test of all such homologies must be the history and destiny of those layers (cf. Wilson, 95).

If, however, prospective resemblances form a basis for homology, there is no reason for stopping with germ layers in seeking to find the earliest homologies. In those cases in which an entire layer can be reduced to a single cell, how is it possible on morphological grounds to affirm homology of the layer but to deny it to the cell? Is it not evident that an altogether unnatural distinction is made when an imaginary line is drawn between blastomeres and layers, on the one side of which homologies may be predicated and on the other not?

If organs which are homologous among annelids and mollusks, such as the prototroch, the apical sense organ, the stomodaeum, and the ventral plate, can be traced back in their development to certain individual cells of similar origin, position, size, and history, are not these cells truly homologous? If not, where in this developmental process shall we say that homologies begin?

I believe there is no escape from the conclusion that the protoblasts of homologous organs are as certainly homologous as are the organs to which they give rise, that the protoblasts of homologous layers are as surely homologous as are those layers, and that the protoblasts of definite regions are as much homologous as are those regions. We therefore reach the conclusion that, in related organisms with determinate cleavage, homologies may be predicated of single cells, whether they be protoblasts of the nervous system, the excretory system, or the locomotor apparatus; of the ectoderm, the mesoderm, or the endoderm; of the right or left, the anterior or posterior portions of the body.

It does not matter, so far as the fundamental idea of cell homology is concerned, how such homology may have arisen. The definite character of the cleavage of Nereis is ascribed by

Wilson ('92) to *precocious segregation.* Lillie ('95) maintains that it is *parallel precocious segregation* that conditions cell homologies. What the cause of this parallel precocious segregation, or of precocity in general, may be is a matter of much doubt.

The term *precocious segregation* was first introduced by Lankester ('77), to indicate the fact that the segregation of parts or layers might be "pushed back into the egg." From the expressions which are frequently used in this connection, such as the "pushing of characters back into the egg," "the reflection of adult characters back upon the egg," etc., it seems that the process is commonly considered a direct rather than an indirect one; or, in other words, that adult characters appear earlier in successive generations, owing to the influence of the body plasm upon the germ plasm. This distinct form of Lamarckism is apparently held by embryologists who repudiate that doctrine in any other form; it is, however, as can be seen by a moment's thought, the very centre and stronghold of the Lamarckian doctrine. On the other hand, it is possible to explain precocity in development by assuming that eggs show multifarious variations, and that natural selection has picked out such as are most beneficial to the species. In fact, there is no doubt that eggs show repetition and variation phenomena as truly as do adult organisms, and they would therefore afford a field for the action of natural selection.

No satisfactory or conclusive evidence as to the cause of precocity can at present be furnished, but the following observations may help to an ultimate solution of this problem:

(1) Adult characters have influenced embryonic characters, and especially cleavage stages, more than the latter have influenced the structure of the adult. This principle finds very many illustrations, among which may be mentioned the following: great individual variations of cleavage produce slight, if any, variations in the adult, as is shown normally in the case of Renilla and Amphioxus (Wilson) and experimentally in the case of many different animals (Driesch, Hertwig, Wilson, Morgan, etc.). Many determinate characters of cleavage, which can have little or no significance for the egg itself, are yet of

importance in building the adult; among these may be mentioned the early appearance of bilateral symmetry; the appearance of bilateral symmetry in diverse directions in the different layers always associated with the future rotation of some of the layers in a definite direction; the segregation of materials for certain organs, layers, and regions of the body into definite cells; the distribution of yolk to the various blastomeres, being found in some cases in many cells, in others being largely confined to a single cell; all these and a hundred other determinate characters have only a prospective value and must have been produced, either directly or indirectly, by the influence of the later upon the earlier stages.

(2) Precocious differentiation, while indicating a shortening of the *process* of development, does not indicate a shortening in its *duration*. Many animals of high organization run through their development in a very short time and yet show no traces of precocity, while many lower animals, although showing a high degree of precocity, yet develop very slowly; *e.g.*, the chick develops in twenty-one days, Crepidula reaches its larval stage only at the end of four weeks, and yet in the former case no precocity is apparent in the early stages, whereas it appears at the very beginning of development in the latter. Even within the limits of a single group the rate of development varies greatly, though apparently the precocity does not; *e.g.*, the relatively rapid development of pteropods as compared with prosobranchs.

Numberless instances might be given to show that the rapidity of development does not depend upon the amount of yolk contained in the egg, as the text-books always have it, nor upon the temperature at which normal development occurs, but rather upon the individual peculiarities of the protoplasm itself (cf. Kofoid '95 and Castle '96). The shortening of the time of development, therefore, is not in any way correlated with precocious differentiation, and hence it is unwarrantable to assume that the latter has been produced by natural selection, owing to the beneficial effects of the former.

(3) Precocity does not insure the development of a larger number of individuals, nor does an egg which manifests pre-

cocity produce more perfectly and more surely the adult organism. The percentage of abnormal forms among animals which show no precocity is no greater than among those with pronounced precocity. (See abnormalities of development among gasteropods, p. 30.)

(4) It seems probable that by a shortening of the process of development there would be a distinct saving of energy; for if we regard only the energy expended in nuclear and cell division, it is possible to see that in an organ which reaches functional activity after a dozen divisions less energy has been expended than in one which reaches this stage only after one hundred divisions. *To this saving of energy precocious segregation may in general be due.*

The "reflection" of similar larval or adult characters would produce similar effects upon different eggs, and consequently *the similarity of the prelarval stages of annelids and mollusks may be held to be due to the similarity of their larvae;* but there is no reason for supposing that this parallel precocity has been *independently* acquired by annelids and mollusks, since it may well have been produced before the phylogenetic separation of those groups.

(b) *Regional Homologies.*

It is certain that a considerable number of accurate cell homologies are found among annelids, lamellibranchs, and gasteropods, but such homologies cannot at present be claimed for all the cells of the cleaving eggs of these animals; and between these and other groups which manifest determinate cleavage, *e.g.*, Turbellaria and Ctenophora, it is probable that no such accurate cell homologies exist. As has been argued elsewhere, one ought not to expect more complete homologies among blastomeres than among organs. In most cases, however, which have been carefully investigated, homologous organs come from the same *regions* of the cleaving egg. This is a fact of the most general application and of the greatest importance. Apical sense organs, cerebral ganglia, and the ectoderm in general come from the animal pole, the entoderm comes from the vegetal pole, while the mesoderm usually comes from the region be-

tween the two. Polar differentiation, however produced, seems to be essentially the same in all cases.[1] In very many bilateral animals the animal pole forms the cephalic end of the antero-posterior axis, though it frequently undergoes great shiftings to reach that point. So far as known, the trunk region of annelids and mollusks always comes from the same region of the embryo. The prototroch always comes from the region between the first and second quartettes of ectomeres. There is good reason to believe that in all cases each quartette of ectomeres occupies homologous regions in the adult. In all these cases there are fundamental homologies of regions, though homologous parts may not always be limited by homologous cell walls, as Whitman ('94) has argued. The fact, however, that so many accurate cell homologies exist among several different groups seems to me to indicate that the formation of cells has a more important rôle in development than Whitman assigns to it (cf. Wilson, '94).

CONCLUSIONS.

In general, the forms of cleavage are the result of three distinct classes of factors, which may vary in importance in different animals. (1) The first and simplest of all are the *mechanical* conditions, such as surface tension, alternation of cleavage, and the like. These conditions are always present and are generally, though not invariably, fulfilled, the result being that certain fundamental features of all cleavage may be referred to such factors.

(2) The fact that cleavage is an inherited character and that definite forms of cleavage and accurate cell homologies are characteristic of several great groups of animals gives it a certain *phylogenetic* value, for however they may have been produced, inherited structural likenesses which run through closely related species, genera, orders, and types must be considered to have a phylogenetic value. The fact that such likenesses are real homologies, as has been argued elsewhere, is evidence upon this point.

[1] One remarkable exception to this statement is known. Castle ('96) has found that the polar bodies are formed at the vegetal (entoderm) pole in Ciona, and the same is probably true of other ascidians.

(3) The principal significance of any determinate form of cleavage is *prospective* rather than *retrospective;* almost every peculiar feature of determinate cleavage can be referred directly to its usefulness in building the body of the future animal.

The cause of this determinate character of cleavage is not to be found primarily in known mechanical conditions nor the extrinsic factors of development, but rather in intrinsic structures, conditions, and forces. In the category of phenomena, which at present can be explained, so far as I can see, only by referring them to such intrinsic causes, may be mentioned the following: (1) the dexiotropic direction of the first cleavage with the consequent alternation in direction of every succeeding cleavage up to an advanced stage; (2) the reversal of the usual direction of cleavage in the formation of certain definite structures, or in the establishment of bilateral symmetry; (3) the establishment of bilateral symmetry in diverse directions in the different germinal layers, and the subsequent coincidence of these different planes of symmetry in a common plane; (4) the general phenomenon of the unequal division of apparently homogeneous cells; (5) the rapid growth and slow division of certain cells (*e.g.*, the trochoblasts) and the slow growth and rapid division of other cells (*e.g.*, the apicals); (6) the segregation of the ectoblast into three quartettes of cells, and the formation of the mesoblast in the fourth quartette.

These are but a few specific cases and many others might be mentioned; in fact, the most important phenomena of development must be included in this category, — among them the proper collocation of parts and coördination of results, all cases of precocity and determinism, and, in fact, the ultimate cause of all specific and generic characters, some of which are frequently manifested at every stage from the beginning to the end of development. Each and all of these phenomena can at present be attributed only to intrinsic causes, since known mechanical conditions are wholly unable to explain them.

The ground here taken is not one of opposition to the possibility of a mechanical explanation of vital phenomena; such

an explanation Biology, as a causal science, is bound to seek after and expect. It is only against that narrow and near-sighted view which mistakes aims for achievements and which would explain all the mysteries of development by such known forces and conditions as gravity, surface tension, cohesion, viscosity, and the like that exception is here taken. It is safe to assert that, before any such explanation can be given, our conception of mechanics in general must be greatly enlarged. The mechanical explanation of vital phenomena is a great task, and one not to be accomplished in a year or a century. We ought not to deceive ourselves by supposing that we have already reached, or are indeed near, such an explanation.

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SUPPLEMENTARY NOTE.

Renewed study of Figs. 56, 62, 65–73 renders it probable that the interpretation of certain cells in those figures, as shown in the plates and explained in the text, is erroneous. I have already called attention to the fact that, at the time of the formation of the fifth quartette (Fig. 54), the entire egg becomes irregular and many landmarks disappear. Owing to this fact it is extremely difficult to follow the lineage through this period. I have all along been aware of certain discrepancies in the interpretation of some of the figures mentioned but supposed that this might be due to variations in the time of division and in the size of resulting cells; *e.g.*, in Fig. 53 there are, in the anterior arm of the cross, eight cells arranged in two rows, four in a row. In Fig. 56 there are apparently only six cells, three in each row. In Fig. 62 the number is indefinite, though only the basals could be plainly recognized.

I am now inclined to the view that the large paired cells marked V′ in Figs. 62, 69, 70, and 71, and $1b^{1.2.2.2.2.2}$ in Figs. 65, 66, 67, 68, 72, and 73 are the same and that they are identical with the *middle cells* of the anterior arm ($1b^{1.2.2.1.2}$ and $1b^{1.2.2.2.2}$, Fig, 50). Accordingly in all these cases the cells lying apical to these middle cells are the *inner* and *outer basals*, Fig. 53. Throughout all these figures the inner basals remain well marked, the outer ones, however, undergo division, forming in Figs. 69, 70, and 71 two large and two small cells ($1b^{1.2.2.1.2}$ and $2b^{1.1.1}$). The latter, which should be labelled $1b^{1.2.2.2.2}$, are probably thrown away, the former remain as the narrow cells ($1b^{1.2.2.2.1}$, Figs. 65, 66, 67, 68) just apical to the middle cells while on the apical side of these are the inner basals. In Fig. 62 the four small cells lying between the inner basals ($1b^{1.2.1.1.1}$, etc.) and the middles (V′) are probably the derivatives of the outer basals, no one of which has yet been thrown away. *According to this interpretation, the first velar row runs through the tip cells of the anterior arm just as it does through the tip cells of the right and left arms, while a portion of the outer basals of the anterior arm, and not the tip cells, is thrown away.*