Analysis of exceptions.

Assuming that all these "contradictory" phenomena happened truly as alleged, and were not pathological or due to error—an explanation which seems quite inadequate —there are at least four possible accounts of such diverse results—each valid, without any appeal to ancestry.

1. That dominance may exceptionally fail—or in other words be created on the side which is elsewhere recessive. For this exceptional failure we have to seek exceptional causes. The artificial *creation* of dominance (in a character usually recessive) has not yet to my knowledge been demonstrated experimentally, but experiments are begun by which such evidence may conceivably be obtained.

2. There may be what is known to practical students of evolution as the *false hybridism of Millardet*, or in other words, fertilisation with—from unknown causes—transmission of none or of only some of the characters of one pure parent. The applicability of this hypothesis to the colours and shapes of peas is perhaps remote, but we may notice that it is one possible account of those rare cases where two pure forms give a *mixed* result in the first generation, even assuming the gametes of each pure parent to be truly monomorphic as regards the character they bear. The applicability of this suggestion can of course be tested by study of the subsequent generations, self-fertilised or fertilised by similar forms produced in the same way. In the case of a *genuine* false-hybrid the lost characters will not reappear in the posterity.

3. The result may not be a case of transmission at all as it is at present conceived, but of the creation on crossing

of something *new*. Our *AB*'s may have one or more characters *peculiar to themselves*. We may in fact have made a distinct "mule" or heterozygote form. Where this is the case, there are several subordinate possibilities we need not at present pursue.

4. There may be definite *variation* (distinct from that proper to the "mule") consequent on causes we cannot yet surmise (see pp. 125 and 128).

The above possibilities are I believe at the present time the only ones that need to be considered in connexion with these exceptional cases*. They are all of them capable of experimental test and in certain instances we are beginning to expect the conclusion.

The "mule" or heterozygote.

There can be little doubt that in many cases it is to the third category that the phenomena belong. An indication of the applicability of this reasoning will generally be found in the fact that in such "mule" forms the colour or the shape of the seeds will be recognizably peculiar and proper to the specimens themselves, as distinct from their parents, and we may safely anticipate that when those seeds are grown the plants will show some character which is recognizable as novel. The *proof* that the reasoning may apply can as yet only be got by finding that the forms in

* I have not here considered the case in which male and female elements of a pure variety are not homologous and the variety is a *permanent* monomorphic "mule." Such a phenomenon, when present, will prove itself in reciprocal crossing. I know no such case in peas for certain.

question cannot breed true even after successive selections, but constantly break up into the same series of forms*.

This conception of the "mule" form, or "hybrid-character" as Mendel called it, though undeveloped, is perfectly clear in his work. He says that the dominant character may have two significations, it may be either a parental character or a hybrid-character, and it must be differentiated according as it appears in the one capacity or the other. He does not regard the character displayed by the hybrid, whether dominant or other, *as a thing inherited from or transmitted by the pure parent at all, but as the peculiar function or property of the hybrid.* When this conception has been fully understood and appreciated in all its bearings it will be found to be hardly less fruitful than that of the purity of the germ-cells.

The two parents are two—let us say—substances† represented by corresponding gametes. These gametes unite to form a new "substance"—the cross-bred zygote. This has its own properties and structure, just as a chemical compound has, and the properties of this new "substance" are *not more strictly* traceable to, or "inherited" from, those of the two parents than are those of a new chemical compound "inherited" from those of the component elements. If the case be one in which the gametes are pure, the new "substance" is not represented by them, but the compound is again dissociated into its components, each of which is separately represented by gametes.

* It will be understood that a "mule" form is quite distinct from what is generally described as a "blend." One certain criterion of the "mule" form is the fact that it cannot be fixed, see p. 25. There is little doubt that Laxton had such a "mule" form when he speaks of "the remarkably fine but unfixable pea, Evolution." *J. R. Hort. Soc.* XII. 1890, p. 37 (*v. infra*).

† Using the word metaphorically.

The character of the cross-bred zygote may be anything. It may be something we have seen before in one or other of the parents, it may be intermediate between the two, or it may be something new. All these possibilities were known to Mendel and he is perfectly aware that his principle is equally applicable to all. The first case is his "dominance." That he is ready for the second is sufficiently shown by his brief reference to time of flowering considered as a character (p. 65). The hybrids, he says, flower at a time *almost exactly intermediate* between the flowering times of the parents, and he remarks that the development of the hybrids in this case probably happens in the same way as it does in the case of the other characters*.

That he was thoroughly prepared for the third possibility appears constantly through the paper, notably in the argument based on the *Phaseolus* hybrids, and in the statement that the hybrid between talls and dwarfs is generally taller than the tall parent, having increased height as its "hybrid-character."

All this Professor Weldon has missed. In place of it he offers us the *sententia* that no one can expect to understand these phenomena if he neglect ancestry. This is the idle gloss of the scribe, which, if we erase it not thoroughly, may pass into the text.

Enough has been said to show how greatly Mendel's conception of heredity was in advance of those which pass current at the present day; I have here attempted

* "*Ueber die Blüthezeit der Hybriden sind die Versuche noch nicht abgeschlossen. So viel kann indessen schon angegeben werden, dass dieselbe fast genau in der Mitte zwischen jener der Samen- und Pollenpflanze steht, und die Entwicklung der Hybriden bezüglich dieses Merkmales wahrscheinlich in der nämlichen Weise erfolgt, wie es für die übrigen Merkmale der Fall ist.*" Mendel, p. 23.

the barest outline of the nature of the "hybrid-character," and I have not sought to indicate the conclusions that we reach when the reasoning so clear in the case of the hybrid is applied to the pure forms and their own characters.

In these considerations we reach the very base on which all conceptions of heredity and variation must henceforth rest, and that it is now possible for us to attempt any such analysis is one of the most far-reaching consequences of Mendel's principle. Till two years ago no one had made more than random soundings of this abyss.

I have briefly discussed these possibilities to assist the reader in getting an insight into Mendel's conceptions. But in dealing with Professor Weldon we need not make this excursion; for his objection arising from the absence of uniform regularity in dominance is not in point.

The soundness of Mendel's work and conclusions would be just as complete if dominance be found to fail often instead of rarely. For it is perfectly certain that varieties *can* be chosen in such a way that the dominance of one character over its antagonist is so regular a phenomenon that it *can* be used in the way Mendel indicates. He chose varieties, in fact, in which a known character *was* regularly dominant and it is because he did so that he made his discovery*. When Professor Weldon speaks of the existence of fluctuation and diversity in regard to dominance as proof of a "grave discrepancy" between Mendel's facts and those of other observers†, he merely indicates the point at which his own misconceptions began.

* As has been already shown the discovery could have been made equally well and possibly with greater rapidity in a case in which the hybrid had a character distinct from either parent. The cases that would *not* have given a clear result are those where there is irregular dominance of one or other parent.

† Weldon, p. 240.

From Mendel's style it may be inferred that if he had meant to state universal dominance in peas he would have done so in unequivocal language. Let me point out further that of the 34 varieties he collected for study, he discarded 12 as not amenable to his purposes*. He tells us he would have nothing to do with characters which were not sharp, but of a "more or less" description. As the 34 varieties are said to have all come true from seed, we may fairly suppose that the reason he discarded twelve was that they were unsuitable for his calculations, having either ill-defined and intermediate characters, or possibly defective and irregular dominance.

IV. Professor Weldon's collection of "Other Evidence concerning Dominance in Peas."

A. In regard to cotyledon colour: Preliminary.

I have been at some pains to show how the contradictory results, no doubt sometimes occurring, on which Professor Weldon lays such stress, may be comprehended without any injury to Mendel's main conclusions. This excursion was made to save trouble with future discoverers of exceptions, though the existence of such facts need scarcely disturb many minds. As regards the dominance of yellow cotyledon-colour over green the whole number of genuine unconformable cases is likely to prove very small indeed, though in regard to the dominance of round shape over wrinkled we may be prepared for more discrepancies. Indeed my own crosses alone are sufficient to show that in using some varieties irregularities are to be expected.

* See p. 43.

Considering also that the shapes of peas depend unquestionably on more than one pair of allelomorphs I fully expect regular blending in some cases.

As however it may be more satisfactory to the reader and to Professor Weldon if I follow him through his "contradictory" evidence I will endeavour to do so. Those who have even a slight practical acquaintance with the phenomena of heredity will sympathize with me in the difficulty I feel in treating this section of his arguments with that gravity he conceives the occasion to demand.

In following the path of the critic it will be necessary for me to trouble the reader with a number of details of a humble order, but the journey will not prove devoid of entertainment.

Now exceptions are always interesting and suggestive things, and sometimes hold a key to great mysteries. Still when a few exceptions are found disobeying rules elsewhere conformed to by large classes of phenomena it is not an unsafe course to consider, with such care as the case permits, whether the exceptions may not be due to exceptional causes, or failing such causes whether there may be any possibility of error. But to Professor Weldon, an exception is an exception—and as such may prove a very serviceable missile; so he gathers them as they were "smooth stones from the brook."

Before examining the quality of this rather miscellaneous ammunition I would wish to draw the non-botanical reader's attention to one or two facts of a general nature.

For our present purpose the seed of a pea may be considered as consisting of two parts, the *embryo with its cotyledons*, enclosed in a *seed-coat*. It has been known for about a century that this coat or skin is a *maternal* structure, being part of the mother plant just as much as the pods

are, and consequently not belonging to the next generation at all. If then any changes take place in it consequent on fertilisation, they are to be regarded not as in any sense a transmission of character by heredity, but rather as of the nature of an "infection." If on the other hand it is desired to study the influence of hereditary transmission on seed-coat characters, then the crossed seeds must be sown and the seed-coats of their seeds studied. Such infective changes in maternal tissues have been known from early times, a notable collection of them having been made especially by Darwin; and for these cases Focke suggested the convenient word *Xenia*. With this familiar fact I would not for a moment suppose Professor Weldon unacquainted, though it was with some surprise that I found in his paper no reference to the phenomenon.

For as it happens, xenia is not at all a rare occurrence with *certain varieties* of peas; though in them, as I believe is generally the case with this phenomenon, it is highly irregular in its manifestations, being doubtless dependent on slight differences of conditions during ripening.

The coats of peas differ greatly in different varieties, being sometimes thick and white or yellow, sometimes thick and highly pigmented with green or other colours, in both of which cases it may be impossible to judge the cotyledon-colour without peeling off the opaque coat; or the coats may be very thin, colourless and transparent, so that the cotyledon-colour is seen at once. It was such a transparent form that Mendel says he used for his experiments with cotyledon-colour. In order to see xenia a pea with a *pigmented* seed-coat should be taken as seed-parent, and crossed with a variety having a different cotyledon-colour. There is then a fair chance of seeing this phenomenon, but much still depends on the variety. For

example, *Fillbasket* has green cotyledons and seed-coat green except near the hilar surface. Crossed with *Serpette nain blanc* (yellow cotyledons and yellow coat) this variety gave three pods with 17 seeds in which the seed-coats were almost full yellow (xenia). Three other pods (25 seeds), similarly produced, showed slight xenia, and one pod with eight seeds showed little or none.

On the other hand *Fillbasket* fertilised with *nain de Bretagne* (yellow cotyledons, seed-coats yellow to yellowish green) gave six pods with 39 seeds showing slight xenia, distinct in a few seeds but absent in most.

Examples of xenia produced by the contrary proceeding, namely fertilising a yellow pea with a green, may indubitably occur and I have seen doubtful cases; but as by the nature of the case these are *negative* phenomena, i.e. the seed-coat remaining greenish and *not* going through its normal maturation changes, they must always be equivocal, and would require special confirmation before other causes were excluded.

Lastly, the special change (xenia) Mendel saw in "grey" peas, appearance or increase of purple pigment in the thick coats, following crossing, is common but also irregular.

If a *transparent* coated form be taken as seed-parent there is no appreciable xenia, so far as I know, and such a phenomenon would certainly be paradoxical*.

In this connection it is interesting to observe that Giltay, whom Professor Weldon quotes as having obtained purely Mendelian results, got no xenia though searching for it. If the reader goes carefully through Giltay's numerous cases, he will find, *almost* without doubt, that none of them were such as produce it. *Reading Giant*, as

* In some transparent coats there is pigment, but so little as a rule that xenia would be scarcely noticeable.

Giltay states, has a *transparent* skin, and the only xenia likely to occur in the other cases would be of the peculiar and uncertain kind seen in using "grey" peas. Professor Weldon notes that Giltay, who evidently worked with extreme care, *peeled* his seeds before describing them, a course which Professor Weldon, not recognizing the distinction between the varieties with opaque and transparent coats, himself wisely recommends. The coincidence of the peeled seeds giving simple Mendelian results is one which might have alarmed a critic less intrepid than Professor Weldon.

Bearing in mind, then, that the coats of peas may be transparent or opaque; and in the latter case may be variously pigmented, green, grey, reddish, purplish, etc.; that in any of the latter cases there may or may not be xenia; the reader will perceive that to use the statements of an author, whether scientific or lay, to the effect that on crossing varieties he obtained peas of such and such colours *without specifying at all whether the coats were transparent or whether the colours he saw were coat- or cotyledon-colours* is a proceeding fraught with peculiar and special risks.

(1) *Gärtner's cases.* Professor Weldon gives, as exceptions, a series of Gärtner's observations. Using several varieties, amongst them *Pisum sativum macrospermum*, a "grey" pea, with coloured flowers and seed-coats*, he obtained results partly Mendelian and partly, as now alleged, contradictory. The latter consist of seeds "dirty yellow" and "yellowish green," whereas it is suggested they should have been simply yellow.

Now students of this department of natural history will know that these same observations of Gärtner's, whether rightly or wrongly, have been doing duty for more than half a century as stock illustrations of xenia. In this

* Usually correlated characters, as Mendel knew.

capacity they have served two generations of naturalists. The ground nowadays may be unfamiliar, but others have travelled it before and recorded their impressions. Darwin, for example, has the following passage*:

"These statements led Gärtner, who was highly sceptical on the subject, carefully to try a long series of experiments; he selected the most constant varieties, and the results conclusively showed *that the colour of the skin of the pea* is modified when pollen of a differently coloured variety is used." (The italics are mine.)

In the true spirit of inquiry Professor Weldon doubtless reflected,

"'Tis not *Antiquity* nor *Author*,
That makes *Truth Truth*, altho' *Time's Daughter*";

but perhaps a word of caution to the reader that another interpretation exists would have been in place. It cannot be without amazement therefore that we find him appropriating these examples as referring to cotyledon-colour, with never a hint that the point is doubtful.

Giltay, without going into details, points out the ambiguity†. As Professor Weldon refers to the writings both of Darwin and Giltay, it is still more remarkable that he should regard the phenomenon as clearly one of cotyledon-colour and not coat-colour as Darwin and many other writers have supposed.

* *Animals and Plants*, 2nd ed. 1885, p. 428.

† "*Eine andere Frage ist jedoch, ob der Einfluss des Pollens auf den Keim schon äusserlich an diesen letzteren sichtbar sein kann. Darwin führt mehrere hierher gehörige Fälle an, und wahrscheinlich sind auch die Resultate der von Gärtner über diesen Gegenstand ausgeführten Experimente hier zu erwähnen, wenn es auch nicht ganz deutlich ist, ob der von Gärtner erwähnte directe Einfluss des Pollens sich nur innerhalb der Grenzen des Keimes merklich macht oder nicht.*" p. 490.

Without going further it would be highly improbable that Gärtner is speaking solely or even chiefly of the cotyledons, from the circumstance that these observations are given as evidence of "*the influence of foreign pollen on the female organs*"; and that Gärtner was perfectly aware of the fact that the coat of the seed was a maternal structure is evident from his statement to that effect on p. 80.

To go into the whole question in detail would require considerable space; but indeed it is unnecessary to labour the point. The reader who examines Gärtner's account with care, especially the peculiar phenomena obtained in the case of the "grey" pea (*macrospermum*), with specimens before him, will have no difficulty in recognizing that Gärtner is simply describing the seeds *as they looked in their coats*, and is not attempting to distinguish cotyledon-characters and coat-characters. If he had peeled them, which in the case of "grey" peas would be *absolutely necessary* to see cotyledon-colour, he must surely have said so.

Had he done so, he would have found the cotyledons full yellow in every ripe seed; for I venture to assert that anyone who tries, as we have, crosses between a yellow-cotyledoned "grey" pea, such as Gärtner's was, with any pure green variety will see that there is no question whatever as to absolute dominance of the yellow cotyledon-character here, more striking than in any other case. If exceptions are to be looked for, they will not be found *there*; and, except in so far as they show simple dominance of yellow, Gärtner's observations cannot be cited in this connection at all.

(2) *Seton's case.* Another exception given by Professor Weldon is much more interesting and instructive.

It is the curious case of Seton*. Told in the words of the critic it is as follows:—

"Mr Alexander Seton crossed the flowers of *Dwarf Imperial*, 'a well-known green variety of the Pea,' with the pollen of 'a white free-growing variety.' Four hybrid seeds were obtained, 'which did not differ in appearance from the others of the female parent.' These seeds therefore did *not* obey the law of dominance, or if the statement be preferred, greenness became dominant in this case. The seeds were sown, and produced plants bearing 'green' and 'white' seeds side by side in the same pod. An excellent coloured figure of one of these pods is given (*loc. cit.* Plate 9, Fig. 1), and is the only figure I have found which illustrates segregation of colours in hybrid Peas of the second generation."

Now if Professor Weldon had applied to this case the same independence of judgment he evinced in dismissing Darwin's interpretation of Gärtner's observations, he might have reached a valuable result. Knowing how difficult it is to give all the points in a brief citation, I turned up the original passage, where I find it stated that the mixed seeds of the second generation "were all completely either of one colour or the other, none of them having an intermediate tint, as Mr Seton had expected." The utility of this observation of the absence of intermediates, is that it goes some way to dispose of the suggestion of xenia as a cause contributing to the result.

Moreover, feeling perfectly clear, from the fact of the absence of intermediates, that the case must be one of simple dominance in spite of first appearances, I suggest the following account with every confidence that it is the true one. There have been several "*Imperials*,"

* Appendix to paper of Goss, *Trans. Hort. Soc.* v. 1822, pub. 1824 (*not* 1848, as given by Professor Weldon), p. 236.

though *Dwarf Imperial*, in a form which I can feel sure is Seton's form, I have not succeeded in seeing; but from Vilmorin's description that the peas when ripe are "*franchement verts*" I feel no doubt it was a green pea *with a green skin*. If it had had a transparent skin this description would be inapplicable. Having then a green skin, which may be assumed with every probability of truth, the seeds, even though the cotyledons were yellow, might, especially if examined fresh, be indistinguishable from those of the maternal type. Next from the fact of the mixture in the second generation we learn that the *semi-transparent seed-coat of the paternal form was dominant* as a plant-character, and indeed the coloured plate makes this fairly evident. It will be understood that this explanation is as yet suggestive, but from the facts of the second generation, any supposition that there was real irregularity in dominance in this case is out of the question*.

(3) *Tschermak's exceptions.* These are a much more acceptable lot than those we have been considering. Tschermak was thoroughly alive to the seed-coat question and consequently any exception stated as an unqualified fact on his authority must be accepted. The nature of these cases we shall see. Among the many varieties he used, some being *not* monomorphic, it would have been surprising if he had not found true irregularities in dominance.

(3 *a*) *Buchsbaum case.* This variety, growing in the open, gave once a pod in which *every seed but one was green.* In stating this case Professor Weldon refers to *Buchsbaum*

* Since the above passage was written I find the "*Imperials*" described in "Report of Chiswick Trials," *Proc. R. Hort. Soc.* 1860, I. p. 340, as "skin thick"; and on p. 360 "skin thick, blue"; which finally disposes of this "exception."

as "a yellow-seeded variety." Tschermak*, however, describes it as having "*gelbes, öfters gelblich-grünes Speichergewebe*" (cotyledons); and again says the cotyledon-colour is "*allerdings gerade bei Buchsbaum zur Spontanvariation nach gelb-grün neigend!*" The (!) is Tschermak's. Therefore Professor Weldon can hardly claim *Buchsbaum* as "yellow-seeded" without qualification.

Buchsbaum in fact is in all probability a blend-form and certainly not a true, stable yellow. One of the green seeds mentioned above grew and gave 15 *yellows* and three *greens*, and the result showed pretty clearly, as Tschermak says, that there had been an accidental cross with a tall green.

On another occasion *Telephone* ♀ (another impure green) × *Buchsbaum* gave four *yellow smooth and* two *green wrinkled*, but one [? both: the grammar is obscure] of the greens did not germinate†.

(3 *b*) *Telephone cases.* *Telephone*, crossed with at least one yellow variety (*Auvergne*) gave all or some green or greenish. These I have no doubt are good cases of "defective dominance" of yellow. But it must be noted that *Telephone is an impure green.* Nominally a green, it is as Professor Weldon has satisfied himself, very irregular in colour, having many intermediates shading to pure yellow and many piebalds. It is the variety from which alone Professor Weldon made his colour-scale. *I desire therefore to call special attention to the fact that Telephone, though*

* (36), p. 502 and (37), p. 663.

† Professor Weldon should have alluded to this. *Dead* seeds have no bearing on these questions, seeing that their characters may be pathological. The same seeds are later described as "*wie Telephone selbst*," so, apart from the possibility of death, they may also have been self-fertilised.

not a pure green, Tschermak's sample being as he says "gelblichweiss grün," a yellowish-white-green in cotyledon-colour, is the variety which has so far contributed the clearest evidence of the green colour dominating in its crosses with a yellow; and that *Buchsbaum* is probably a similar case. To this point we shall return. It may not be superfluous to mention also that one cross between *Fillbasket* (a thorough *green*) and *Telephone* gave three *yellowish* green seeds (Tschermak, (36), p. 501).

(3 *c*) *Couturier cases.* This fully yellow variety in crosses with two fully green sorts gave seeds either yellow or greenish yellow. In one case *Fillbasket* ♀ fertilised by *Couturier* gave mixed seeds, green and yellow. For any evidence to the contrary, the green in this case may have been self-fertilised. Nevertheless, taking the evidence together, I think it is most likely that *Couturier* is a genuine case of imperfect dominance of yellow. If so, it is the only true "exception" in crosses between stable forms.

We have now narrowed down Professor Weldon's exceptions to dominance of cotyledon-colour to two varieties, one yellow (*Couturier*), and one yellow "tending to green" (*Buchsbaum*), which show imperfect dominance of *yellow*; and one variety, *Telephone*, an impure and irregular green, which shows occasional but uncertain dominance of *green*.

What may be the meaning of the phenomenon shown by the unstable or mosaic varieties we cannot tell; but I venture to suggest that when we more fully appreciate the nature and genesis of the gametes, it will be found that the peculiarities of heredity seen in these cases have more in common with those of "false hybridism" (see p. 34) than with any true failure of dominance.

Before, however, feeling quite satisfied in regard even

to this residuum of exceptions, one would wish to learn the subsequent fate of these aberrant seeds and how their offspring differed from that of their sisters. One only of them can I yet trace, viz. the green seed from *Telephone* ♀ × *Buchsbaum* ♂, which proved a veritable "green dominant." As for the remainder, Tschermak promises in his first paper to watch them. But in his second paper the only passage I can find relating to them declares that perhaps some of the questionable cases he mentioned in his first paper "*are attributable to similar isolated anomalies in dominance; some proved themselves by subsequent cultivation to be cases of accidental self-fertilisation; others failed to germinate**." I may warn those interested in these questions, that in estimating changes due to ripening, *dead* seeds are not available.

B. Seed-coats and shapes.

1. *Seed-coats.* Professor Weldon lays some stress on the results obtained by Correns† in crossing a pea having green cotyledons and a thin almost colourless coat (*grüne späte Erfurter Folger-erbse*) with two purple-flowered varieties. The latter are what are known in England as "grey" peas, though the term grey is not generally appropriate.

In these varieties the cotyledon-colour is yellow and

* "*Vielleicht sind einige der l.c.* 507 *bis* 508 *erwähnten fraglichen Fälle auf ähnliche vereinzelte Anomalien der Merkmalswerthigkeit zu beziehen; einige erwiesen sich allerdings beim Anbau als Producte ungewollter Selbstbefruchtung, andere keimten nicht.*"

† Regarding this case I have to thank Professor Correns for a good deal of information which he kindly sent me in response to my inquiry. I am thus able to supplement the published account in some particulars.

the coats are usually highly coloured or orange-brown. In reciprocal crosses Correns found no change from the maternal seed-coat-colour or seed-shape. On sowing these peas he obtained plants bearing peas which, using the terminology of Mendel and others, he speaks of as the "first generation."

These peas varied in the colour of their seed-coats from an almost colourless form slightly tinged with green like the one parent to the orange-brown of the other parent. The seeds varied in this respect not only from plant to plant, but from pod to pod, and from seed to seed, as Professor Correns has informed me.

The peas with more highly-coloured coats were sown and gave rise to plants with seeds showing the whole range of seed-coat-colours again.

Professor Weldon states that in this case neither the law of dominance nor the law of segregation was observed; and the same is the opinion of Correns, who, as I understand, inclines to regard the colour-distribution as indicating a "mosaic" formation. This is perhaps conceivable; and in that case the statement that there was no dominance would be true, and it would also be true that the unit of segregation, if any, was smaller than the individual plant and may in fact be the individual seed.

A final decision of this question is as yet impossible. Nevertheless from Professor Correns I have learnt one point of importance, namely, that the coats of all these seeds were *thick*, like that of the coloured and as usual dominant form. There is no "mosaic" of coats like one parent and coats like the other, though there may be a mosaic of colours. In regard to the distribution of *colour* however the possibility does not seem to me excluded that we are here dealing with changes influenced by conditions.

I have grown a "grey" pea and noticed that the seed-coats ripened in my garden differ considerably and not quite uniformly from those received from and probably ripened in France, mine being mostly pale and greyish, instead of reddish-brown. We have elsewhere seen (p. 120) that pigments of the seed-coat-colour may be very sensitive to conditions, and slight differences of moisture, for example, may in some measure account for the differences in colour. Among my crosses I have a pod of such "grey" peas fertilised by *Laxton's Alpha* (green cotyledons, coat transparent). It contained five seeds, of which four were *red-brown on one side* and grey with purple specks on the other. The fifth was of the grey colour on both sides. I regard this difference not as indicating segregation of character but merely as comparable with the difference between the two sides of a ripe apple, and I have little doubt that Correns' case may be of the same nature*. Phenomena somewhat similar to these will be met with in Laxton's case of the "maple" seeded peas (see p. 161).

2. *Seed-shapes.* Here Professor Weldon has three sets of alleged exceptions to the rule of dominance of round shape over wrinkled. The first are Rimpau's cases, the second are Tschermak's cases, the third group are cases of "grey" peas, which we will treat in a separate section (see pp. 153 and 158).

(*a*) *Rimpau's cases.* Professor Weldon quotes Rimpau as having crossed wrinkled and round peas† and found

* Mr Hurst, of Burbage, tells me that in varieties having coats green or white, e.g. *American Wonder*, the white coats are mostly from early, the green from later pods, the tints depending on conditions and exposure.

† In the first case *Knight's Marrow* with *Victoria*, both ways; in the second *Victoria* with *Telephone*, both ways.

the second hybrid generation dimorphic as usual. The wrinkled peas were selected and sown and gave wrinkled peas *and round* peas, becoming "true" to the wrinkled character in one case only in the fifth year, while in the second case—that of a *Telephone* cross—there was a mixture of round and wrinkled similarly resulting from *wrinkled* seed for two years, but the experiment was not continued.

These at first sight look like genuine exceptions. In reality, however, they are capable of a simple explanation. It must be remembered that Rimpau was working in ignorance of Mendel's results, was not testing any rule, and was not on the look out for irregularities. Now all who have crossed wrinkled and round peas on even a moderate scale will have met with the fact that there is frequently *some* wrinkling in the cross-bred seeds. Though round when compared with the true wrinkled, these are often somewhat more wrinkled than the round type, and in irregular degrees. For my own part I fully anticipate that we may find rare cases of complete blending in this respect though I do not as yet know one.

Rimpau gives a photograph of eight peas (Fig. 146) which he says represent the wrinkled form derived from this cross. It is evident that these are not from *one pod* but a miscellaneous selection. On close inspection it will be seen that while the remainder are shown with their *cotyledon*-surfaces upwards, the two peas at the lower end of the row are represented with their *hilar*-surfaces upwards. Remembering this it will be recognized that these two lower peas are in fact *not* fully wrinkled peas but almost certainly *round* "hybrids," and the depression is merely that which is often seen in round peas (such as *Fillbasket*), squared by mutual pressure. Such peas, when sown, might of course give some round.