the stratum-interval has by the rarefaction of the gas been raised to twice or three times its first-measured value, then in all the other tubes the intervals have been doubled or trebled.

The tube-diameters in my experiments varied between 2 millim. and 4 centim. The above-mentioned regularity came out, independently of whether the different cylinders formed separate vessels with two metallic electrodes each, or whether, united into a single tube, they were inserted in a line one behind another in the current.

The law is moreover found to hold good equally whether the mean interval be taken from a large or a small number of strata, provided the numbers in the different tubes be equal, although at the same time the absolute value changes. From this it can be concluded that *each single* interval also increases in accordance with the above-mentioned law.

The value of the mean interval is found with great regularity to be, in every tube, as much smaller as the number of strata from which it is derived is greater. Yet the amplitude of the undulation, multiplied by the greatest number of strata employed, never reaches the value of the smallest of the single intervals. Consequently, from the above law it follows that, if in any tube the magnitude of the mean stratum-interval for a series of gas-densities $D_1, D_2 \dots D_n$ is known, and also, for a number of other tubes, each value of the mean interval that corresponds to any one of those densities, the number of the strata which these tubes can show at all the densities from D_1 to D_n can be calculated.

The proportion $\frac{Jd}{J\delta} = \frac{J'd}{J'\delta}$ permits us to conclude that the function according to which the mean interval varies with the gas-density is the same for tubes of different widths. Experiments for the purpose of ascertaining that function gave the following result:—

If the rarefactions of the gas increase in a geometrical series, the stratum-intervals are augmented also, very nearly, in a geometrical series. But the exponents of the two series are not identical; that is, the stratum-intervals are not (as was once maintained by the other side) inversely proportional to the pressure of the gas. On the contrary the measurements prove that the intervals increase much more slowly than the rarefactions—approximately at the rate of $\frac{1}{3}$ when the rarefaction is 3. I will defer more definite statements until I have determined the exponents with the greatest possible exactness, for which I am at present experimenting on a Toepler pump in the form described by von Hagen*.—*Monatsberichte der Kön. Akad. der Wissenschaften zu Berlin*, 1881, pp. 876–878 (separate impression, communicated by the Author).

ON THE ELASTICITY OF RAREFIED GASES. BY E. H. AMAGAT.

This subject has already been treated by Mendeleeff, Kirpitchoff and Hemilian, by Silgerström, and by myself. Those researches

* Wied. Ann. xii. p. 425.

having led to different results, I thought it necessary to resume my experiments, considerably improving my apparatus, especially in what concerns the measurement of the pressures, which is the only difficulty peculiar to this investigation. The method employed having been already described in the *Annales de Chimie et de Physique*, t. viii. (1876), I shall only dwell on the modification which the differential barometer has undergone; it is the essentially delicate part of the apparatus, all the other parts of which have also been considerably improved. This barometer consists of a single glass tube bifurcating, at about 70 centim. above the level of the mercury in the cistern, into two wider cylindrical branches, one of which forms the barometric chamber, and the other is put into communication with the space filled with the gas the pressure of which is to be measured. The immediate result of this disposition is that there is no need to attend to the difference of temperature between the two mercurial columns, which are here joined into one at a little distance below the menisci. The branches of the bifurcation are prolonged upwards by stems of very small diameter, having each a glass cock, and joining again to form a single stem. The rest of the apparatus is disposed so that the manometer can be charged in place by the process generally adopted nowadays, which consists in first exhausting it with a Sprengel pump; this was worked, moreover, during all the time of the filling, so as to maintain the vacuum continually dry by the intervention of a tube containing phosphoric acid. All suction of air through the slender point was avoided by covering the surface of the mercury with a layer of sulphuric acid, which remained in the cistern during all the experiments. Above the lower single branch was a glass cock, by closing which the differential barometer could be transformed into an ordinary truncated barometer, and thus the errors due to variations of the atmospheric pressure be eliminated—which is extremely important.

In order to avoid as far as possible the errors due to refraction and capillarity, the two branches of the manometer, before being soldered to the single stem, were rounded and polished inside with the same copper mandrel, so as to be rendered perfectly cylindrical; a plane facet was then cut on the exterior, quite parallel to the generating lines of the interior cylinder. This done, the pieces were soldered, the necessary precautions being taken to keep the plane facets rigorously in the same plane.

These pieces are very difficult to obtain: a large number of them break or split, either during the rounding, or the soldering, or even after these operations are finished. The cylinders were smoothed and cut by M. Lutz; the manometers were afterwards finished by M. Alvergnyat; that is to say, they were made with all the skill and perfection that could be wished for.

To eliminate capillarity-errors, an internal diameter of 2 centim. was given to the cylinders; and they were evidently perfectly equal: it was easy to verify that the mercury in them was in perfect equilibrium under the thread of the cathetometer.

I shall not dwell upon the precautions which I took with respect to the illumination of the meniscus (by means of a pencil of electric light sifted by passing through a column of water coloured with a little bichromate of potass) in order to be certain of viewing the upper part of it. Here a cause of error exists which is much more frequent than is generally thought, especially when the black silhouette of the meniscus is projected upon a bright ground.

The measurements were performed with a small cathetometer of a peculiar construction, on which $\frac{1}{200}$ millim. could be read off, and which I had made by M. Benevolo, in my private atelier, specially for these researches.

In my first investigation I descended only to 6·5 millim. pressure; this time I have often operated with pressures below 1 millim. I always arrived at the result that the deviation is of the order of magnitude of unavoidable errors. Indeed, for initial pressures of 12 millim. (in round numbers), two series composed of numerous fairly concordant results gave for the value of $\frac{pv}{p'v'}$ (v being sensibly $= 2v'$) the numbers 0·9986 and 1·0020 relative to air; for the initial pressures comprised between 3 and 4 millim. the results varied between 0·9999 and 1·0040; and for pressures near 1 millim. the extremes are 0·999 and 1·015: this divergence corresponds to an error of 15 millim. in the measurement of the pressure. All these numbers are means.

In his experiments M. Mendeleef obtained a series of products pv . This appears a more favourable condition for showing how those products vary. According to him, they go on decreasing with the pressure, starting from a certain pressure which would be 6 decim. for air. In order to prevent any delusion in this respect, it is well to observe that every sensibly constant cause of error in the estimation of the pressures, taking effect upon smaller and smaller pressures, and consequently giving a relatively greater and greater error, will produce the illusion of a regular augmentation or diminution of the products pv . This is what must result, for example, from the want of absolute vacuum in the barometric chamber, even if it be only on account of the effect produced by the mercury vapour.

In short, a minute examination of the possible errors has shown me that, even if one could attain in the readings the precision spoken of by M. Mendeleef (thousandths of a degree, and thousandths of a millimetre), that pressure would be illusory in the presence of errors proceeding from manifold causes—such as the refraction- and capillarity-errors (which, even when the precautions I have indicated are taken, are never absolutely cancelled), the error due to the compelled imperfection of the barometric chamber (which, causing all the pressures to appear a little too low, tends to produce the illusion of a negative deviation), that due to condensation of gases on the sides of the vessels or even on the mercury, &c. &c.

By admitting an error of one or two hundredths of a millimetre,

which is no exaggeration, we arrive at divergences of the order of magnitude of the deviations found; it is therefore impossible to pronounce a decision upon either the direction or even the existence of those deviations. All that we can say is that at the lowest pressures at which experiments have been made (1 millim. or even less; I have experimented at two tenths of a millimetre) no abrupt change in the law of the compressibility of gases appears to be produced; they still follow Mariotte's law, with the exception of divergences for which the experiments cannot be responsible. It is certainly possible that sufficient rarefaction, acting like a great elevation of temperature, would cause other gases to follow the law $p(v-a)=c$, as takes place for hydrogen; but there is a great distance from this to the boundary state spoken of by Mendeleef and Siljerström, in which the gases would become infinitely little compressible—a mere hypothesis, to which the numerical results of M. Siljerström do not even appear to lead, as M. Petier has already remarked in the *Journal de Physique*.

The study of carbonic acid has led me to analogous conclusions. For hydrogen the deviations found have varied between -0.0010 and 0.0028 for initial pressures between 3 and 6 millim. in round numbers.—*Comptes Rendus de l'Académie des Sciences*, Aug. 7, 1882, t. xcv. pp. 281–284.

ON THE INFLUENCE OF TEMPERATURE UPON THE SPECTRA OF METALLOIDS. BY M. D. VAN MONCKHOVEN.

Kirchhoff and Bunsen have shown that the temperature of the flame in which a substance is reduced to vapour has no influence upon the position of the bright lines of its spectrum. When, for instance, sodium or lithium is volatilized in the flame of a spirit-lamp, or in that of the oxyhydrogen blowpipe, the lines remain the same, but their brilliancy increases with the temperature: most frequently some new thin lines appear at the elevated temperatures; but it never happens that those which have already been emitted at lower temperatures disappear. If this is always the case as regards the metallic vapours, it is never so with the lines emitted by the metalloids*. Plücker has in fact shown that oxygen, nitrogen, sulphur, selenium, &c. give two different spectra which have no line in common, according as the spectral tubes containing these substances are heated by the ordinary spark of the electrical machine or by that of a Leyden jar. He admits therefore, and with him nearly every physicist, that certain elementary bodies give, at a high temperature (Leyden jar), a spectrum different from that given by the same body at a low temperature (ordinary spark).

But numerous and varied experiments have proved that we can obtain those spectra called those of *high temperature* at very

* Hydrogen is an exception; but this gas is known to be a true metal, not only as to its chemical properties, but also as to its physical. Hydrogen bears, as regards conductivity of heat and electricity, the same relation to other gases as mercury to the other liquids.