Betula intermedia, *Thom.*
B. fruticulosa, *Vahl*? Thingvalla.
B. nana. Thingvalla.
Salix glauca, *Linn.*, not *Sm.* Reinevalla-hals.
S. phylicifolia. Thingvalla.
S. lanata.
S. pyrenaica var. *norvegica*, Fries. Reinevalla-hals.
S. herbacea.

Orchidaceæ.

Orchis latifolia.
Habenaria viridis.
H. hyperborea.

Melanthaceæ.

Tofieldia palustris, *Huds.*

Juncaceæ.

Juncus balticus. Is this the *J. effusus* of Hooker's Fl., or *J. arcticus* of Vahl's List?
J. supinus.
J. bufonius.
J. trifidus.
J. triglumis.
Luzula spicata.
L. multiflora.

Alismaceæ.

Triglochin palustre.

Aroideæ.

Sparganium natans.

Potamogetoneæ.

Potamogeton lanceolatus, Sm.
P. nigrescens, *Fries.*
P. filiformis. Maria Havn, Hval Fiord.
Zostera angustifolia, *Reich.*

Cyperaceæ.

Scirpus cæspitosus.
Eleocharis uniglumis.
Eriophorum capitatum.
E. polystachion γ. elatius, *Koch.*
Elyna spicata.
Carex dioica.
C. chordorhiza. Maria Havn, Hval Fiord.
C. incurva.
C. curta.
C. atrata.
C. capillaris.
C. vaginata.
C. rariflora.
C. cryptocarpa, Meyer.
C. filipendula, *Drej.*
C. vulgaris, *Fries.*
C. hyperborea, Drej.
C. rigida.

Gramineæ.

Anthoxanthum odoratum.
Alopecurus geniculatus.
Phleum commutatum.
Agrostis alba.
Arundo stricta. Near the Geysers and at Maria Havn, Hval Fiord.
Sesleria cærulea.
Aira alpina.
Trisetum subspicatum β. ciliatum.
Poa annua.
P. pratensis.
P. alpina.
P. Balfourii, Parn.
P. cæsia.
P. cæsia β. glauca.
Festuca ovina.
F. rubra γ. arenaria.

Equisetaceæ.

Equisetum umbrosum. Thingvalla.
E. palustre.

Filices.

Polypodium Dryopteris.
P. Phegopteris.
Woodsia ilvensis.
Athyrium Filix-fœmina.
Cystopteris fragilis α.
C. fragilis β. dentata.
Botrychium Lunaria.

Lycopodiaceæ.

Lycopodium selaginoides.

VIII.—*On the Power of the Living Plant to restrain the Evaporation of the Cell-Sap.* By Hugo v. Mohl*.

It is a known fact, attested by numerous weighings, that the living plant, when exposed to light (even diffused daylight,

* Botanische Zeitung, May 7, 1847. Translated by Arthur Henfrey.

which is not capable of increasing the production of vapour from a dead substance permeable by water), gives off a greater quantity of watery vapour to the atmosphere than it does in the dark. The fundamental cause of this phænomenon, the alterations which are brought about in the plant by light, and which condition this increased separation of water, are unknown to us*. The said phænomenon however leads, and I believe in strict correctness, to the assumption, that the evolution of watery vapour from plants is to be referred to two causes; in the first place, to the universal physical law of vaporization, in obedience to which every moist substance gives off water until the atmosphere around is perfectly saturated; and secondly, to a process the more recondite conditions of which are as yet altogether unknown to us, dependent on the vital action of the plant. If I have correctly gathered the views which are advanced in physiological works, the generally received opinion is this:—1st, that the said physical production of watery vapour is regarded as to a certain extent understood; and it is assumed that this is only more or less interfered with by the more or less perfect condition of the cuticle, which is not readily permeable by water or watery vapour; 2ndly, that the said second cause is considered to account for a more abundant separation of water than the physical cause alone would be capable of producing.

A series of facts now appear to me to stand in opposition to this view; I will only mention the well-known phænomenon, that those plants which are most difficult and tedious to dry, as for instance the bulbous plants, the genus *Sedum*, &c., dry very quickly if previously killed by immersion in boiling water. Moreover, it is well known how quickly plants dry which have been killed by poison, frost, &c. From these circumstances it undoubtedly follows that a dead plant, in what way soever it may have been killed, dries quicker than a living plant of the same species, notwithstanding that the evaporation, occurring peculiarly in the living plant under the influence of light, is wanting, and only a true physical separation of water takes place.

I did not remember to have found special researches into this circumstance brought forward in physiological writings; it ap-

* As far as my knowledge goes, no positive observations have been brought forward to show that this separation of water takes place in submerged plants, which would prove the nature of the fact to be not an exhalation of vapour, but a secretion of drops of watery fluid; yet this is in the highest degree probable, since it would be inexplicable how water-plants could accumulate in their interior large quantities of such substances as are contained in so small proportion in water, as for instance the iodine compounds in the plants of sea-water, if they did not in the course of time absorb a considerable quantity of water, and again give it off after depriving it of particular salts.

peared to me therefore not to be altogether without interest to undertake a series of weighings, by which the said facts might be more accurately determined. I selected for this purpose some hot-house plants with thick leaves, since I had reason to hope that, on account of the relatively weaker evaporation in these, the results would be more distinct than in thin-leaved plants: that they might die quickly without the subtraction or addition of water, I let them lie in the open air for twenty-four hours, at a temperature of between −3° and −9° R., in which time they of course were not merely frozen through and through, but also completely killed. I then weighed the plants, and let them lie for fifteen days in a heated room, and compared their loss of weight with that of cut living specimens of as nearly the same size as possible, which had laid beside the frozen specimens. I consider it superfluous to publish the whole series of weighings, and confine myself to the statement of those made at intervals of five days, the results of which are contained in the following table. The numbers express the loss of weight in per-centage of the original weight of the plants:—

	1st–5th day.	6th–10th day.	11th–15th day.	1st–15th day.
Living Plants.				
Polypodium crassifolium. Leaf.	35·9	17·0	7·4	60·3
Ficus elastica. Leaf.....	11·4	7·1	14·3	32·8
Vanilla planifolia. Leaf.....	4·9	11·5	16·4	32·8
Sanseviera guineensis. Leaf. ...	2·7	2·5	1·5	6·7
Epiphyllum truncatum. Stem...	7·9	7·3	6·1	21·3
Stapelia hirsuta. Stem.....	4·6	5·8	6·3	16·7
Average.....	11·4	8·5	8·6	28·4
Frozen Plants.				
Polypodium crassifolium	35·8	18·3	8·9	63·0
Ficus elastica	32·0	13·3	17·3	62·6
Vanilla planifolia	19·1	14·5	10·9	44·5
Sanseviera guineensis	8·3	6·6	5·8	20·7
Epiphyllum truncatum	16·5	9·9	12·2	38·6
Stapelia hirsuta	8·9	19·5	3·0	31·4
Average.....	20·1	13·7	9·7	43·5

I continued the weighings no further, although none of the plants mentioned had lost all their water on the fifteenth day, because the results appeared to me to be sufficiently distinct*.

* I cannot forbear to remark, that the slight loss of weight, which is shown by the weighing of *Stapelia hirsuta* (in the frozen specimen) on the

In the figures of this table lies undoubtedly the proof, that in a dead plant evaporation goes on more actively than in a living one, and that this is the more active the thicker their leaves are. If I am asked what power limits the production of vapour in the living plant, I openly confess that I am unable to answer this question. Those even who believe in the existence of a peculiar vital force, will be little inclined to assume that this force can act in direct opposition to the physical production of vapour; there are indeed only two possible ways of explaining the phænomenon. Either it must be assumed, that in consequence of the death an alteration takes place in the solid parts of the plant, in the cell-membranes, which makes them less dense, more readily penetrable by water or aqueous vapour, than they are in the living plant; or we must assume that chemical changes occur in the cell-contents of the dead plant; that compounds, which by reason of their hygroscopic peculiarity retain water with a certain power in the living plant, are decomposed, or are separated from the cell-fluid and rendered inactive. Our present knowledge of the structure and of the nature of the chemical conditions of plants scarcely place us in the position to decide whether one or other, or both, of these circumstances occur. An alteration in the membrane of the elementary organs, which indeed many may be inclined at first to reject, does not appear to me to be so totally improbable, since in a dead plant the tension which the parts of a living plant exhibit is immediately lost in so great a degree, as to render it impossible to ascribe this to the slight loss of water occurring in the earliest period, and the mere mechanical collapsing of the cells arising from this loss of water, and one is compelled to think of the removal of a tension connected with life. That the loss of this tension renders the cell-membrane more readily permeable by water and aqueous vapour is at least conceivable, and to me at least so much the more probable that I believe that I have often observed foreign substances, such as iodine, penetrate the membrane of a cell which though dead was still full of water, much faster than that of a living cell. I am well-aware that this view will meet with little sympathy at a time when the universal endeavour is to refer the functions of living plants to purely physical and chemical processes,—when in absorption and excretion of fluids the phænomena of endosmose are singly and solely regarded: I must be content, yet entreat a consideration of how little service are these purely physical explanations in reference to the study of the absorption or excretion of sap through the cells of plants;

fifteenth day, raises a suspicion in my mind that I have made a mistake in writing down the weight. This of course I cannot now ascertain; but, at the same time, it does not essentially prejudice the general result.

how this reference of the whole phænomenon to endosmose leaves totally unexplained a series of such phænomena as the swelling up of a particular region of cells in consequence of irritation in sensitive plants. It may be also from the influence of the phænomenon in question, that in many plants the primordial utricle separates from the cell-wall in the dead plant, and in this way the cell-sap comes into direct contact with the cell-wall. However, it appears to me, as I have already remarked, that it is rash at present to talk either about this or other possibilities, since facts, on which a solid theory can be built, time has yet to furnish us with.

IX.—*On the relative Duration of the Power to germinate, in Seeds belonging to different Families.* By M. Alph. DeCandolle *.

(*First experiment.*)

The relative permanence of the faculty of germination in different species of seeds has never been examined with the precision that the present condition of science demands. The "practice" of gardens has taught in a vague and superficial manner, that certain seeds soon lose their power of germination, others but slowly; that the collecting of seeds, the manner in which they are preserved, transported, and lastly, sown, influence greatly the result of the sowings. It is well known that by a suitable degree of humidity and heat, may be obtained the germination of seeds which otherwise would remain inert or be spoilt. Facts of this kind have resulted from the observation of every horticulturist, and it would be useless to seek to contest them, because the conditions of the sowings vary and are scarcely ever comparative. On the other hand, physiologists have directed attention in their works to the germination of some very old seeds†, but these are isolated cases, perhaps exceptional, and which cannot be compared with each other, since the seeds have been submitted to different conditions.

It appeared to me to be of some interest to ascertain the faculty of germination, after a given lapse of time, in seeds, belonging to different families, but collected simultaneously in the same garden, transported and preserved in the same manner, finally, sown in equal number in similar conditions of soil, humidity and temperature. Well-observed physiological facts have

* From the Ann. des Sc. Nat., Dec. 1846. Translated by Arthur Henfrey, F.L.S. &c.

† DeC. Physiol. Végét. p. 618 *et seq.* Desmoulins, Documents relatifs à la faculté germinative conservée par quelques graines antiques. Pamphlet in 8vo. Ed. 2. July 1846.