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The oxidation of quercite to pyrogallol is caused only by certain varieties of *Pseudomonas aromatica*, so that we have here a very specialised function. Green-malt extract allowed to grow "aromatic" by spontaneous corruption at low temperature abounds in that species and always contains numerous quercite bacteria which besides, are fairly common in sewage and even in canal water as also in "aromatic milk".

Melanine formation from tyrosine is proper to certain sea-vibrios and to *Microspira tyrosinatica* not uncommon in sewage water and easily found by this reaction. It is a microbe closely allied to the cholera and the photogenic vibrios. The tyrosinase function is sometimes suddenly lost by unknown influences, but may return in the same stock. Notwithstanding, the species can be considered as fairly constant and remains so for years in the laboratory.

Beer, poor in extract, colours dark brown when corrupting at the air. This is owing to the presence of a vinegar bacterium, *Acetobacter melanogenum*, which produces a pigment reminding of caramel from peptone. By the secretion products of *A. melanogenum* gelatin is as it were tanned and becomes insoluble in boiling water. Perhaps chinon is inferred in this process.

In natural varieties of the species of microbes, which in all other respects show no difference, the oxidation function in regard to certain substances may be either or not present, but if present it may be very constant in these varieties.

**Zoology.** "*The Eutherian and the Metatherian early blastocyst*".  
By Prof. A. A. W. HUBRECHT.

The careful description of the early development of the Marsupialia by Prof. J. P. HILL in vol. 56, pt., of the Q. J. of micr. Sc., has been anxiously awaited by numerous vertebrate embryologists, who, being acquainted with HILL's important contributions (together with WILSON) to the ontogeny of Monotremes, expected that a firm basis would henceforth be established on which the mutual relationships of the more primitive and the more specialised Mammalia might be built up. In this respect however the valuable publication, above referred to, is a deception. Far from being exhaustive it presents the limited number of observations available in the light of an interpretation in which the distinction of what is normal from what is abnormal, is largely dependent on numerical relations and in which the representatives of the so-called abnormal blastocysts are not fully introduced to the reader, nor sufficiently described at length, to enable the interested student to form an opinion for himself.

And yet this would have been doubly desirable because of the fact that Prof. HILL, who, in his earlier paper on the placentation of Perameles, has so markedly drawn together the Eutheria and the Metatheria, finds in the development of Dasyurus grounds for again separating the two subclasses more definitely.

Immediately after having become acquainted with HILL's first mentioned paper I felt it my duty to attempt to convince myself personally that the differences, just alluded to, do exist and I found in Prof. HILL's laboratory the most hospitable reception and at his hands the most liberal treatment, which permitted me to see and weigh everything for myself, and even to draw and to model such preparations as might seem to favour interpretations different from his own. I cannot too highly value this disinterestedness, thanks to which the problems involved will all the sooner be brought into a light full enough for fellow-workers to draw their own conclusions.

And so I will here attempt to give a brief survey of the principal differences which HILL has detected between the results obtained by him for Dasyurus and my own generalisations, which were chiefly based on personal acquaintance with the Eutherian ontogeny.

There is no doubt that the cleavage phenomena in Dasyurus, up to the 16 cell-stage, are decidedly peculiar and that the arrangement of the 16 cells in two rows of 8 cells each, fully deserves the attention which HILL has directed to it. The first three cleavages seem to occur constantly in a meridional sense. Only the fourth cleavage takes place in a plane perpendicular to the three preceding ones; the result being an aequatorial band of two cellular belts, one composed of 8 smaller cells (representing what HILL calls the formative half of the blastocyst), one other of 8 somewhat bigger cells (the non formative half).

HILL is no doubt justified in emphasizing the points of difference between this stage and the Eutherian morula. They are the following:

a. The Dasyurus blastulastage is normally open above and below, until (very soon after) both the upper and lower solution of continuity will have ceased to exist, thanks to continued proliferation (in the direction of the opposite poles) of the cells constituting the two belts just mentioned.

b. The unilaminar blastocyst does *not* contain an embryonic knob, which has been described by all authors writing on the Eutherian blastocyst, as a group of cells applied at one spot against the exterior trophoblast, and which is composed of the cells *that will furnish the embryonic* (formative) *ectoderm as well as the whole of the embryonic entoderm.*

In *Dasyurus*, as HILL repeatedly says, what is by him called the formative hemisphere (itself a derivative of the belt of 8 smaller cells) of the hollow blastocyst, fulfils the part that was just indicated for the Eutheria by italics, whereas the lower or non formative hemisphere of the blastocyst constitutes the trophoblast of the Metatheria, comparable to that of the Sauropsida and Prototheria and *a fortiori* also of the Eutheria<sup>1)</sup>.

My own interpretation of the Metatherian morula, given on p. 7 of my article in vol. 53 of the Q. Journ. and not based on any personal observations, has taken its starting-point from SELENKA's figures of early Opossum blastocysts, about which HILL expresses grave doubts and which he refuses to look upon as normal. So here the two specialists who have explained to us the early developmental phases of Marsupials are diametrically opposed, one (SELENKA) describing and figuring the presence — inside of the unilaminar blastocyst — of a mothercell (Urentodermzelle) of the entoderm, whereas the other (HILL) is convinced that normally there is no cellular enclosure inside this unilaminar blastocyst wall of *Dasyurus* and thus no mothercell of any embryonic knob comparable to the "inner cell mass" of Eutheria in the Marsupials.

For myself I have based my comparative considerations on SELENKA's data, but have interpreted them differently, looking upon SELENKA's "Urentodermzelle" as the mothercell not of the entoderm only, but of the whole inner cell mass (embryonic knob).

The fact that Prof. HILL *does* figure one case (l. c. Pl. 3 Fig. 37) in which a *Dasyurus* blastocyst contained one big cell in its cavity and that therefore this case — emphatically stated to be *abnormal* by HILL — *does* present a certain amount of comparability with SELENKA's figures above alluded to, made me all the more anxious

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<sup>1)</sup> There is a misunderstanding on p. 107 of HILL's latest paper as to my *not* considering the extra-embryonal ectoderm of Sauropsida as trophoblast. This misunderstanding may have arisen in consequence of HILL having cited the condensed text of my Boston address, while my original paper (Q. J. vol. 53 p. 20, 24, 25) would have left no doubts in his mind and would at the same time have convinced him that the wonderful phenomenon in *Dasyurus* so excellently figured in his fig. 42—46, 48—50, has been welcomed by me as a beautiful confirmation of my contention that in Sauropsida and Ornithodelphia we ought to identify the so-called extra-embryonal ectoderm with the entire trophoblast of Eutheria. Here again the Rauber-cells of the rabbit, of *Sorex* among others have led HILL astray, as they have formerly done BONNER, and *the intercalation* of embryonal ectoderm into the trophoblastic outer layer has not been sufficiently kept in view, although I have particularly called attention to its details in *Tupaja*, *Tarsius*, *Sus*, *Cervus* and so many others.

to become personally acquainted with his early blastocysts of *Dasyurus* among which, as he distinctly mentions, in addition to the one figured in fig. 37 he has come across yet *more* "abnormals." Three of these "abnormals" are here figured and are seen to contain proliferating cells. Their aspect in many respects resembling that of SELENKA's Pl. 17 fig. 11 and Pl. 18 fig. 2, on which my own interpretation, differing slightly from that of SELENKA was based.

From the figures here given, magnified about 150 times and obtained from consecutive sections of one blastocyst each, anybody may make plastic reconstructions in space. It can then not be denied that some of these blastocysts do contain an inner cell mass which in the case of fig. 1 to 5 was of the utmost regularity and composed of 16 cells. In most cases this mass is adherent at one spot to the trophoblast, as we notice it in *Eutheria*. It moreover strikes us that in fig. 1—4 the 16 cells seem to be imbedded in a sort of matrix, distinctly the same as is present in the preparation from which HILL has taken his fig. 37 and which he has there termed *cyl*, whereas in that case (of which I also give illustrations in fig. 7*a—d*) the one cell enclosed inside the blastocyst and designated by HILL by the letters *abn* (standing for abnormal) is yet single in contradistinction to the 16 cell stage just now described and figured. This one cell is not imbedded in, but in close contact with the mass *cyl*. I do not wish for the present to give a further description or interpretation of this matrix, which HILL designates as *coagulum* and which is also found in those blastocysts which he regards as normal and in which there is *no* cellproliferation inside the blastocyst-wall.

I cannot convince myself that the histological aspect of the enclosed cells would justify anyone to stigmatize them as "abnormal".

However, from the evidence at present available I am not going to conclude that, contrary to HILL's conviction, the blastocyst here figured are *normal* and that those which he regards as normal — and which though lacking any internal cell mass are more numerous — should be looked upon as *abnormal*. I am only pretending that a decision on this head is for the present moment *premature*, and that we must necessarily postpone its definite solution until the examination of a much larger batch of blastocysts of either *Dasyurus* or *Didelphys* has furnished us with a key to this riddle<sup>1)</sup>.

<sup>1)</sup> I may here remark that an attempt may be made to explain away the difficulties against a direct comparison between the "normal" and the "abnormal" blastocysts in assuming that in many cases of *Dasyurus* the embryonal knob cells arrange themselves in a flat layer without ever being overgrown by the unilaminar trophoblast wall (such as is also the case in all the reptiles and birds as far as

The other specimens here figured are developmental phases in which the same separation between an outer trophoblast and an inner cell-mass is also visible. Three of them are instructive as representing yet different stages from fig. 1—5.

In fig. 6*a—g* sections are figured in which an inner cellmass is *apparently* present. Close inspection shows that the cells which in fig. 6*b—e* appear to constitute an embryonic knob, at the same time form part of the outer surface of the blastocyst. Whether these very sections furnish arguments on which to conciliate HILL's interpretations with my own, must remain undecided for the present. Fig. 8 makes us acquainted with a blastocyst in which some two or three cells appear to be enclosed within an expanded trophoblast, but here again we may not look upon the specimen as decisive.

Fig. 9*a—d* represent a stage just a little earlier than that of fig. 1—5. The enclosed cells are imbedded in a similar matrix and also number about sixteen.

The size of the enclosed cells is intermediate to that of fig. 7 and 1—5. The distinction between trophoblast and inner cellmass is equally evident.

I finally mention, but do not figure, a somewhat later and considerably larger blastocyst, in which the cells that seem to represent the embryonic knob are histologically less perfect than those in fig. 1—4 and might raise doubts whether this particular specimen is or is not a link in a normal developmental series.

The facts which I have called attention to and which place us in the position of having to suspend our judgment with respect to fundamental support of HILL's theoretical speculations, prevent us *a fortiori* from weighing the respective merits of HILL's theoretical conclusions as compared to my own, and from entering into a debate such as he has opened in the article cited. It should not be lost sight of that just because the questions there raised are fundamental the discussions ought to be preeminently thorough and unprejudiced. The opposition with which my speculations on the first origin of the allantois have been met in different quarters is largely caused by that necessary sequel they lead to, viz. that *no* plausible phylogenetic explanation of the ventral stalk of the Primates and of the free allantois of other mammals and of the Sauropsids is possible as long as we hold on to the line of descent which is so emphati-

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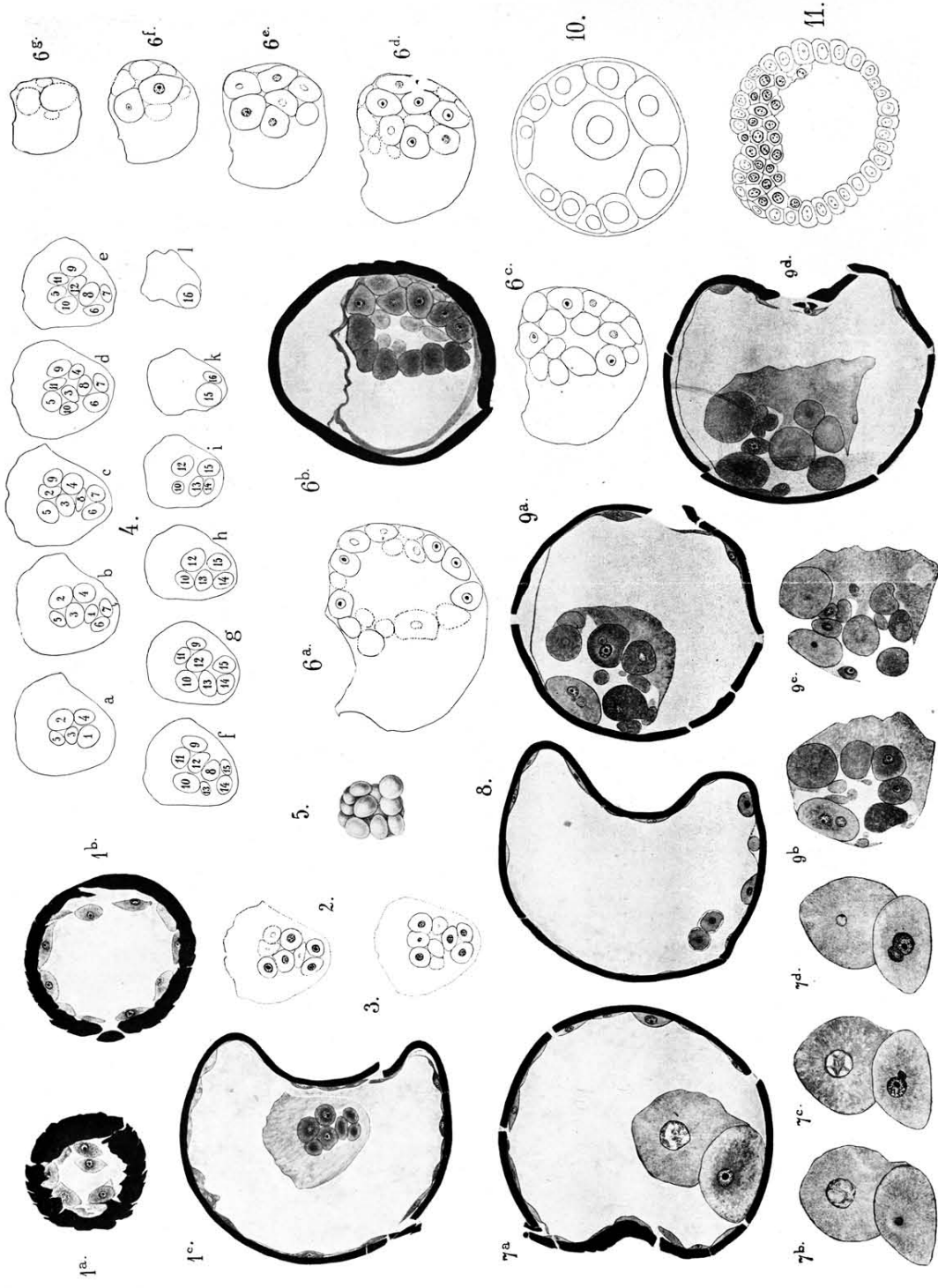
we know them), whereas in other cases these same cells undergo a certain amount of development within this wall and only later become intercalated among the trophoblast cells in the way they do so variously in numerous Eutheria.

cally insisted upon by HILL and others. the derivation of viviparous mammals from oviparous ancestors, which were provided with an eggshell and had a free allantois.

The Prototheria (Ornithodelphia), to the embryology of which HILL has so largely contributed, are thus entirely out of the direct line of descent of all the other mammals. It is only natural that HILL should be a bit prejudiced on this particular point. He admits in his above-cited paper (p. 109 last paragraph) that the unilaminar blastoderm of the Prototheria is unmistakeably the trophoblast, but denies that the cells situated internally to that in the region of the white yolk bed are the mother-cells of the embryonic knob. I feel inclined to believe that the footnote on p. 1079 and the misunderstanding there explained away, will make him reconsider this denial and agree with me that also here we ought to suspend our judgment till more material is available.

I must finally point out that if my contentions on these last few pages concerning the phylogeny of the allantois may to some appear to be too provisional or unreserved I have just in hands and partly already in the press a description of the very early stages of Galeopithecus. In this mammal — the full description of which I hope will be published before the autumn — we find a representative of an order which is undeniably primitive, in the possession of developmental features that force us to conclude that in the very early stages it possesses a connective stalk (Haft- or Bauchstiel) between embryonal shield and trophoblast, whereas later on this stalk disappears in consequence of the development of the coelom and is gradually replaced in about the same situation by a free allantois, the origin of which may be traced to processes in the very matrix of the original connective stalk. This would be a direct argument from ontogeny favourable to my speculations and not explainable in any other way. I cannot however here do more than hint at it.

This paper might perhaps yet have harboured some amount of refutation of certain objections to my theoretical views that were advanced by MAC BRIDE in a paper on Amphioxus in No. 215 of the Q. J. (vol. 54). I refrain from doing so because in that case there are no positive facts upon which to base a rejoinder, as in the case of Prof. HILL's attack. Moreover, since it is patent that MAC BRIDE (l. c. p. 332) has failed to understand my own views about the phylogeny of the allantois to such an extent that he can present it as follows: "along the stalk of connection between embryo





"and vesicle the bladder subsequently grew and so the allantois "was formed" any fruitful discussion on my phylogenetical speculations is excluded from the starting point. And I prefer to abide by MAC BRIDE's final judgment, that: "Prof. HUBRECHT has read the book "of Vertebrate development upside down" until an accumulation of facts on either side will have brought the balance into a position that will allow us to determine what is *up* and what is *down*."

EXPLANATION OF FIGURES. <sup>1)</sup>

Fig. 1—5. *Dasyurus* N<sup>o</sup>. 7.

1*a*—1*c*. Three sections through blastocyst and shell. In *a* and *b* only trophoblast cells; in *c* the inner cell mass (embryonal knob?) enclosed in matrix (coagulum?).

Fig. 2 and 3. The two sections through the inner cell-mass immediately following that of fig. 1*c*.

Fig. 4*a*—4*l*. Eleven consecutive sections through the above, numbered so as to enable us to reconstruct the mulberry shape.

Fig. 5. The actual reconstruction of the 16 cells composing the inner cell-mass.

Fig. 6*a*—6*g*. *Dasyurus* N<sup>o</sup>. 11.

Six sections, of which *b*—*f* are actually consecutive, showing accumulation of massive cells in one blind corner of the blastocyst. It is closed at the other end in 26 consecutive sections situated before 6*a*; it is thus unilaminar. In 6*b* the eggshell is indicated.

Fig. 7*a*—*d*. *Dasyurus* N<sup>o</sup>. 12.

Sections through the same blastocyst and inner cell that have served for Prof. HILL's fig. 37. The nucleus (?) of what HILL calls the coagulum has a very different character from that of the cell. 7*a* has its place in the series between 7*b* and 7*c*.

Fig. 8. *Dasyurus* N<sup>o</sup>. 9.

A section showing shell, trophoblast and two apparently independent cells inside the blastocyst.

Fig. 9*a*—*d*. *Dasyurus* N<sup>o</sup>. 8.

Four sections through a blastocyst that has many points in common with that of fig. 1—5. The size of the cells of the inner cell-mass is intermediate between that of fig. 7*a* and fig. 1*c*; the number is also about 16; the matrix (coagulum) is less regular. The number of trophoblast cells is 62, they are less numerous than in fig. 1*c* where we count 192.

Fig. 10 and 11. Copies of two figures taken from SELENKA's development of the Opossum (Wiesbaden Kreidel 1887) showing (fig. 10) SELENKA's "Urentodermzelle" and (in fig. 11) the contrast between trophoblast and inner cell mass, the latter on the point of coming to the surface.

<sup>1)</sup> The number by which the *Dasyurus* are referred to in this paper refer to preparations which in HILL's collection are labelled as follows:

<i>Dasyurus</i> 7:	2β, 16. VII. 01. ** 39
"	8: 2B. 16. VII. 01. 39
"	9: 46. Picro. abn. 29. 6. 04
"	10: 45. Herm. abn. 29. 6. 04
"	11: 45. Herm. * 29. 6. 04
"	12: 2B. 16. VII. 01. 397.