the plant. The rate of evolution of oxygen indicates the rate of assimilation. Numerous difficulties were encountered in making this method practical; they have been overcome by my automatic recorder. A piece of a water plant, *e.g., Hydrilla verticillata,* is placed in a bottle completely filled with tank-water containing sufficient CO_2 in solution, the open end of which is closed by a special bubbling-apparatus, the bubbler, for measuring the oxygen evolved. The bubbler consists of a U-tube, the further end of which is closed by a drop of mercury acting as a valve. The oxygen evolved by the plant, entering the U-tube, produces an increasing pressure, which eventually lifts the mercury valve and allows the escape of a bubble of gas. The valve then immediately closes until it is lifted once more for the escape of another equal volume of gas. The movement of the mercury completes an electrical circuit, which either rings a bell or makes an electro-magnetic writer inscribe successive dots on a revolving drum (fig. 102). The automatic method eliminates all personal errors of observation; it is so extremely sensitive that it is possible to measure a deposit of carbohydrate as minute as a millionth of a gram. In illustration of the practical working of the apparatus I will give the following example. The plant with the apparatus is so placed as to face the northern light; the bell rings each time it has evolved a certain amount of oxygen representing an equal volume of absorbed CO_2. If a person now stands obstructing the light, the assimilation is slowed down and the bell now strikes at longer intervals. When strong sunlight is thrown on the plant, the successive strokes on the bell become greatly quickened. The plant is such a sensitive detector of light that it may be employed as a photometer for indicating the slightest variations in the intensity of the light of the sky."

I need not point out to the initiated the many individual faults, even errors, in this plausible and very interesting exposition, but certain comments may be made by the way. 1. Whatever may be the usage in India, or elsewhere in the English-speaking world, discussion has demonstrated that *carbon fixation* is a better term than *assimilation,* and that *photosynthesis* is still better because self-descriptive. 2. Water plants probably obtain as much carbon from carbonates and bicarbonates, where they are present, as from carbon dioxide which, in solution in water, may be *called* "carbonic acid." 3. "When sunlight falls upon these plants" much more happens than merely that "carbonic acid is broken up," for—to mention only one thing—the temperature rises, producing purely physical effects in the water, in the plant cells and tissues, and bubbles arise which are not wholly, and may not be even mainly, oxygen. Hence any apparatus devised to demonstrate photosynthesis and depending upon evolution of gas in water of unknown composition, of undetermined temperature, in unmeasured light, should be used for demonstration, graphic representation, but never for one moment considered as *measuring* "a deposit of carbohydrate." This has been recognized for so long in botanical laboratories in this country that the method is employed only on the lecture table, or in elementary laboratory experimentation.

I do not need to multiply quotations. "Resonant recorder," "acuity of perception," "the plant biophytum is found to be eight times more sensitive than a European and four times more so than a Hindu"—these also are fair samples of vocabulary, of deduction, and of aviation. It is a book as dangerous as it is fascinating. Would that it might be followed by a book of equal charm, but exhibiting the respect for the truth which keeps the occidental scientific man from mixing poetry, mysticism and grandiose generalization with his descriptions of the facts of nature! Nature is indeed more wonderful, more beautiful, more impressive than the products of man's imaginings, reflections and theorizings.

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WHEN IS MID-WINTER?

I HAVE long intended to answer the communication by Charles H. Briggs in SCIENCE for April 29, regarding the date of midwinter, but have delayed until I could speak from observational data. I have never before heard the shortest day called the middle of winter. One should hardly expect the coldest weather to fall then, for though it is the day when the hemisphere receives least sunshine, yet the general run of weather should continue to grow colder so long as the solar energy received per day is insufficient to replace the heat radiated to space. For this reason the curve of temperature shows a lag in phase as against that of sunshine.

Our texts of descriptive astronomy and most almanacs tacitly accept the amount of this lag as a month and a half, making the four seasons coextensive with the four quadrants of the sun's apparent motion, thus calling the shortest day the *beginning* of winter. This is an easy way of defining the seasons and one entirely independent of local conditions. Perhaps this last fact is one cause of its apparently wide acceptance.

In addition to the astronomical definition, Webster and other lexicographers give as the "popular" definition of the seasons, groups of three months each, beginning (for the U. S. A.) on March 1, June 1, September 1 and December 1, thereby antedating the astronomical seasons by three weeks. This has the

Station	Latitude	Longitude	Year's Record	Mean °C.	Annual Range °C.	Midsummer	Mid-winter
Estación Misionera	−23° 23′	+58° 25′	4½	24.6	10.1	Jan. 20	July 1
Asunción	25 18	57 40	7	22.8	11.2	" 13	June 30
Salta	24 46	65 24	16	17.4	7.4	" 10	" 30
Tucumán	26 50	65 12	7	18.7	13.2	" 9	" 30
Andalgalá	27 30	66 26	5	19.4	16.1	" 9	July 3
Córdoba	31 25	64 12	20	16.8	13.9	" 14	" 2
Rosario	32 56	60 39	11	17.7	15.0	" 18	" 5
La Plata	34 55	57 56	18½	16.3	14.3	" 21	" 15
Chos Malal	37 27	69 50	4½	14.1	16.1	" 18	" 9
Colonia 16 Octubre	43 5	71 20	5	9.6	15.0	" 22	" 9
Isla de los Estados	54 23	63 47	7	5.7	6.4	" 24	" 22
Isla Laurie	−60 43	+44 47	8	−4.4	1.21	" 6	" 14

qualities of simplicity and independence of local conditions just as fully as the other, and is more convenient for tabular work. Just how popular and widely accepted it is I do not know. The Oficina Meteorológica Argentina uses it, transposed, of course, in all their summaries, but I must confess that I had not heard of this definition till I had occasion to look into their work.

A rational definition of the seasons should be based on the characteristics of the annual temperature curve. This will perforce introduce the local element, but that is not necessarily disadvantageous. Mr. Briggs defines midwinter in a way which seems logically sound and quite acceptable, though the 60° F. is perhaps a bit arbitrary. That his date of midwinter and half the coal supply does not agree with the proverb he cites vitiates neither, for the proverb refers to hay, which with other crops does not become available immediately the cold weather is over, but later in the growing season. Half the store of these should consequently remain some time after midwinter.

Partly to furnish Mr. Briggs data from South America for comparison and partly to clarify my own ideas on the matter, I have summarized the La Plata temperature record[1] and have selected several other stations of wide geographical distribution from among the many discussed in the Anales de la Oficina Meteorológica Argentina. Since some stations have their annual range entirely above and another entirely below the 60° F. used by Mr. Briggs, a departure from his procedure was necessary. I have used as base line the general mean of the station and have defined midsummer and midwinter as the dates whose ordinates bisect the areas between the mean temperature and the observed temperature above it and below it, respectively.

From the table it will be seen that midsummer in Argentina, as determined, agrees closely with midwinter in the Twin Cities as defined by Mr. Briggs. On the other hand, our midwinter is appreciably earlier than the date he deduces for midsummer. There is also an indication of later dates for midwinter as one moves southward while the date of midsummer varies less uniformly and by a less amount.

I remember well the resentment felt as a boy when, on the occasion of a cold snap a week or so before Christmas, one of my elders remarked that winter had not yet begun. Perhaps this started vaguely the idea which has since become a conviction, that in the astronomical definition of the seasons the lag is grossly overestimated. In order to determine its true amount I have considered a tentative definition of the seasons on the basis of the temperature curve as follows:

That part of the curve of annual variation of temperature containing the maximum (minimum) and subtended by a horizontal chord 91 days in length is to be considered summer (winter); the intervening ascending (descending) portion is to be considered spring (autumn.)

Selecting from among the dozen stations used above those five with the longest series and applying this tentative definition to the smoothed ($9c' = a + 2b + 3c + 2d + e$) decade temperatures, I obtain as the first days of summer and winter the dates given below:

Station	Beginning of Winter	Beginning of Summer
Salta	May 17	Nov. 15
Córdoba	May 16	Nov. 30
Rosario	May 21	Dec. 2
La Plata	May 27	Dec. 6
Isla Laurie	May 29	Dec. 19

The progression of summer with latitude is remarkably strong. That of winter is less, but still well marked and in the same direction. Comparing these dates with those of the astronomical definition one sees that only summer at Isla Laurie agrees even

[1] The readings at 7 A. M., 2 P. M. and 9 P. M. over ten (eleven) day intervals were averaged and then the corresponding decades of each year combined.

approximately. Other dates for summer are from two to five weeks earlier and the dates for winter from three to five weeks earlier. Consequently the "popular" definition represents the facts for this Republic for better, and even that overestimates the lag for the northern provinces.

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QUOTATIONS

INTERNATIONAL CONGRESSES

AMONG the many things of value lost through the world war was that informal yet efficient organization known as the International Congress of Applied Chemistry, which was responsible for holding once in three years a scientific conclave, truly international in its attendance, work and publications. Four languages were official—French, Italian, German and English. Representatives on an equal footing came from everywhere and were welcome. Latest accounts of scientific progress furnished the keynote.

How well we remember the last of these international congresses in 1912! There was the gathering in Washington in Continental Hall, where the leader of each national delegation spoke following the playing of his national anthem by the Marine Band. There was a notable afternoon with the President of the United States, the reception, the half-day of sight-seeing and then the special trains to New York where the work of the congress was conducted.

Columbia University and the College of the City of New York fairly swarmed with hundreds of chemists. The meetings, held on the sectional plan according to subject, were open to all and at stated times the congress gathered to hear the principal addresses delivered by representatives of the leading foreign countries. Here we heard the glowing account of the development of the arc process in Norway by Eyde himself. Bernthsen demonstrated that nitrogen and hydrogen could be compelled to combine to form ammonia. Perkin discoursed on synthetic rubber, and the address of Ciamician on photochemistry remains a classic. No one who saw the multitude of products of industrial chemistry which Duisberg brought from Germany will ever forget that occasion in the great hall at City College. Of course there were banquets, sight-seeing, garden parties and receptions, but they were incidental. The congress did real work, as the twenty-nine volumes now on our shelves amply testify.

The International Congress was able to function without a continuous organization and without a paid secretariat and headquarters subject to national influences. The congress decided where its next meeting would be held, selected the man to be responsible at that place and left it to him to form his own organization, work out the details and proceed. The war spoiled the congress planned for 1915, which was to have been in Russia, under the chairmanship of Dr. Walden, the eminent scientist who is the visiting lecturer at Cornell this semester.

It is history that the war gave rise to scientific organizations in several countries, and it is but natural that these should have been the ones to form a new international organization. With the effect of the war still upon them, conditions were at first imposed which prevented the adherence of the former enemy countries to the new union, but fortunately those difficulties have been remedied and any country, the science of which can be represented through a central national body, is welcome.

At first the principal business of the International Union of Pure and Applied Chemistry, which is sponsored by the International Research Council, was the creation of good will and better understandings and beginning anew the promotion of scientific work on a true international basis. Although some committees for scientific work have been formed, it is patent that the union has added little, if anything, to the sum total of scientific knowledge and has devoted itself more to questions of policy and diplomacy through social activities. This has been going on for eight years, but for the last year or two the active members of the union have come to realize that if it is to survive and perform a useful function its program must be changed.

The union is too much restricted in membership and in the number of individuals involved to accomplish its own ends. At present it brings together far too few really to hasten the day of better international relationships. If augmented in numbers it meets too often, and at the basis of it all is the neglect of its real opportunity again to make available the advantages of the world international congress. It is conceivable that some of the work of the union would require the meeting of a small group more frequently than once in three years, provided the union can be looked upon as a sort of nucleus or holding organization to which is entrusted the promotion of chemistry, international so far as the science is concerned. This involves assuming responsibility for a scientific congress to be held very much along the lines of the old international congress.

This subject from time to time has been forcefully brought to the attention of the officials of the union and was discussed at the Washington meeting when Ernst Cohen, the president, stressed the importance of organizing a truly international congress of chemistry along democratic lines. At the recent meeting in Warsaw articles providing for such congresses were pre-