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Replication of Guye and Lavanchy's experiment on the velocity dependency of inertia

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Abstract

In the debate over the circumstances which led to a full understanding and acceptance of Einstein's theory of relativity, historians have in particular investigated the role played by the attempts to corroborate relativity by experimental investigations. We report here on the replication in progress of the celebrated experiment performed on cathode rays by the Swiss physicist Charles-Eugène Guye and his assistant in their attempt to vindicate the relativistic mass formula. Our replication has not only proved helpful in better understanding Guye's experiment. Trying to replicate it, we realized some key issues of Guye's setup and method which put his achievement in a new perspective and offer new hints for its proper historical assessment. In particular, our work suggests the need of a detailed historical investigation of an important experimental issue in Guye's times, the problem of the cathode ray production and control.

Keywords: *Charles-Eugène Guye, relativity, relativistic mass formula, cathode rays*

Introduction

The special relativity theory has been studied from many perspectives by historians of physics. For instance, several studies have considered the circumstances that led to its final acceptance as one of the cornerstones of 20th century physics. In particular, some historians questioned the exact role played by the attempts to corroborate relativity by experimental investigations, in particular the early ones devoted to the study of the velocity dependency of inertia. Some argued that such experiments cannot be considered to have played a role, for they could not distinguish between Lorentz' electromagnetic theory and Einstein's relativity theory (Brush 1999). Though making sense from the perspective of modern philosophy of science, this does not seem to match the statements of the years 1920s and 1930s. Indeed, in textbooks or conferences from the time, one finds that it is often referred to experiments on the velocity dependency of inertia as evidence of the special theory of relativity, and especially to Charles-Eugène Guye's results obtained in the period 1907-1915. Of course, this may be understood observing that *once* the relativity paradigm was accepted, it was then easy for those willing to promote the theory to emphasise (and possibly inflate retrospectively) the support of the experiments in the acceptance of the theory. Nevertheless, a careful historical study of these experiments and their advent as experimental evidence for the special theory of relativity should help understanding what the attitude towards relativity was around 1920. In particular, these experiments have been criticised in the 1930s and 1950s. On what grounds were they then accepted in the late 1910s? Were they as carefully studied (and submitted to critical scrutiny) as Kaufmann's were fifteen years before or were they actually accepted on different grounds not immediately linked to their intrinsic precision?

Guye and Lavanchy's conclusions were first proposed in July 1915 (Guye 1915) and then six years later in a much more detailed account including data that had not been published earlier (Guye 1921). Working with cathode rays, they claimed to have verified

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Lorentz-Einstein's formula with great accuracy. This conclusion was accepted almost immediately and then often quoted. Einstein himself, though known as not taking much interest in the experimental support of his conceptions, wrote eulogistic letters to Guye. The latter's results have been considered as the most accurate evidence for the special relativity theory until 1957, when two physicists, P. Faragó and L. Jánossy (Faragó and Jánossy 1957), showed that one had to revise this conclusion: according to these authors, Guye's data did not offer a reliable enough evidence to distinguish Lorentz-Einstein's formula from Abraham's rival formula[1]. The analysis of Faragó suggests that Guye as well as his reviewers did not analyse cautiously enough the data which actually required more sophisticated treatments. This in turn hints at the claim that Guye and Lavanchy's result was received without much scrutiny because relativity was not, at the time, anymore an issue. One could argue (somewhat provocatively), that in the period when relativity theory was gaining definitive acceptance, there was a definite need, for the sake of the credo that every theory had to be covered by *explicit* experimental evidence, to have someone perform the experiments, even if the underlying issue was already set. Guye and his collaborators just happened to be at the right time to do the job.

Against this, one could argue that there were still opponents to Einstein's views in the 1920s. Those could have used the opportunity to easily attack the theory through a criticism of Guye's experimental conclusion, had the latter been indeed accepted on rather insufficient grounds. The fact that it did not happen suggests then that there were actually solid experimental reasons behind the adherence of the community. However, one could counter argue that precisely no one cared anymore to scrutinise further Guye's results, including relativity foes, because the Einstein's theory was clearly winning the battle and going against it was getting unpopular.

As it appears then, it is necessary to step further into Guye's experiments if one wants to reach a clearer understanding of the reasons why they were first accepted and used as a strong experimental evidence for the special relativity theory. For obvious reasons, a close understanding and evaluation of his results could not be obtained using Guye's accounts only. Other reviews, especially those of Lorentz and Gerlach (Lorentz 1929, Gerlach 1926), could not be of much help either, since they only referred to Guye's descriptions and conclusions without analysing them in an independent way. This is one of the reasons why we decided to complement our investigations with a reconstruction of Guye and Lavanchy's experimental setup in order to experiment on it and then be able to reach a first-hand and better assessment of the specificities of this experiment and of the published accounts. Such a reconstruction had clearly to avoid some easy pitfalls, but we could count here on the growing literature devoted the replication methodology, and the expertise of its main pioneers.

In this paper, we aim at describing the process followed so far towards a replication of Guye and Lavanchy's experiment. It is thus to be taken as a workbench report that may help understanding how a replication work may be undertaken, and how it often raises new questions. Regarding the historical questions raised in the previous paragraphs, no definitive conclusions will be drawn. However, it will be shown that an important outcome of our work has been to broaden our view of the experiment by calling our attention to points we had originally not considered, and that eventually turn out to be most relevant. In the first two sections, the historical context and the experiment will be described. Then we will present the work we have done so far: the reconstruction of the experimental setup, our practice with it, the problems encountered, and our achievements.

A history of Guye's electron mass experiments.

In February 1906 Walter Kaufmann made public his conclusion on the experiments he had been performing since 1901 on the electric and magnetic deflections of β-rays. He claimed that Lorentz' theory of the electron, and thus "the possibility of founding physics on the principle of relative motion", had to be rejected (Kaufmann 1906, p. 534). Soon after, Max Planck, following on Adolf Bestelmeyer's observation (Bestelmeyer 1907) that the electric field in Kaufmann' setup may well have been perturbed because of a deficient vacuum, published a strong criticism of Kaufmann's conclusion, and called for new experiments to decide between Abraham's electron theory and what he called the *Relativtheorie* (Planck 1907).

Among the few physicists who took up this experimental challenge was Charles-Eugène Guye. Professor of experimental physics at the University of Geneva since 1900, he had already written on this issue[2], however referring only to Abraham's theory.

[1] In 1902, Max Abraham was the first to propose a formula for the variation of mass with velocity as a result of his purely electromagnetic conception of the electron. For details, see Goldberg (1970).

[2] See Guye (1906).

Assisted by his student Simon Ratnowsky, Guye designed a new and clever method in order to discriminate between Abraham's formula and Lorentz-Einstein's for the transverse mass[3] of the electron. The experiment started in 1907 and three years later Guye and Ratnowsky claimed to have shown that Abraham's theory had to be rejected; however, they did not feel *yet* confident enough to support Lorentz-Einstein's (Guye 1910). In the meantime, other experimentalists working on this issue published their results[4]. However, all were subject to criticisms, so that M. von Laue could still write in 1913 that: "[from the side of experiments in the dynamics of the electron] the theory of relativity has not received a definitive support yet" (Laue 1913, p. 18).

This state of experimental indecision motivated Guye to start a new experiment in 1913. In collaboration with a new student, Charles Lavanchy, he improved the initial setup in order this time to verify specifically Lorentz-Einstein's formula. Their results were communicated to the *Académie des Sciences de Paris* on July 12, 1915. Guye and Lavanchy announced that: «la formule de Lorentz-Einstein sur la variation de l'inertie en fonction de la vitesse se trouve vérifiée avec une très grande exactitude par l'ensemble de nos mesures». This conclusion was readily accepted by such leading physicists as Lorentz, Langevin, and Einstein himself (Lorentz 1916, Einstein 1997, Langevin 1950). In 1921, Guye published an extended Memoir in which he reviewed his two experiments, and provided a number of yet unpublished experimental values. Most significantly, he even reinterpreted his results viewing them as an indirect proof of the mass energy equivalence formula ($E=mc^2$)[5].

As we see, Guye had needed two sets of experiments to reach a definitive conclusion in favor of relativity. He claimed that the improvements in the second setup were the key to the high accuracy required to corroborate the relativistic formula. Now, one may wonder why Guye started everything again three years later instead of directly improving the first experiment while still on run. Sure enough, we know that Guye's experimental endeavours had been slowed down because of the administrative burden linked to his becoming dean of the Faculty of Science in 1909[6], but one is curious about the exact circumstances that made him stop his first series of runs without trying to push the precision to corroborate Lorentz-Einstein[7]. One also wonders in what sense the second experiment was "better" than the first, and to what extent its "improvements" played a decisive role in the acceptation of Guye's conclusions. Let us keep in mind that in the years separating Guye's two experiments, relativity was gaining momentum, so it is indeed legitimate to question the real reasons behind Guye's two-legged celebrated experimental proof of Einstein's theory. Could it be that Guye realised the importance of a dedicated experimental investigation of relativity only after finishing the first experiment? Trying to answer these questions brings us back to the issues discussed in the introduction: we believe that studying Guye's achievements provides a rich case study well-suited to contribute some new material for a better understanding of the history of the reception of the special theory of relativity.

The method of identical trajectories and the experimental setup.

Let us start with some considerations on the method used in both experiments. The electrons produced in a cathode ray tube are accelerated using a source of high voltage. In order to determine their velocity and the corresponding mass value, one studies their trajectory when passing through deflecting fields. Now, either one deflects the electron beam in a magnetic field and determines independently the accelerating voltage, or one deflects the beam using *both* a magnetic and an electric field. Guye and Ratnowsky preferred to avoid the problematic direct determination of the high accelerating voltage, and hence opted for the second method. The latter requires however in turn the knowledge of the deflecting fields, which is tricky since one has to take into account the non-uniformities in their geometry and intensity[8]. Inspired by a paper of Jean Malassez on the relation between the accelerating voltage and the velocity of the electrons in a cathode ray tube (Malassez 1905), Guye and Ratnowsky found a way of circumventing this problem. They realised that constant deviations of the

3 Abraham was the first to have shown that a velocity dependent mass can be expressed as a symmetric tensor whose decomposition along the direction of motion gives rise to two masses: the longitudinal mass along the direction of motion, and the transverse mass in the perpendicular directions. These two masses are equal for a null velocity.

4 For a review of these experiments, see Miller (1981).

5 This is particularly interesting since, previously, Guye had not ventured into such considerations, viewing his results as contributing to a study of velocity-dependent inertia. This suggests a definite increase in his awareness and his appraisal of the special relativity concepts.

6 This is at least the explanation for the delay given by Guye in his Memoir.

7 We are clearly hitting here on the classical issue of when and why one decides to stop an experiment: our project can then be seen as another case study of this classic problematic, see for instance Galison (1987).

8 This issue had already been raised in Simon's measurement of the charge to mass ratio for cathode rays, in which he chose to map the deflecting field in order to take into account its non-uniformities (Simon 1899).

beam correspond to identical trajectories and that this enables to measure the ratios of two different transverse masses and of the two corresponding velocities without actually knowing precisely the geometry of the fields. Guye and Ratnowsky named their method "the method of identical trajectories" (Guye 1910). Using this method, one can obtain the ratios of the masses μ and μ' and the ratios of the corresponding velocities v and v' measuring only the deflecting voltages V and V', and the intensities I and I', which can be done with great accuracy[9].

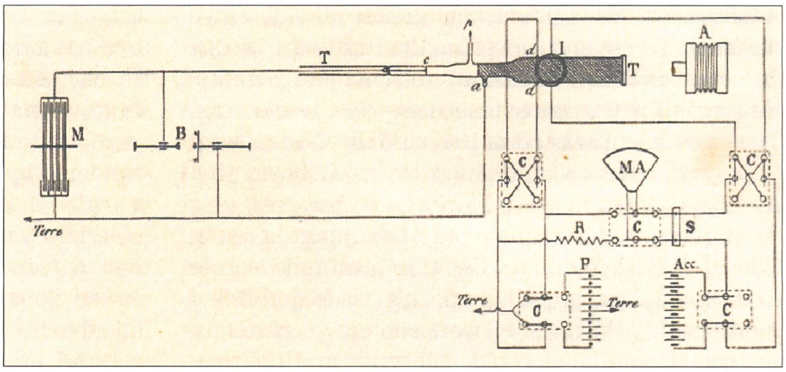


Fig. 1. The schematic of Guye and Lavanchy's experimental setup (extracted from Guye's 1921 memoir).

In the second experiment Guye and Lavanchy did not actually work with exactly identical trajectories but rather with almost identical trajectories. Indeed, in the first experiment, part of the manipulation consists in bringing the spot to a given position through careful tuning of the deflecting voltage and intensity: the longer the time needed to achieve this, the larger the uncertainties due to the inevitable variations in the electron emission. In order to improve the precision, Guye and Lavanchy had then to operate faster. They found a way to do so changing slightly the initial method: they realized that its principle could be applied as well to the case where the deviations were *almost* constant. The necessity to bring the spot back to its position can then be dispensed with, and one records instead its *different* positions with a camera. The ratios of the masses and velocities are then expressed with the following formulas[10].

$$\frac{\mu'}{\mu}=\frac{V I'^2 y^2 x'}{V' I^2 y'^2 x} \qquad \frac{v'}{v}=\frac{V' I\, x y'}{V I' x' y}$$

The schematic of Guye's experimental setup is shown in Fig. 1 (see Fig. 2 and Fig. 3 for pictures of the installation in Guye's laboratory). The cathode ray tube *TT* consisted of three glass parts (see Fig. 4): from right to left, a 3 cm diameter accelerating tube hosting the cathode and the anode, an 8 cm diameter deflecting tube containing the electric deflecting plates, and, on its extremity, a disk screen. The electrodes were 10 cm apart, the deflecting tube was 30 cm long, and the glass parts were fitted together with sealing wax. The electrodes were connected to a Wimshurst type electrostatic machine *M* from Roycourt in Paris. The negative pole was connected to the cathode *c* consisting in a flat aluminum disk, and the positive earth-grounded pole to a brass cylinder anode *a*. At the end of the anode cylinder a 0.2 mm diaphragm enabled to produce a thin beam. The deflection tube contained two horizontal electric plates 4.5 mm apart, connected to a battery *P* delivering the voltage difference *V*. A pair of magnetic coils was placed on each side of the tube, fed with the current *I* of accumulators *Acc*. The screen was covered with calcium tungstate, a fluorescent substance that was then currently used for X-ray screens[11]. This enabled to take pictures of the impacts of the deflected electrons with a camera *A*. The deflecting voltage and the intensity were measured using a high precision Siemens and Halske milli-ammeter *MA*. The deviations were measured on the clichés. All the electric components were controlled with commutators and interrupters *C*. Finally, the tube was placed in the middle of two pairs of frames supporting the magnetic coils designed to cancel the Earth magnetic field. To exhaust the tube, Guye and Lavanchy used a Gaede rotary mercury pump connected through aperture *p*.

Fig. 5, extracted from the *Mémoire*, shows one of the 150 clichés obtained by Guye and Lavanchy: each column of spots on the cliché corresponds to five impact points, respectively, from bottom to top, one magnetic deviation, one electric, the un-deflected beam, the opposite electric deviation, and the opposite magnetic deviation. This is what Guye called one "determination". Each cliché carries from 10 to 18 successive determinations at the same velocity obtained by cumulating the successive columns slid-

[9] In order to be able to derive the dependence between v' and m', it is necessary to know v and m. To be brief, Guye and Ranowsky measured a low velocity v (reference velocity) and calculated the corresponding m according to both theories. Then, the values of m and v' were derived and compared to the theoretical prediction.

[10] x and x' are the electric deviations, y and y' are and the magnetic deflections.

[11] Also known as Scheelite. Its use for fluorescent screens was pioneered by T. Edison in 1896.

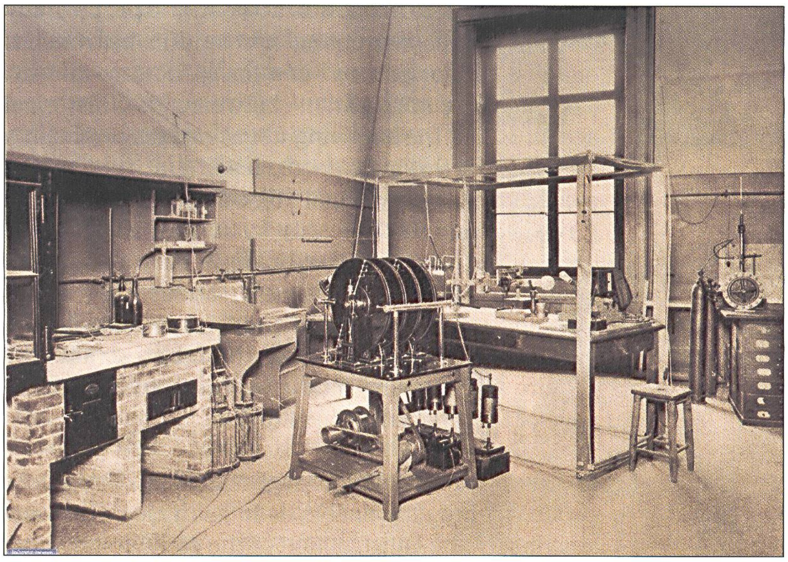


Fig. 2. Guye's experiment (extracted from Guye's memoir).

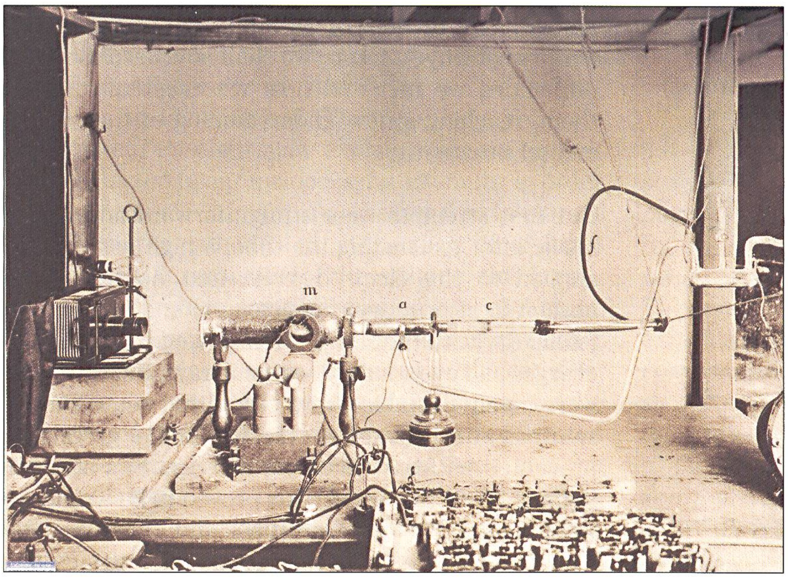


Fig. 3. A detailed view of the cathode ray tube and of the associated devices (extracted from Guye's memoir).

ing each time the photographic plate by a fixed distance. Fig. 6 shows a plot of μ/μ_0 versus β summing up the results: according to Guye, it clearly illustrates the much better agreement with Lorentz-Einstein's formula than with Abraham's[12].

The reconstruction of the experiment.

We started with the reconstruction of the tube since this was clearly the central device of the setup. As we intended first to get familiar with the physics of cathode rays and cathode ray tubes, we put aside, in this first stage of the replication, the reconstruction of the other crucial pieces of the equipment, except for the electrostatic machine on which more later on. In what concerns the design of the tube, we relied on the description given by Guye and Lavanchy, which we confronted with the original, kept in the History of Science Museum in Geneva[13] (see Fig. 4). Each source of information had its advantages and drawbacks: Guye's descriptions proved sometimes wanting, while, on the other hand, the interiors of the original tube could not be examined in full detail because of its shielding that could not be removed[14]. At first, Guye's descriptions appeared to fit quite well the original specimen. However, a more detailed confrontation of both sources revealed some undocumented features and some discrepancies. In particular, the original tube has a side appendix that is never mentioned in any of Guye's papers. Such side appendixes, of common use at the time, were usually intended to contain substances like active coal in order to control the vacuum. Since Guye and Lavanchy stress that they did not need to use such a substance while performing the measurements, it is possible that the active coal served only in the preparatory stages. We included it in our replica but did not use it either. Once we were sure of the required design, we had a copy constructed in the Physics Department facilities of the University of Geneva (see Fig. 8). The initial reconstruction of the tube

[12] There is actually much to say about the construction and the interpretation of this experimental curve: this point was at the center of the criticism of Faragó and Jánossy (Faragó and Jánossy 1957), see introduction.

[13] This tube, together with its sibling probably used in the first experiment, is kept in a specially designed wooden cabinet that has been constructed in Guye's time probably at the latter's own incentive: this corroborates the high awareness Guye had of the importance of his achievement, especially when one examines the description of its content, see Fig. 7.

[14] Also, it appeared to us that most probably the tube underwent some manipulations before being displayed, and/or was slightly damaged afterwards, since some of its inner parts were loose.

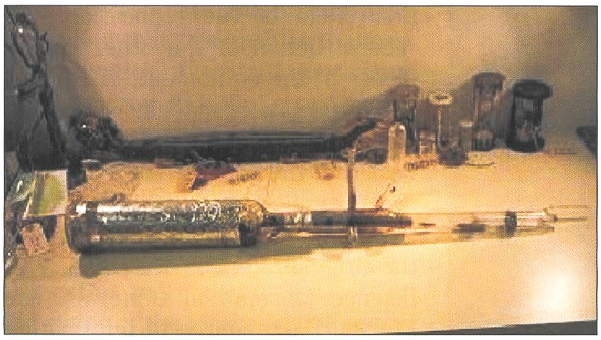


Fig. 4. The cathode ray tube used by Guye and Lavanchy (courtesy of The Geneva Science Museum).

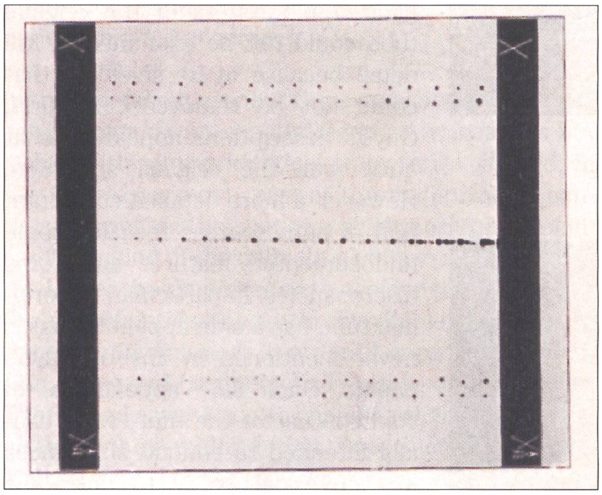


Fig. 5. A cliché recording Guye's "determinations" (extracted from Guye's memoir).

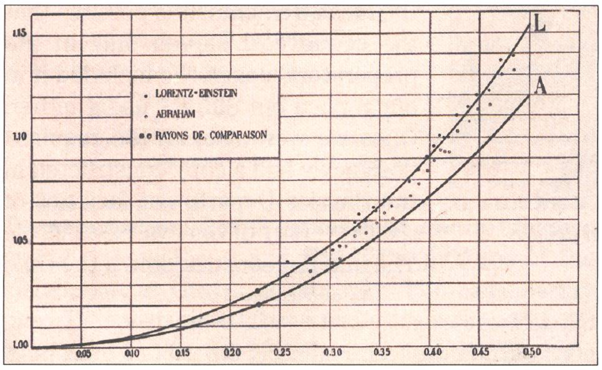


Fig. 6. A plot of Guye and Lavanchy's results (extracted from Guye's memoir).

was easy, but soon it proved necessary to bring some modifications. Indeed, when trying to operate the tube, we realised some important features of its conception that we failed to take into account in the beginning, or which were concealed in the original: many details related to the alignment of the metal parts inside the tube, to the tuning of the distance between the deflecting plates, and to the construction of the electrodes had to be reexamined. We were confronted here to a characteristic aspect of any attempt at reconstructing a scientific instrument: one just cannot proceed in a linear, bottom-up way. The initial confrontation between the historical accounts and the surviving items enables to obtain a blueprint of the instrument, leading at best to the construction a provisional copy. Experimenting with it reveals then the necessity for further adjustments or modifications that could not have been anticipated on the basis of the first observations. The latter, obtained by "unprepared minds" often leads to the neglect of subtle features of the construction that acquire full meaning only after the actual performance of the device has been tested.

As it turned out, going through this stage of the replication did not only enable us to get a clearer understanding of the construction of the cathode ray tube: our whole conception of what was at the core of Guye's experiment changed. We realised that the production of the rays that we initially tended to play down, rather concentrating on their deflection, was actually one of the crucial and most characteristic features of Guye's setup. We shall now describe what difficulties we met and how we eventually solved them, reaching a new understanding of the experimental situation.

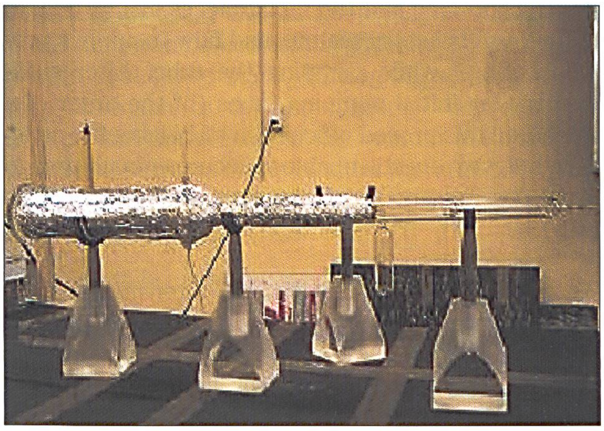
Our first attempts were straightforward and rather naïve: after exhausting the tube, a high voltage was applied to the electrodes. At first no rays were observed; after numerous attempts and some more familiarity with the setup, we managed to obtain discharges and eventually even some rays, but the latter were highly unstable. We thought that some kind of spurious effect related to shielding and/or grounding prevented us from observing the rays in a steadier regime. When this hypothesis proved to be wrong we went back to Guye's descriptions and found that the way the cathode rays were produced was actually described: we had not really paid attention to it before being confronted to the issue itself. Guye and Lavanchy explain it in a few words: the pump had to be stopped once the desired pressure was reached, and then the regime of the electrostatic machine had to be tuned operating its brushes[15]. This had us come back to our setup and try to replicate the procedure described with our own instruments. Little by little, we learned out how the pressure and the voltage were interdependent and how we could handle the delicate interplay within our setup. What we found out, to our surprise, was that, in the range of voltages we were working, the higher the voltage, the lower the pressure.

Further reading taught us that this dependency was actually well known at Guye's time, at least in a phenomenological way. Cathode rays had been discovered through studies of the electrical discharge in rarefied gases during the second half of the nineteenth century. Physicist of the time had an extensive knowledge of the phenomena associated with the discharge, in particular as regards the conditions required for the discharge to take place: there is a definite interdependency between the pressure P, the discharge voltage across the electrodes V, and the distance between them d. This dependency was studied in 1889 by Friedrich Paschen who showed that the discharge voltage depended only on the product $P.d$ (Paschen 1889). Many experimentalists investigated further this law. Guye himself, before his celebrated experiments, had started to study quite exhaustively this dependency at high pressures, a fact which should be clearly linked to his later decision to undertake an experimental study of inertia in response to Planck's call. The reasons behind Paschen's law explain what we have at first neglected in our unsuccessful attempts: in order to accelerate electrons, it is necessary first to extract them from the cathode and, in the absence of direct heating or UV irradiation of the cathode, this is controlled by a rather complex conjunction of factors. The main point is that, in the absence of any other effect able to eject the electrons out of the cathode, the latter are produced because of the impact, on the cathode, of positively charged particles accelerated by the electric force between the cathode and the anode. The emission relies hence on the remaining ionised particles inside the tube. When there are "too many" particles inside the tube, it is necessary to apply a high voltage in order to enable the ions to travel and reach the cathode despite the obstacles of the other particles. When they are "too few", it is also necessary to apply a high voltage in order for the few electrons travelling towards the anode to ionise enough particles that will in turn travel towards the cathode. A minimum discharge voltage exists for intermediate values of $P.d$. This explains the overall shape of Paschen's curve (Fig. 9). Guye and Lavanchy worked within a regime corresponding to the left side on the curve.

Fig. 7. The wooden cabinet with Guye's two cathode ray tubes (courtesy of The Geneva Science Museum).

The mechanism of the emission just described explains most of the difficulties that have to be overcome in order to produce cathode rays in a regime stable enough to perform the measurements: the experimental know-how consists in a careful tuning of the voltage generator (an electrostatic machine), and the *related* control over a high and steady vacuum[16]. However, not any intensity range of the rays is suitable to be able to perform the measurements as we next learned.

Fig. 8. The reconstructed tube.



[15] «Une fois le degré de vide atteint dans le tube, nous interrompions le fonctionnement de la pompe; puis le débit et la tension étaient réglés au moyen d'un système de balais B dont on pouvait faire varier le nombre et l'écartement. [...] Pour chaque vitesse cathodique, nous sommes arrivés, par des tâtonnements souvent très longs, à régler le degré de vide dans le tube, la vitesse de rotation de la machine, et enfin l'écartement et le nombre des balais qui donnaient à l'émission son maximum de stabilité.» (Guye 1916, p. 354).

[16] How steady will be explained below.

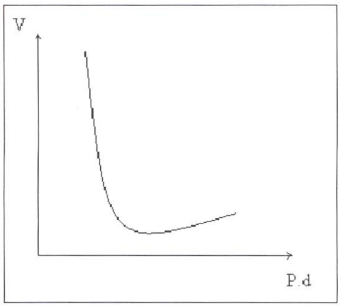


Fig. 9. Paschen's curve.

with lower-current discharges. This amounted to find a way to see the impacts of the electrons on the glass screen for very low intensities. Without the use of the *Scheelite*, the intensity threshold for seeing the spot was about 0.01 mA. When we finally managed to obtain a thin and homogeneous *Scheelite* coating, it became possible to observe the spot for much lower intensities (about 10^{-6} mA). In the end, provided a good vacuum was initially established inside the tube, the rise of the current was substantially slower, so that it was possible to observe a steady emission for up to about 4 minutes. The fluorescence of *Scheelite*, described by Guye as merely convenient in taking pictures of the spots, now appeared crucial for it enabled to work within an adequate discharge regime. This shows that mastering the experiment relied not only on mastering the vacuum (and of course the high-voltage) techniques, but also on using a sensitive screen[19].

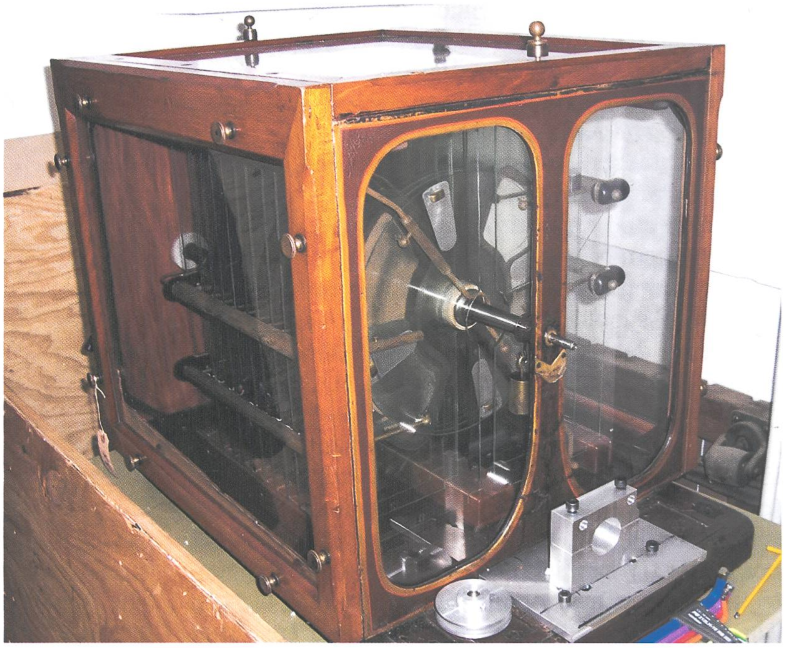


Fig. 10. The Töpler electrostatic machine used by Guye (courtesy of The Geneva Science Museum).

At first we could not manage to observe a spot for more than a few seconds, because the current intensity was rising so fast that the 1 mA limit of our generator was soon reached[17]. What was happening can be understood in the following way. The pressure inside the tube increases rapidly because of the heavy outgassing of the tube and its interiors. The number of free particles available for the emission mechanism increases correspondingly, rising the current until reaching the limit of the generator. The impacts of the electrons on the inner surfaces contribute as well in enhancing further the outgassing: one has then the onset of a mechanism which tends to race. Now, a "natural" outgassing of the tube cannot be avoided, all the more so in a situation where it is not possible to heat the whole tube up to a high enough temperature because of the sealing wax: there is then always a natural increase in the current. The experimental skill consists then in carefully controlling this effect to secure enough time for reliable measurements before the generator limit is reached. Trying to achieve this, we first learned how to produce a "good" vacuum, namely a steady vacuum in the case of a non-operating tube. We learned how to carefully clean the walls of the tube and the metal pieces inside, how to secure a satisfactory sealing, and how to apply a partial heating[18] while pumping the tube. After sufficiently long initial pumping times (of the order of a week), all this proved efficient in stabilizing the emission, but we were still unable to secure enough time to make possible any measurements (at least two minutes).

We eventually found out that the intensity range in which we were working did not allow for more: our initial current values were already too high, leading to a too fast onset of current racing. To further improve the situation, we had to learn how to work

[17] This upper bound is roughly the same as the one Guye was confronted to with his electrostatic machine.

[18] Where there was no sealing wax.

[19] This very point is now under investigation. Indeed, Guye had the possibility to work with especially fluorescent glasses. We do not know whether he did it or not. Neither do we know about the interaction between the coating and the glass with respect to the fluorescence. This is the reason why, by now, we talk of a sensitive screen rather than emphasizing the use of Scheelite.

Postponing any further study of how to control the emission until we could integrate in our setup a genuine electrostatic machine, we concentrated next on the sequence of the manipulations required to obtain the measurements. Here too, we learned from trials and errors. While not using exactly the same setup as Guye's[20], we managed to penetrate the issue of the measurement close enough to be able be evaluate how Guye must have proceeded himself. The measurement consists in taking pictures with 5 to 15 seconds of aperture time for the two opposite magnetic and two opposite electric deflections of the electron beam, the deflecting voltage and intensity being measured throughout the operation. In principle, it is necessary to work in the dark. Fortunately, given the very low sensibility of the photographic plates Guye used, and the time over which the plate were exposed, not a complete darkness is required, so that it is possible to activate the interrupters in a reliable way without any special artifact. An efficient way of going through a measurement cycle nevertheless requires a fairly good organisation: the manipulation of the experimental setup by the experimentalist has to be optimised. So far, we have been able to make about 5 determinations for velocities ranging from 30% to 40% of the speed of light. Guye and Lavanchy claimed to have made 10 to 18 such "determinations" in the range from 25% to 49 %. This difference is easily explained. First, the use of a digital camera prevented us from taking successive pictures as fast as we wished, because of the low acquisition time. Secondly, trying to make measurements clearly revealed that the *Scheelite* deposit was not good enough, as regards the intensity threshold for having a spot that could be photographed. In addition, with our setup, measurements are actually harder to make for relatively low voltages because the size of the spot and its corresponding electronic intensity threshold are more dependent on the homogeneity and thickness of the *Scheelite* coating. This point is particularly important since the measurement of the reference velocity (taken at low voltage, see note 20), fields, and deviations control all the other values. Up to now, we have not been able to measure the reference parameters with less than a 10% error. Although the corresponding error on the reference mass was small, we have calculated that such an error prevented any distinction between Abraham and Lorentz-Einstein. Despite the obvious importance of this point, Guye and Lavanchy did not insist on it, neither did they list it among their experimental achievements. We clearly have to investigate this in more detail, if only to be work out how to interpret this attitude.

As we mentioned before, in our study of the emission of rays and its stability we initially used a modern power supply. Except for the voltage control, the latter did not enable any further tuning of the discharge voltage and of the intensity in a way characteristic of the possibilities offered by a dedicated electrostatic machine with variable collectors. Once we became aware of the difficulties inherent to the peculiar emission mechanism used by Guye and how it was possible to master it, the necessity of understanding better if using an electrostatic machine would provide further control became a priority. The more we were realising how much the whole sequence of manipulations and the overall success of the determinations depended on the emission control, the more important the issue of the voltage source appeared to us. The History of Science Museum eventually granted us the permission to study and possibly restore a machine Guye himself used during the first experiment: a 20 disks Töpler electrostatic machine (see Fig. 10). Although it is not the one Guye had been using during the second experiment, and although it is not enabling a tuning of its performance exactly as described by Guye and Lavanchy, it should eventually help us understanding further what the specific issues related to the use of such a machine are. Initial tests have shown that some of its parts were highly deteriorated. It has since been cleaned, and some of its parts are currently being repaired or reconstructed. Initial tests enabled us to produce cathode rays at about 10kV. After the restoration, we are expecting to obtain cathode rays of higher energy in the range of 40kV[21].

Conclusion.

In the previous section, we described the pathway that we followed in the reconstruction of Guye and Lavanchy's experiment. We have shown how, starting with descriptions and published accounts together with original instruments, we managed to achieve a first stage in the reconstruction of the experimental setup and practice.

Working on the reconstructed setup made it clear that Guye and Lavanchy's experiment greatly relied on mastering the cathode rays emission technique. It consisted in being able to keep the vacuum steady while tuning the electrostatic machine so that a discharge could take place. Having chosen a step-by-step approach to the experiment, we were confronted first to the issue of the vacuum. We observed that the steadiness of the vacuum was affected by the discharge, and that it was easier to achieve for low-current discharges (less than 50 μA). We also

20 We used a simple ammeter to measure the deflecting intensity and voltage and a digital camera to take pictures of the spots.

21 We are relying here on the estimates of Hermann Starke (Starke 1903).

observed that it was indeed possible to observe the impacts of these low-current cathode rays (electrons) on the end of the tube by using a *Scheelite* screen ($CaWO_4$). We eventually managed to measure variations of the inertia for velocities in the range 30 to 40% of the speed of light. However, we could not really perform precision measurements because of difficulties to work with low velocity rays.

Very few historians have focused on cathode rays emission techniques. We, in the course of the replication, simply could not avoid it. It is useful at this point to remember Gerlach's classification (Gerlach 1926) that distinguishes rays emitted through heating of the cathode (*Glühelektronen*), rays emitted through illumination of the cathode by UV light (*Photo-elektronen*) and rays emitted through a "self-discharge" (*selbständig Entladung – Kathoden-strahlen*). All these emission techniques were at the time subject to discussions. Experimentalists working on measurements of the e/m ratio, on the velocity dependency of inertia, and on X-rays all had to choose the emission technique they thought the most appropriate for their purpose. While we do not have yet enough material to attempt a complete survey and comparison of the various emission techniques and their uses during the first decades of the 20th century[22], we certainly think that the experience we gained sheds already new light on Guye's work, and prompts new questions. Indeed, almost all experimentalists working on high-speed cathode rays considered that the "self-discharge" was not an easy way to produce cathode rays compared with the two other techniques[23], and, as a matter of fact, it was even judged inappropriate[24]. Since Guye chose to work with it, we may wonder whether this technique was after all as inappropriate as was thought. Which were Guye's arguments for such a choice? In their accounts, Guye and Lavanchy offer no justification. They only observe that they *managed* to avoid using UV light. This suggests that they thought it was an achievement worth enough to be noticed, but we have failed to understand their reasons so far. Could it be that they considered it important for a better justification of their conclusions?

While we have now reached a clearer vision of the key issues in Guye's experiment, many points still require more investigation: our work is still in progress. Our next steps will be to perform a number of systematic deviation measurements over the whole range explored by Guye and Lavanchy. We shall also find a camera and an ammeter closer to what they used. In the meantime, the Töpler machine will be restored, and will replace the modern generator in the experimental setup.

Most significantly, the replication process had us slightly deviate from our initial questions. The issue of the emission has appeared to be most crucial in the realisation of the experiment. We have been led to ponder upon experimental points like the reference velocity measurement that Guye did not discuss. The replication process has certainly broadened our vision of Guye and Lavanchy's experiment by connecting it to initially unsuspected issues, concealed in the written sources, but nevertheless most relevant as regards our initial historical question about the acceptation of Guye's conclusion and the way it has been advocated.

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[22] We plan to focus on the debate over cathode rays emission techniques in Guye's time in a forthcoming publication.

[23] Hupka (1910) who had worked on the velocity dependency of inertia for high-speed electrons, justified his choice of using UV light rather than the self-discharge claiming that it enabled to accelerate the electrons through a higher potential difference (up to 90 kV).

[24] Proctor (1910) justified his choice to avoid using the self-discharge as a way to produce and accelerate cathode rays by stating that it is "desirable if not essential" that "the discharge may take place in the highest possible vacuum".

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