that the projectile would be deflected upwards by the resistance of the air, so as to increase the elevation for which the gun was laid. This was named "kite-like action" in England, and Didion remarked that there would be a considerable deviation of the projectile "tant dans le *plan vertical*, que dans le plan horizontal" (*Traité*, xiii. 1860). Suppose now that the range r and the time of flight t, for an elevation α, have been carefully calculated. Then from the range table of the same gun, corresponding to same range r, find t' the time of flight, and α' the elevation. Then if $t=t'$ the coefficients of resistance giving the required time and range, are correct, and $\alpha-\alpha'$ is due to kite-like action. Thus, using the range table of the 4-inch B.L. gun: $v=1900$ f.s.; $w=25$ lbs.; $\alpha=12°$; and $\kappa=0·97$.

(γ)

	Range.	Time of flight.	Elevation.
Calculation ...	$r=5666$ yards	$t=17''·008$	$\alpha=12°\ 0'$
R. Table ..	$r=5666$,,	$t'=16''·964$	$\alpha'=11°\ 26'$
Difference ...	0	$t-t'=0''·044$	$\alpha-\alpha'=0°\ 34'$

Here for a range of over three miles, the calculated and observed times of flight, from point to point, differ by only $0''·044$, a negligible quantity. The resistance of the air must therefore have increased the elevation, for which the gun was laid, by $\alpha-\alpha'=34'$, due to jump and kite-like action, for an elevation of the gun of 11° 26′. The whole range table of the 4-inch B.L. gun was thus treated in 1892 (NATURE, No. 1190). These calculations have recently been repeated for elevations 7° to 20°, and published in my Second Supplement, where all the leading steps in the calculation of the ranges, &c., have been given. It was then found that when an elevation of 7° is given to the gun, 21′ is added to the elevation by kite-like action, so that 7° + 21′ must be used for the elevation when it is required to *calculate* the range and time of flight for an elevation 7° of the gun. The elevation of the gun is given in degrees below, and the addition thereto made by kite-like action and jump is given in minutes, 7° + 21′, 8° + 23′, 9° + 26′, 10° + 29′, 11° + 33′, 12° + 38′, 13° + 45′, 14° + 53′, 15° + 63′, 16° + 74′, 17° + 86′, 18° + 98′.

From the results of calculations of range and time above referred to, I have deduced the following table:—

Range.	Time of Flight.			Elevation.		
Yards.	R.T.	Calculated.	Diff.	R.T.	Calculated.	Diff.
	″	″	″	° ′	° ′	′
4000	10·49	10·35	−0·14	6 21	6 32	+11
5000	14·30	14·19	−0·11	90 12	9 37	+25
6000	18 40	18·51	+0·11	12 36	13 18	+42
7000	23·50	23·59	+0·09	16 32	17 59	+87

showing clearly that both *range* and *time* of flight—given by *experiment* and *calculation*—agree, when a proper allowance is made for jump and kite-like action.

From the fair application of all these tests, it appears that calculated and experimental ranges and times of flight agree perfectly well for all practical purposes. Hence the laws of resistance determined by me—the general tables published by me—and the adaptation by me of J. Bernoulli's method of calculating trajectories are all quite satisfactory. But care will be required not to make my methods responsible, in any way, for the disturbing effects of jump or of kite-like action. Consequently range tables cannot at present be prepared by calculation alone, but when obtained by experiment, they may be tested at any point by the method (γ) already explained.

This chronograph might be used with great advantage to test the shooting qualities of all big guns. For this purpose the elongated projectiles should be provided with heads of similar forms. The charges used should be such as would give the velocity v, for which the gun is to be tested, near the middle screen. Fire each projectile through the equidistant screens till n satisfactory rounds have been obtained. Calculate K_v', K_v'', &c., for each of these rounds. Then the approximate value of $K_v=\frac{1}{n}(K_v'+K_v''+\&c.)$, and the mean error will be an indication of the steadiness imparted by the gun to its projectile, and so on for any number of guns. The shooting qualities of any guns could be compared for the velocity v, by simply comparing the numerical values of K_v given by each gun—the lower the numerical value of K_v the better the shooting. Target practice might be carried on simultaneously with these screen experiments. The best of the guns taken in South Africa should be brought home and tested in the manner above recommended.

August 1901. F. BASHFORTH.

Horn-feeding Larvæ.

So far back as June, 1898, you published in NATURE a short article from my pen dealing with "Horn-feeding Larvæ"; it opened up the question as to whether the larvæ of the insect *Tinea vastella*, Zell. = *gigantella*, Stn. = *lucidella*, WKr., fed on the horns of living animals. I mentioned at the time that Dr. Fitzgibbon, in 1856, brought home from the Gambia two pairs of horns, one belonging to *Kolus ellipsiprymnus* and the other to *Oreas canna*, which he had purchased from the natives; the horns were perforated by grubs enclosed in cases which projected abundantly from the surface of the horns, the blood at the base of the horns not having thoroughly dried up on them when brought to market.

Dr. Henry Strachan, of Lagos, wrote a letter, dated July 22, 1898, which appeared in NATURE, and in that letter he stated that the living horns were attacked and infested with the larvæ, as cocoons and pupæ had been extracted from such horns within an hour of the killing of the animals owning them. This he states on the unimpeachable authority of an officer who made the observation.

During 1899, 1900, and until July of this year, I have travelled very considerably in West Africa, having spent these years in Northern and Southern Nigeria, as well as Ashanti and the hinterland of the Gold Coast; I have made close observation of many species of horned animals, and have spent many days with native big game hunters. I have seen many cases in which the horns of dead animals have been infested with the larvæ of the *Tineidae*, but have never met with it in those of living animals. The natives with whom I have been associated, who are keen hunters and extremely keen observers, assure me they have never seen any protuberances containing grubs on the horns of living animals. During our campaign in Ashanti, I questioned officers who came with troops from all parts of the West Coast as well as the East Coast of Africa; also some from Uganda and the Lakes: they all unhesitatingly say that they have never seen cocoons on living animals, although well acquainted with them on the horns of dead animals. Dr. Fitzgibbon's statement stood alone until Dr. Strachan's letter appeared. I venture to suggest that the point still remains *sub judice*. W. J. HUME MCCORQUODALE.

August 30.

NEW GARDEN PLANTS: A STUDY IN EVOLUTION.

THE appellation "new garden-plants" is rather puzzling to those who are neither botanists nor gardeners, and, indeed, it is used with somewhat different significations by both these classes of experts. Considering that not the least of the many services rendered by the Royal Gardens, Kew, is the annual publication, as an appendix to the *Kew Bulletin*, of a list of "new garden plants," some explanation of what is meant by this designation may not be without interest. Let us take an illustration. The maidenhair tree, *Ginkgo biloba*, was in reality introduced into our gardens in 1750 or thereabouts. But let us suppose for our present purpose that it was introduced only in this year of grace 1901. Would it in that case have any right to be considered a "new plant"? If we look on it as the direct lineal descendant of a tree that grew in Greenland in Miocene times and had its ancestry still further back in the Oolitic period, we could hardly consider it as "new." The only novelty about it would be its introduction into gardens. Similarly, the Welwitschia, now in cultivation at Kew and elsewhere, was, in garden parlance, a new plant. It was new to Welwitsch when he discovered it in the deserts of Mossamedes in South-West Africa, but nobody looking at the uncouth "monster" would deem it new. Rather would he think of it, as he has a clear right to do in the case of the Ginkgo, as a survival from a prehistoric past. Welwitschia has not, so far as we know, been discovered in a fossil state, but if our Antarctic "discoverers" should light upon its traces near the South Pole, no one would be greatly surprised.

In such cases as these, then, it is the introduction into gardens as cultivated plants that constitutes the novelty. And so it is with the hosts of species of orchids, palms, ferns and other plants with which the zeal of botanists or the enterprise of collectors enriches our gardens. Many of these are absolutely new—new to science, that is, as well as new in gardens. Others are novelties so far as the garden is concerned, but have previously been known to, and duly recorded by, the botanist.

But there is still another category of "new garden plants," and one of such vast interest to the student of evolution that we cannot but express our astonishment that so fertile a field of research has hitherto attracted so few labourers. We allude to new plants actually created in gardens by the skill of the gardener. The materials, no doubt, exist in nature, the gardener does but rearrange them, as the milliner forms "ravishing creations" by tasteful intermixture of tulle and ribbon. But the gardener does more than the milliner. He not only effects kaleidoscopic changes of the same materials, but he sets in operation previously pent-up forces—forces which are made manifest in the phenomena of variation, adaptation and progressive evolution. The modern gardener, by means of incessant vigilance and adjustment of the conditions of environment, so far as he is able to do so, *cultivates* the plants committed to his charge so as to obtain the most healthy foliage, the finest flowers or the most luscious fruit according to his particular requirements. But cultivation is not everything. It improves the old, but it does not create the new. Selection, again, by which the gardener profits much, does not in all cases result in absolute novelty, but only in enhanced quality, a lessened amount of variability and a greater degree of fixity or constancy. A seedsman's "stock" of broccoli, or whatever it may be, is carefully "selected" by the choice and retention of what is required and by the rejection or elimination of what is not desired. The "rogues," that is the plants which do not come up to the high standard of perfection, are ruthlessly destroyed. By these procedures, carried on year by year, the stock at length becomes almost absolutely pure, and, what is more, it is kept so because the tendency to vary has become quiescent. Alter the conditions, exercise less vigilance, variation will again set in and the stock become correspondingly deteriorated. Cultivation, selection and elimination tend to preserve the old rather than to create the new.

Novelty in garden plants, apart from the direct importation of new species from foreign countries, is secured in various ways, such as the conservation or selection of variations which originate naturally. By repeated selection and elimination the desired variation is, as we have just said, finally "fixed." It becomes constant and capable of reproduction by seed. Another method of obtaining novelties is by the observation, retention and propagation of bud-variations or sports. A third and most effectual means is secured by the practice of cross-breeding.

Variation in some degree is almost universal; no two leaves on the same branch are alike, the peas in a pod really contradict the meaning of the proverbial adage, for, instead of being strictly alike, they are more or less different. But the discontinuous variation, the "sport" proper, as it is understood in gardens, is the representative of a more pronounced degree of variation—one that occurs suddenly, or at least its earlier manifestations are so inconspicuous as to be overlooked. It appears simultaneously in widely separated areas. It is mysterious in origin as it is striking in appearance. No doubt in many cases this sporting is a reversion to some ancestral condition, or is due to a separation of previously amalgamated characteristics, but what brings about the separation is a mystery. In any case, the gardener has little or no control over the phenomena of sporting; he does but avail himself of what nature provides him without any effort of his own.

It is a very different thing with cross-breeding. The larger number of "new garden-plants" at the present day are due to intentional cross-pollination or fertilisation. All degrees of this process occur from the union of male and female elements from individuals that present the least degree of distinctiveness up to the combination of the sexual elements in plants so wide apart as to be, for practical purposes at any rate, placed in distinct genera, and in one recorded case in a distinct order. Bigeneric hybrids have been recorded between *Philesia* and *Lapageria* (= Philageria × Mast.), between *Urceolina* and *Eucharis* (= Urceocharis × Mast.), between *Rochea falcata* and *Crassula coccinea* (= Kalorochea × Veitch), *Libonia* and *Sericographis* (= Sericobonia ×), between *Montbretia* and *Tritonia*, between numerous genera of Gesneraceæ, between *Scilla* and *Chionodoxa* (= Chionoscilla ×). Amongst orchids no fewer than 150 bigeneric crosses are recorded (Hurst, in *Journal* of the Royal Horticultural Society, 1900, vol. xxiv. p. 102).

In 1849 Donckelaar, the younger, the curator of the Botanic Garden at Ghent, raised a hybrid out of *Gesnera discolor* by pollen of a Gloxinia. This was called *Gesnera Donckelaariana* by Lemaire in the Jardin Fleuriste (1854), t. iv. p. 382. The good faith of the gardener was needlessly and unjustly impugned, and the hybrid nature of the plant was doubted by Decaisne, as was not unnatural at that time. But now that, as we shall presently see, the gardener has succeeded in actually producing by art the same form that exists in nature, there is no more occasion for scepticism.

Decaisne suggested that Donckelaar's plant was no hybrid, but a new species accidentally introduced with other species of Gesneraceæ. This view received confirmation some years later when Messrs. Veitch received from Colombia a plant which on flowering presented all the characteristics of *Gesnera Donckelaariana.* This plant was figured and described as a species by Sir Joseph Hooker in the *Botanical Magazine*, t. 5070 (1858). Several years afterwards (in 1894) Messrs. Veitch produced a hybrid between *Gesnera pyramidalis* crossed with pollen of Gloxinia "Radiance." This received the name of "Gloxinera ×," and is a sufficient proof that bigeneric hybrids may occur in Gesneraceæ. The Gloxinera was figured and described by Mr. J. Weathers in the *Gardeners' Chronicle* (February 2, 1895), and formed the subject of an interesting note from Count de Kerchove de Denterghem in a subsequent number (February 9, 1895, p. 175).

Many other bigeneric hybrids are recorded among the Gesneraceæ, but, until botanists have agreed as to the limitations and nomenclature of genera in this order (which they are far from having done at present), we must suspend our judgment as to the precise status of the numerous hybrids that are alleged to have been raised. For an account of them up to the time of publication, the reader may be referred to Mr. Burbidge's excellent work on "Cultivated Plants, their Propagation and Improvement" (1877), and to Dr. Focke's "Die Pflanzen Mischlinge" (1881), p. 326 *et seq.*

A still further degree of hybridisation is recorded in Maund's "Botanic Garden" (v. p. 468), where a cross from *Digitalis ambigua* (Scrophulariaceæ) by pollen of *Sinningia speciosa* (Gesneraceæ) is described. This, then, was a biordinal hybrid.

Fertile hybrids, the existence of which was once denied, are now too numerous to admit of further doubt. Mr. Hurst, *l.c.*, cites the occurrence of such plants in ninety distinct genera and only four in which the hybrids are quite infertile. Ninety per cent. of some forms of tuberous Begonias come true from seed, as is recorded in Mr. Lynch's excellent paper on the evolution of plants in the *Journal* of the Royal Horticultural Society (vol. xxv. 1900, p. 24). In that paper numerous illustrations are adduced to show that some garden hybrids, perhaps we might say a large proportion, "come true from seed," that is, the parental characters are reproduced in the progeny as markedly as in the case of any so-called species. Bigeneric hybrids are sometimes equally fertile. For instance, there are two Iridaceous genera, Montbretia and Tritonia, so distinct one from the other that they have always been considered as separate genera. Now the plant called *Montbretia crocosmiaeflora* × by Lemoine was raised by that eminent French gardener between *Tritonia aurea*, which furnished the pollen, and *Montbretia Pottsii* as the female parent. This is what M. E. Lemoine says in the volume to which we have just referred (p. 128):—

"It is generally admitted by all that hybrids are, as a rule, either absolutely barren or at most produce descendants as lacking in number as they are also in vigour and in reproductive qualities. Now *Montbretia crocosmiaeflora* × is a hybrid, and by no means an ordinary hybrid, for it is one of the very small group of bigeneric hybrids, its two parents ranking as species of different genera, and yet it has given birth to a long line of vigorous and fertile plants." This hybrid produces seed naturally, but as the progeny is almost identical with the parent form there is no particular object in the gardener raising such seedlings. But when the flowers of this hybrid are pollinated, by pollen taken from either of the original plants, then modification sets in and these modifications have become fixed (see p. 129).

Chionoscilla ×. The hybrid genus between Chionodoxa and Scilla, which occurs spontaneously when the two plants are grown together, is reported by Hurst to have produced fertile seeds.

Whether the facts that some of the so-called genera not only interbreed but "come true from seed" are to be taken as proofs against their autonomy as separate genera or not is a point of the highest interest, to which we can only allude, but which we cannot here discuss. We must be permitted for our present purpose to set aside theoretical considerations and to look on both species and genera as convenient subdivisions necessitated by the requirements of classification, but which, though probably so, are not yet proven to be phylogenetically "natural." All that we are concerned here to assert is that the gardener has succeeded in producing forms as distinct one from another as, often far more so than, those which we call species, and even genera, and which physiologically as well as morphologically "behave" in the same way that species do.

Tuberous Begonias furnish a case in point. They are no older than, scarcely so old as, the middle of the last century. Their history is perfectly well known. They have grown, as it were, under our very eyes. Were it not so there is no botanist who, seeing them for the first time, but would call them new species and think himself very fortunate in getting new species with such definite and easily recognisable marks of distinction. A distinguished French botanist, the late M. Fournier, even constituted a new genus, Lemoinea, to receive some of these widely divergent forms.

But, some will say, these creations of the gardener's skill are not permanent; alter the conditions and they will disappear. Moreover, they can only be propagated by division and not by seed. Were these objections universally true they would, of course, be fatal to our contention. But they are not universally true, and those that are true are just as applicable to natural species. Some at least, as we have seen, have a high degree of permanence, and many are capable of reproduction from seed.

It must not be supposed that these hybrid productions are all of artificial origin. So far back as 1852, Weddell enumerated, in the *Annales des Sciences Naturelles*, numerous natural bigeneric hybrids, and, of course, hybrids between species are now known to occur frequently among wild plants. But what is very interesting in this connection is the fact that gardeners have, over and over again, demonstrated the hybrid nature of certain wild plants by actually producing them artificially. The younger Reichenbach, from his great knowledge and experience, asserted that several orchids examined by him were of hybrid origin. He arrived at his conclusions solely from the observation of morphological characters. But Veitch and many others have since actually created in their orchid houses, by means of cross fertilising the two species, the same form that occurs in nature. They have proved by demonstration what Reichenbach merely conjectured from appearances. An enumeration of these orchid hybrids that have been produced in gardens is given by Mr. Rolfe in vol. xxiv. of the *Journal* of the Royal Horticultural Society, p. 188. Years before Reichenbach, Dean Herbert came to a similar conclusion as to the hybrid nature of certain Pyrenean narcissi, and he too proved the accuracy of his opinion by producing the hybrid form by artificial means. In our own times, Engelheart is doing the same sort of work and arriving at the same conclusions.

In the last class of cases, the gardeners have, as we have said, succeeded in reproducing the identical form that occurs in nature, and that form, of course, cannot be considered in any sense as a *new* garden-plant. But in the other cases mentioned, such as the Begonias, the Streptocarpus, the Clematis, &c., forms have been produced which have not, and could not have, any counterpart in nature. Some of the Andine Begonias very possibly hybridise naturally because they grow in proximity, or at no very great distance from each other. But what are we to say to the new "race" or "species," as we might term it, produced in gardens by fertilising the descendants of these South American Begonias with one discovered in Socotra by Prof. Bayley Balfour? It is hard to conceive of the possibility of a natural hybrid in this case, but, as artificially produced by the gardener, it is one of the greatest ornaments of our hot-houses and much more distinct from other "species" than most of the South American forms among themselves. It is true that in this case, up to this time, the flowers have been mostly sterile, but there are not wanting indications that the sterility may be naturally replaced by fertility, whilst it is certain that the gardener will discover the means to counteract the present nearly barren condition.

It would be easy to multiply instances wherein the gardener has produced new forms morphologically, and in some cases physiologically, worthy of specific or even of generic rank, but it is unnecessary to cite more, as the fact admits of no dispute. We have alluded to them here for the sake of illustrating one category of "new garden plants."

A point of much practical importance arises with reference to the names that should be given to these garden productions. The Kew list to which we have referred takes the names as they are published in the gardening journals, which in their turn copy them from the labels or the catalogues of the horticulturists. The journals are duly cited in the Kew list, but in no case is the author's name mentioned.

In the majority of instances this is the only course that could be advantageously followed, for the names are generally given without adequate research and with no reference to system. They are, in fact, the outcome of the nomenclator's fancy solely. But in many cases the plant is authoritatively described in the gardening periodicals, and when that is the case the customary citation might with advantage be made in the Kew list.

One most objectionable practice the gardeners have, and that is of imitating the names given by botanists *secundum artem.* In the eyes of the scholar, botanical nomenclature is mostly barbarous, but garden nomenclature is too often ludicrous. It is more than that, it is misleading. A botanist ignorant of the history of a garden plant and finding it provided with a Latin generic and specific name would naturally suppose that he had to deal with a species properly described and recorded, and would waste his time and patience in fruitless search unless by good fortune he lighted on the Kew *Bulletin.*

But if some sort of provisional name could be given to plants of garden origin or to plants of unknown status, such name to be so framed as not to give rise to misapprehension, horticulture would not suffer and science—at least indirectly—would be the gainer.

The Royal Horticultural Society has, at various times, endeavoured to grapple with this evil, and has even formulated a code of rules to be followed by the horticulturists when introducing "new" plants to the notice of the Society or the public. The rules are excellent, but they are far more frequently honoured in the breach than in the observance, and the Society seems powerless to enforce its own precepts even in its own records. The alliance of old custom with new developments, however anomalous, seems likely to persist in the future as it has done in the past. The Kew publications to which we have referred are invaluable to the student by lessening the difficulties of research and neutralising the anomalies of which mention has been made.

THE PHOTOGRAPHIC CHART OF THE HEAVENS.[1]

IT is to be regretted that a whole year has been allowed to intervene between the meeting of the International Committee charged with the construction of the photographic chart of the heavens and the official publication of the proceedings of the members, since the interest that would otherwise attach to the utterances of so many expert astronomers in conference assembled is materially lessened by the delay. Doubtless the collection of proofs from sources so scattered and so distant demands a long time, but the most careful and praiseworthy desire to secure accuracy might have been satisfied with a shorter period. Two very evident drawbacks result from this method of treatment. Not only have more or less complete statements appeared in various scientific journals, but the reports on the amount of progress effected by the various participants in the scheme refer to a twelvemonth since and are already ancient history.

But, on the other hand, it is abundantly evident that these meetings, held from time to time, perform a very useful work wherever widespread cooperation is necessary. They not only afford evidence of the earnestness of purpose and determination to successfully prosecute the scheme, that originated under the auspices of the late Admiral Mouchez, but they supply the means of most readily combining the activities of many observatories to secure a common aim. The readiness with which so many astronomers acceded to the request to undertake the observations of Eros, and the adoption of a uniform plan of wide-reaching extent, could scarcely have been effected in the time at disposal without personal intercourse and mutual encouragement. It is true that the observations have all been made and much of the reduction completed before we get the official report, but this in no way detracts from the value of the results immediately obtained, while the proceedings of the Conference will remain as a valuable historical document bearing on the progress of astronomical science.

To the general methods of observation of Eros and the success which has attended the scheme we have already referred (NATURE, vol. lxiii. p. 502), and may pass the matter aside with the reassuring reflection that the latest reports fully confirm the success that was anticipated from the earlier measures. Of the degree of completeness accomplished in the photographic surveys of the heavens it is not easy to form a very exact notion, owing to no tabular statement accompanying the report and the varied methods of description adopted by the various authorities, but the following table will exhibit fairly accurately the amount of progress reported up to the date of the meeting :—

Limits of Zone in declination	Observatory	Number of plates for catalogue	Number of plates for chart	Number of plates measured
90° to 65°	Greenwich	1106	1076	608
64 ,, 55	Rome (Vatican)	476	106	15
54 ,, 47	Catania	Complete	None	36
46 ,, 40	Helsingfors	Complete	⅓	380
39 ,, 32	Potsdam	Complete	None	(100,000 stars)
31 ,, 25	Oxford	Complete	None	736
24 ,, 18	Paris	Complete	97	650
17 ,, 11	Bordeaux	402	17	293
10 ,, 5	Toulouse	½	45	⅛
4 ,, − 2	Algiers	Complete	97	497
− 3 ,, − 9	San Fernando	Complete	596	145
− 10 ,, − 16	Tacubaya	746	None	203
− 17 ,, − 23	Santiago	(Aban	doned)	
− 24 ,, − 31	La Plata	(Aban	doned)	
− 32 ,, − 40	Rio	(Aban	doned)	
− 41 ,, − 51	Cape	Complete	Complete	126
− 52 ,, − 64	Sydney	Complete	(Greater part)	
· 65 ,, − 90	Melbourne	900	Complete	

Of the plates for the chart it is intended that there should be two series, made respectively with one exposure of an hour and three exposures of half an hour each. The word "complete" in the chart column is meant to apply to one of these series, but Sir David Gill has made considerable progress with the second series. The arrangements made for supplying the lacunæ caused by the South American observatories finding themselves unable to fulfil their engagements have already been reported (p. 335).

To judge from the number of papers presented on the determination of photographic magnitude, this subject still seems to occupy a large share of the attention of the Committee—larger, indeed, than to an outsider the subject seems to warrant. On the occasion of the meeting in 1896, the committee decided that the several observatories were at liberty to determine the photographic magnitude, either by estimation or by measurement, simply stipulating that whatever system was adopted it should be one capable of precise definition and permit

[1] "Réunion du Comité international permanent pour l'exécution de la Carte photographique du ciel, tenue à l'Observatoire de Paris en 1900." (Paris: Gauthier-Villars, 1900.)