The Interplay of Theoretical Assumptions and Experimental Practice in the History of 20th-Century Ether-Drift Experiments

Roberto Lalli - Max Planck Institute for the History of Science - rlalli@mpiwg-berlin.mpg.de

Abstract: Historians of physics have paid great attention to the history of the Michelson-Morley experiment and its repetitions in order to elucidate their role in the genesis and widespread acceptance of the theory of special relativity (SRT). This narrow focus of investigation has established a historiographical tradition that looks at the ether-drift experiments from a perspective interested especially in their theoretical interpretations and in the conceptual controversies they sparked. In his examination of Michelson's research activities, the historian of science Richard Staley has held a different perspective exploring the broad network of interests and intellectual commitments that shaped Michelson's early work on the ether-drift experiments. Staley maintains that in order to understand Michelson's contributions it is essential to take into consideration historical issues specifically belonging to the experimental life, such as the relationships with instrument makers and the developments of measuring techniques. In the present paper, I develop Staley's approach focusing on the set of cultural and social stimuli underlying the work of the experimenters who either performed Michelson-Morley-type experiments or conceived novel kinds of ether-drift experiments during the 20th century. I will show that in the experimenters' view of the ether-drift experiments, theoretical commitments were inextricably connected with different factors related to the fragile equilibrium between innovations and traditions in experimental practice as well as in instrument making. I argue that the analysis of such interconnections is indispensable to write a full-fledged history of the 20th-century ether-drift experiments. As a first step in this direction, a survey of the ether-drift experiments and their relationships with both theoretical assumptions and changing experimental practice is exposed. I will show the usefulness of this approach in relation to the analysis of the controversies surrounding the well-known positive ether-drift effect obtained by Dayton C. Miller in the early 1920s.

Keywords: Special Relativity Theory, Experimental practice, Michelson-Morley experiment, Kennedy-Thorndike experiment, Sagnac effect.

1. Introduction

Ether-drift experiments are among the most discussed topics in history of modern physics. Several scholars of different disciplines have explored the role played by the Michelson-Morley experiment (MM) in the genesis and widespread acceptance of Einstein's theory of special relativity (SRT). The general issues these studies have been focusing on concerned the relationship between experimental results and great conceptual changes from different perspectives: historical, epistemological, and sociological (Grünbaum 1961; Holton 1969; Miller 1981; Stachel 1987; van Dongen 2009).

A similar set of questions also shaped the historical scrutiny of the repetitions of the MM experiment performed through the 1920s. The dismissal of the spurious positive result obtained by the American experimental physicist Dayton C. Miller in his repetition of the MM experiment has been considered an important case study to investigate the dynamical and complex relationships between theoretical presuppositions and empirical findings in the reception of innovative theories. Historians, philosophers, sociologists, and even physicists have been interpreting the modalities through which the physics community reacted to Miller's results from various perspectives reaching very different, at times opposite, conclusions (Darwin 1939; Polanyi 1958; Lakatos 1970; Swenson 1972; Lalli 2012). Recall, e.g., the recent controversy stemmed from the analysis of constructivist sociologists of science Harry Collins and Trevor Pinch (Collins, Pinch 1993).

Miller's case, they argued, demonstrates that the majority of physicists had accepted SRT on the basis of an experimental finding – the null result of the MM experiment – that remained actually ambiguous until, at least, the 1960s. Nevertheless, according to Pinch and Collins, the null result of the MM experiment became an important rhetorical weapon to build consensus around the validity of SRT. Pinch and Collins's claims about the lack of clear methodological rules as well as about the relevance of rhetorical strategies of mainstream science received a series of responses by physicists who put the accent on the complex range of motivations – both theoretical and experimental – which persuaded physicists of the validity of SRT independently of the null result of the MM experiment (Dolan *et al.* 1997; Gottfried, Wilson 1997).

In connection to, and perhaps because of, the influential debates on the emergence of novel theories and on the dynamics through which they become mainstream systems of belief, the historical studies on the 20th-century ether-drift experiments have been focusing exclusively on the relationship between the Michelson-Morley-type experiments (MMT) and SRT. This state of affairs has entailed a historiographical approach that has been interpreting the 20th-century ether-drift experiments as an episode internal to the history of SRT. No attention has so far been paid, instead, to the experimental and material culture related to these kinds of experiments.

This theory-driven historiographical approach has been recently criticized by the historian of science Richard Staley (2008) who has followed a different perspective to explore the scientific work of Albert A. Michelson. Staley argued that many different factors played a decisive role in the realization of the 1887 MM experiment: the relationship with instrument makers, the development of novel measurement techniques, and the daily practice with precision instruments. All these factors were specific of the experimental culture. They have no correspondence with the series of activities theorists usually do. In particular, Staley stressed that it is not possible to understand the multifaceted aspects of Michelson's activities without taking into account the historical evolution of the concept of precision and of data analysis practices around the 1880s.

Staley's approach is inspired by the recent turn of the historiography of science, which reassesses experimental physics as an object of analysis that ought not to be subordinated to theoretical developments. The historian of science Peter Galison (1987, 1997) has especially called for a revaluation of this perspective embracing Ian Hacking's (1983, p. 150) motto: "experiments have a life of their own". This expression, in Hacking's and Galison's view, means that experimenters' work has many different targets, which are not limited to simply checking the validity of theories, although it *also* has this role.

This perspective led Staley to conclude that Michelson's work on the 1887 MM experiment was not exclusively aimed at testing the motion of the Earth through the ether. According to Staley, Michelson was much more interested in making the Michelson interferometer – namely, the instrument employed in the MM experiment – an instrument useful in various branches of physics from spectroscopy to metrology. This motivation, Staley's argument goes, is one of main reasons why Michelson did not repeat the MM experiment in various periods of the year as he had planned to do.

Following the same approach, I will explore the set of cultural and social stimuli affecting the work of those experimenters who replied the MM experiment or conceived novel ether-drift experiments. The meaning of the term *ether-drift* has been experiencing tremendous shift since the late 19th century, and have long disappeared from physics literature. It would be, therefore, ambiguous to study the historical development of this kind of experiments through the 20th century before clarifying what one should regard as ether-drift experiments. The first aim of this paper is to propose a tentative definition of ether-drift experiment, which makes the long-term historical scrutiny of this kind of experiments feasible. In the third section, I present a chronology of the experiments performed through the 20th century that are consistent with the aforementioned working definition. Lastly, I focus on Miller's case to ask how the historiographical approach here pursued might give rise to novel interpretations of this controversy. In particular, I make a comparison between Miller's experimental practice and those of other physicists who repeated the MM-type experiment and confirmed the null result, then providing further support for SRT. From these comparisons, I will draw some conclusions about the interconnections between theoretical presuppositions and practical knowledge in the history of the ether-drift experiments and, more generally, on the historical evolution of *epistemic virtues* related to experimental physics (Daston, Galison 2007).

2. Working definition of ether-drift experiments

The first aim of this section is to identify the set of experiments and experimental practices we can refer to when we talk of 20th-century ether-drift experiments. For the reasons summarized in the introduction, historical studies have so far been focusing on the repetitions of the MM experiment as the main examples of 20th-century ether-drift experiments. There is, however, no straightforward rationale that forces us to consider the MMT experiments the only ones which can legitimately bear the name of ether-drift experiments. There were many other experiments performed in the 20th century that might well be considered as ether-drift experiments, and it is necessary to find a general definition, satisfactory from the historiographical perspective. Should we define ether-drift experiments those experiments that have been defined in this way by the physicists who performed the experiments? Or should we call ether-drift experiments those experiments that have been referred to by at least one physicist as a test of the motion of the Earth through the ether? Or, still, should we formulate a criterion independent of the period in which the experiments were actually conducted? For instance, should we consider ether-drift experiments all those experiments that aimed at measuring a possible anisotropy of the velocity of light?

Any choice between these proposals would lead to a completely different classification of the 20th-century ether-drift experiments. Moreover, each of them is questionable for different reasons. From the historiographical perspective, the less problematic choice would be to define ether-drift experiments those experiments that have been explicitly called in this way by the authors of the experiments. As a partial answer, I suggest to start from this definition to isolate a core group of experiments (hereafter called EF1). Definition EF1 appears, however, to be too restrictive. Several recent repetitions of the MM experiment, e.g., do not mention, of course, the word ether. They refer to possible anisotropies of the light velocity or to the breaking of Lorentz symmetry. Taking into account these limitations, I propose to include in the list of 20th-century ether-drift experiments all those experiments that have been explicitly called repetitions of one among the EF1 experiments. Having tried to restrict the possible inferences depending on theoretical presuppositions in the classification of experiments, this choice seems to be adequate to the historiographical approach here pursued. This working definition allows indeed for the study of the historical evolution of experimental strategies and practices linked to a large set of experiments (hereafter called EF2).

3. Chronology of the 20th-century optical ether-drift experiments

The working definition EF2 previously outlined might serve to establish a chronology of ether-drift experiments through the 20th century. It is useful to start, however, with a subset of such experiments to explore the usefulness of this general approach. In this paper, I limit myself to *optical* ether-drift experiments only, leaving aside other kinds of explorations of the motion of the laboratory through the ether.[1] It is also useful to further restrict the criteria in order to consider only those optical experiments that have been repeated a substantial number of times during the 20th century. Only three types of experiments seem to match the definition and criteria here adopted: the MMT

---

[1] I am referring in particular to the electromagnetic Trouton-Noble experiment and its repetitions (Janssen 1995).

experiments, the Kennedy-Thorndike-type (KTT) experiments, and the Sagnac-type (ST) experiments.

The first two kinds of experiments have many features in common. They both were conceived to detect a possible anisotropy of the velocity of light depending on the second order of the quantity V/c, where V is the velocity of the observer with respect to the ether, and c was the speed of light in the ether (now the speed of light in vacuum). More precisely, the anisotropy the experimenters were empirically exploring was an anisotropy of round-trip (or two-way) velocity of light; namely, the average velocity of light from the source to a distant point and back again (Zhang 1997). The third experiment was, instead, conceived to detect an effect depending on the first order of the ratio Ω/c between the angular velocity of a rotating interferometer and the speed of light (Post 1967).

Analyzing the chronology of these kinds of experiments, it is possible to draw some preliminary conclusions concerning the interconnections between theoretical developments and the evolution of particular practices of experimentation in optics. Unsurprisingly, the MMT experiments had by large the predominant role in the history of 20th-century ether-drift experiments. The first physicist to repeat the MM experiment in the 20th century was Edward W. Morley himself, with the assistance of D.C. Miller from 1904 and 1906. Morley and Miller (1905, 1907) interpreted the result of their own experiment as a confirmation of the null result previously obtained by Michelson and Morley in 1887 as well as by Michelson alone in the first experiment of the kind performed in 1881. After the Morley-Miller experiment, there were no further MMT experiments until 1921, when Miller himself began a long series of repetitions. Miller's main motivation to perform again a MMT experiment in the 1920s was related to the widespread favorable reception of Einstein's theories. Miller was disturbed by the enormous success of relativity theories after Eddington's announcement that the 1919 eclipse observations had confirmed the binding of light rays near the Sun predicted by Einstein's general relativity theory (GRT). Miller was especially frustrated by the way in which the null result of the MMT experiments was commonly employed to build consensus on the validity of SRT. Beginning in 1921, Miller argued that the results of the previous MMT experiment had never been null, but only inferior to what expected on the basis on the theories of planetary motion accepted at the time in which the experiments had been performed (Swenson 1972).

After several years of observations Miller (1925) began claiming that his recent experiments confuted the previous interpretation of MMT experiments' results: He had obtained a positive result corresponding to an ether-drift of about 10 km/s. Before Miller had announced his unexpected results, another MMT experiment took place in Heidelberg. In 1924, the German experimental physicists Rudolf Tomaschek reported that he had found no evidence of an ether-drift confirming the predictions of SRT. Like Miller's, Tomaschek's work was also motivated by the widespread acceptance of Einstein's theories in the early 1920s. It was part to the program of Tomaschek's supervisor Philipp Lenard to challenge the relativity theories and restore a pre-Einsteinian worldview (Beyerchen 1977).

Miller's surprising announcements prompted several experimental research projects aimed at testing Miller's conclusions both in Europe and in the United States. The elder

Michelson himself repeated the MM experiment helped by his assistant F. Pearson and the Mount Wilson Observatory astronomer Francis G. Pease (Michelson *et al.* 1929). All these repetitions confirmed the null result of the MM experiment, or, more precisely, did not corroborate the positive result Miller had recently obtained. Among the various repetitions performed in the late 1920s, the Joos experiment (1930) was particularly relevant for at least two reasons. First, it reached a precision concerning an upper limit of the hypothetical ether wind – namely 1.5 km/s – much greater than the ones reached by previous MMT experiments. Second, Joos experiment was the last experiment of the series of MMT experiments devoted to check Miller's claims. In other words, it closed the controversy on Miller's results, at least as far as the experimental testing was concerned (Lalli 2012).

Michelson had a key role also in the first repetition of the ST experiment after the French physicist Georges M. M. Sagnac (1914) had experimentally discovered the Sagnac effect in 1913. In collaboration with the American astrophysicist Henry Gale, between 1921 and 1925 Michelson performed a ST experiment with a large interferometer in order to observe the Sagnac effect (or lack of it) related to the rotational motion of the Earth. The conclusion of the Michelson-Gale experiment was that the observed fringe displacement was of the value expected according to the full Sagnac effect. As Michelson and Gale (1925) recognized, this result excluded that the ether was even partially dragged by the Earth in its rotational motion. In the following years, this conclusion was often employed to contrast the positive result obtained by Miller. Many physicists considered the positive effect observed by Miller – which could be explained only assuming that the ether was partially dragged by the orbital motion of the Earth – as inconsistent with the result of the Michelson-Gale experiment (Joos 1934).

In 1932, Roy J. Kennedy and Edward M. Thorndike performed their new kind of ether-drift experiment, which, according to the authors, confirmed Einstein's formula of the relativity of time. Contrary to what happened for the MM experiment, nobody repeated the KT experiment until 1990. After this date, several such experiments were performed with increasing precision. All these tests corroborated the null-result of the original KT experiment posing an upper limit of 10^{-8} to the anisotropy of round-trip velocity of light (Tobar *et al.* 2010). The reason for the recent interest in KTT experiments is to be found in recent theoretical developments, which predict a breaking of Lorentz symmetry (Colladay, Kostelecký 1997). Until the 1990s, virtually all physicists accepted the null result obtained by Kennedy and Thorndike without any further experimental confirmation.

After World War II, there was a substantial recovery of the MMT experiments (Essen 1955; Jaseja *et al.* 1964; Shamir, Fox 1969). It seems that the motivations underlying this recovery were unrelated to any specific theoretical developments, although they were described as new tests of SRT. Rather, they were conceived and performed as a way to employ new instruments and technologies developed for different reasons. The inventors of new devices were eager to show the qualities of their apparatus by improving the precision of an important experiment such as the MM experiment was. The English physicists Louis Essen (1955), e.g., utilized a microwave resonator invented by him to calculate the refraction index of the atmosphere, while

Charles Townes participated in the 1964 MMT experiment with the Maser he had contributed to develop in the mid-1950s (Jaseja *et al.* 1964).

To summarize, the reasons why experimenters dealt with ether-drift experiments changed markedly during the 20th century. A number of these experiments had the explicit aim to test the validity of SRT. Yet, many of the experiments (e.g., those performed in the period 1950s-1970s) seemed to be concerned mainly with the development and application of new technologies.

4. Experimental practices and the controversy on Miller's results

I turn now to the central part of my contribution, in which I argue that a more detailed study of the experimental culture could help rewriting the history of controversial aspects of 20th-century ether-drift experiments, such as the repetitions of the MMT experiments performed in the 1920s. As I argue elsewhere (Lalli 2012), three key actors played a central role in the controversy about Miller's results: D.C. Miller, A.A. Michelson, and Georg Joos. The aim of this section is to compare the set of practices and skills that shaped their actions and influenced their judgments when they entered the controversy.

4.1. The Morley-Miller experiments

In order to fully understand the experimental tradition to which Miller belonged when he re-proposed the MMT experiment in the 1920s, it is necessary a short analysis of the Morley-Miller experiments. The instrument Morley and Miller employed was simply an evolution of the same apparatus developed by Michelson with the help of Morley in the 1880s. Morley and Miller fully followed the experimental tradition Michelson had inaugurated. The only significant modification was an increasing of the instrumental sensibility due to a longer optical path. All the other elements remained unmodified: the optical parts, the light source, and the mechanism of rotation of the interferometer – namely, the rotation of a massive sandstone block on a tank filled with mercury. By the same token, the observational procedure was entirely preserved. In order to observe the possible ether wind – or, better, its projection on the interferometer's plane – Michelson measured the fringe displacements following the rotations of the interferometer. In case of a relative motion between Earth and ether, Michelson's methodology would have produced a sinusoidal signal periodic in each half turn of the interferometer. Michelson divided the 360 degrees of the circular path of the interferometer in 16 identical parts. One of the two experimenters had to rotate with the interferometer keeping his eye on the eyepiece of the telescope attached to the interferometer. In any of the prearranged 16 points, the observer read the displacement of the central fringe of the interference pattern.

Morley and Miller re-proposed an identical methodology for both the acquisition and reduction of data. They first calculated the average of the 16 displacements among sets of observations (usually 20 turns of the interferometer) with some corrections. The 16 values obtained corresponded to a "single observation" (Miller 1933, p. 212). Later, they applied the Fourier harmonic analysis to each single observation in order to isolate the second harmonic – namely, the sinusoidal signal with period corresponding to half turn of the interferometer.

Miller and Morley also accepted the same theoretical presuppositions underlying the 1887 MM experiment concerning the cosmic motion of the Earth. They, i.e., calculated that the motion of the Earth through the ether was the sum of the Earth's orbital motion, its diurnal rotation, and the Sun's cosmic motion, which at that time was thought to be about 19 km/s toward Hercules constellation.[2]

After having completed their first sets of observations in Cleveland and having reached the conclusion that their data did not show any evidence of the ether-drift effect, Morley and Miller added a further theoretical presupposition, which distinguished their work from that of the previous MMT experiments. Morley and Miller hypothesized that the ether-drag could depend on some experimental conditions. One hypothesis – which was also previously explored by Michelson (1897) in a different experiment – was that the drag of the ether decreased with altitude; namely, Morley and Miller supposed that the ether wind could be greater at high altitude than at the sea level. The second hypothesis was that opaque and solid bodies could not allow the ether to pass through them; namely, that the walls of the laboratory could prevent the experimenters to discover the ether drift. In order to test both these hypotheses, Morley and Miller performed further experiments on the top of the Euclid Heights (now Cleveland Heights), covering the optical paths of the interferometer only with transparent materials. In 1907, Miller and Morley communicated to the scientific community that these latter experiments had also given a null result. However, the authors also reported that they had observed a temperature effect, which they had not been able to remove.

4.2. Miller's experiments

In 1921, Miller resumed the experiment from the exact same point where it was disrupted some 15 years earlier. The theoretical presuppositions were those adopted to justify the experiments on the top of the Euclid Heights in 1906. Miller believed that a higher altitude could favor the detection of the possible decrease of the drag coefficient of the ether with altitude. This reasoning was fundamental in the choice of the location where Miller would perform the experiments: the Mount Wilson Observatory (about 1700 meters above sea level). Miller's belief that the existence of the luminiferous ether was essential to explain the wave phenomena of light was implicit in the decision itself to re-perform a MMT experiment.

This for what concerns Miller's main theoretical presuppositions. To comprehend, instead, the influence of specific experimental practices in shaping Miller's experiments, one has to focus on the apparatus itself and the experimental methodology Miller actually employed in his repetitions. Apparently, Miller did not change much with respect to his

---

[2] This value approximately corresponds to what is now called the motion of the Sun with respect to the Local Standard of Rest, namely the mean motion of the Milky Way stars in the neighboring of the Sun.

previous work. The instrument was the same employed in the last experiments performed with Morley. The procedures of observations and of data reduction also remained unmodified. It is interesting to ask, then, whether there were other elements of Miller's experimental culture that deeply affected the 1920s sets of experiments.

The answer to this question is affirmative, and the way in which these elements entered is revealing of the manners in which daily practices shaped both the data acquisition and their interpretation. After the conclusion of the Morley-Miller experiments, Miller had become one of the leading experts of acoustics in the United States. In 1908, Miller invented the Phonodeik – a sound recording device that converted acoustical vibrations in visual images recorded on a photographic plate. In order to analyze the complex sound waves registered by the Phonodeik, Miller also developed a strong expertise in employing Henrici harmonic analyzers – instruments that determined the Fourier harmonic components of complex periodic curves (Fig. 1). Not only did Miller become one of the major experts in operating such instruments, he also built his own harmonic analyzer, which was able to draw up to 30 Fourier harmonics instead of the 10 of the original instrument. In his publications, Miller often emphasized that the machine was very easy to utilize, and that it allowed for very quick calculation of harmonics (Miller 1916, 1926).

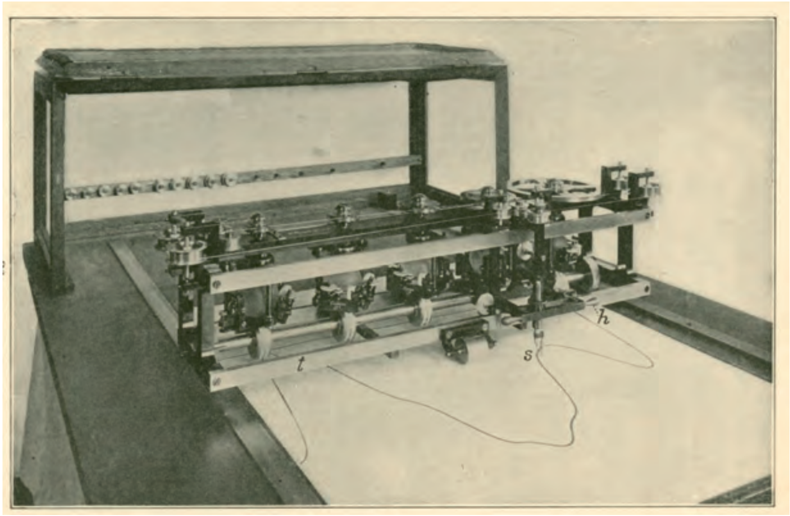


Fig. 1. Miller's Henrici Harmonic Analyzer (Miller 1926, p. 96). Courtesy of the library of the Max Planck Institute for the History of Science

In 1925, Miller began claiming that he had observed a positive effect in the Mount Wilson experiments. This effect, he argued, corresponded to an ether-drift of about 8-10 km/s. No known ether theory could explain the direction and magnitude of the ether wind Miller had allegedly found. For this reason, Miller decided to follow a purely inductive method, without any hypothesis concerning the relative motion of Earth and ether. He made extensive series of observations in several hours of the day and in various epochs of the year. For Miller, the enormous amount of data could allow for an inductive discovery of the direction and magnitude of the ether-drift, and, consequently, of the direction and magnitude of the Earth absolute motion as well as of the direction and magnitude of the ether drag. This procedure, of course, required considerable amount of time to perform the experiments as well as entailed mental and physical efforts by the experimenters. More importantly for my argument, Miller's method required even more time to analyze and reduce the enormous quantity of acquired data. Later, Miller (1933) reported that he had taken more than 200,000 readings of fringe shifts in his 1920s observations. Miller could deal with such a great number of data only because he had at his disposal a quick method for the harmonic analysis of irregular curves: the modified Henrici harmonic analyzer he daily employed for analyzing the sound waves recorded by his Phonodeik. When Miller reported his positive result with the interpretation of the second harmonics, Miller explicitly stated that he had employed this instrument to analyze the data. Without it, we can fairly assume, it would have been practically impossible to calculate the harmonics in a reasonable amount of time. Without the expertise Miller had reached in employing the Henrici mechanical analyzer, the inductive method on which the entire enterprise was based would have been difficult to imagine. In other words, the purely inductive method Miller tried to pursue from 1924 onward not only depended on a set of beliefs concerning the ether and the motion of the Earth through it, but, more subtly, it relied on the mastery of a device that made this method feasible.

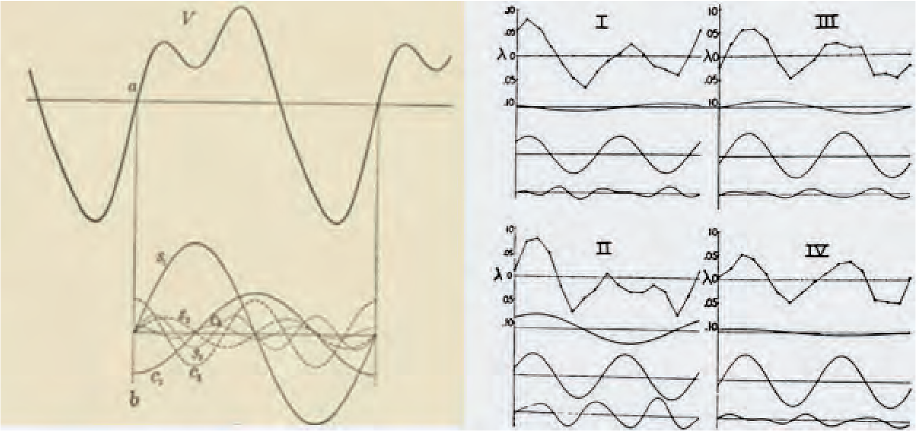


Fig. 2. Comparison between (left) the reduction to harmonics of a violin tone (Miller 1926, p. 101) and (right) the harmonic analysis of Miller's ether-drift observations (Miller 1933, p. 227)[3]

---

[3] Courtesy of the library of the Max Planck Institute for the History of Science for the image on the left. The image on the right is reprinted figure 21 with permission from Miller 1933. Copyright 1933 by the American Physical Society.

4.3. Michelson's interpretations

When Michelson began working on the first MMT experiment in 1881, he was perfectly aware that temperature variations – as well as changes in pressure and humidity – could cause significant fringe displacements in a Michelson-type interferometer. A temperature gradient inside the laboratory entailed a fringe shift periodic in each half turn of the interferometer; namely, the same kind of effect as that produced by a hypothetical ether wind. In his first paper on the subject, Michelson calculated the fringe shift due to the measured temperature, and demonstrated that this background did not affect significantly the experimental results obtained. This calculation, however, was very primitive, and no detailed study was conducted in the following years.[4] This meant that the physicists who worked on the MMT experiments did not share a common certified knowledge on the relevance and influence of the temperature effects in their experiments.

The tacit knowledge related to how temperature affected the observations, however, played a fundamental role in judging the reliability and meaning of observations. Michelson's evaluation of Miller's results is a case in point. After Miller had first announced a positive result, Michelson confessed to the Caltech theoretical physicist Paul S. Epstein that he disapproved of Miller's methodology because Miller did not change the experimental conditions enough (Epstein 1925).

Miller and Michelson also had different judgments about the result of the 1887 MM experiment. After Miller's announcement, Michelson (1925) confirmed that "The *Ether drift* experiments finished with decidedly negative results – probably zero – displacement but certainly less than one thirtieth of that which Miller found". Miller was, instead, contending that the result was positive but smaller than expected on the basis of the accepted theories of Earth motion. This disparity would later become even stronger, when Miller would re-analyze the data of the MM experiment claiming that they confirmed the positive results recently obtained. Evidently, Michelson and Miller held very different opinions on the signal-background relationship in the experiment Michelson had performed in 1887.

This diversity of interpretation was probably due to Michelson's evaluations on the role of temperature effects. That this was the case is suggested by Michelson's interpretation of a periodic effect found in his *own* repetition of the MM experiment performed from 1926 to 1929 (Michelson *et al.* 1929). Michelson was informed that his collaborator Pease had obtained a periodic effect of the type expected – namely, periodic in each half turn of the interferometer – although the displacement was smaller than that reported by Miller. After having examined the data, Michelson answered Pease that the "curious and interesting graphs […] seem to me to indicate *rather clearly* temperature effects" (Michelson 1928, emphasis mine).

In summary, Michelson was always skeptical about Miller's positive results as well as his experimental methodology. It is a well-known historical fact that Michelson had no sympathy for relativity theories, as Michelson's several public statements in favor of the

---

[4] The only detailed study of the temperature effects on the results of MMT experiments was made by Robert S. Shankland and his co-workers in 1954, when they proposed that temperature fluctuations were the main cause of the effect found by Miller (Shankland *et al.* 1955).

ether show (Holton 1969). It is, therefore, illogical to attribute his negative opinion about the positive results obtained by Miller to some theoretical preconceptions. Moreover, Miller and Michelson seemed to belong to the same experimental tradition both in experimental design and in the prominence given to experimental results with respect to theoretical developments. Nevertheless, what for Miller was an ether-drift signal, for Michelson became an artifact due to a systematic error – a background produced "rather clearly" by temperature effects. In his judgments, Michelson relied upon his daily work – the practical knowledge that had led him to develop interpretative skills on how instruments actually worked. Michelson's behavior was based on a level of knowledge that cannot be represented by any explicit equation or logical reasoning. His previous experience in the experimental practices he had employed and developed made him able to decide whether a detected effect was a signal or an artifact due to systematic errors.

4.4. Joos's quest for objectivity

I conclude my analysis of the interplay between theoretical assumptions and experimental practice in the history of 20th-century ether-drift experiments with the comparison between the experiments of Miller and G. Joos. In the design of the experimental set up, Joos implemented some features proper of Miller's experiments, such as the large optical path and the extensive series of observations. Yet, he discarded at the outset crucial theoretical assumptions that had shaped Miller's observational methodology. The result of the experiments of Sagnac and Michelson-Gale led Joos to exclude that opaque and/or massive bodies could drag the ether. Rather, he believed that it was fundamental to adequately shield his apparatus in order to reduce the effects due to temperature, humidity and pressure. Joos hermetically covered the optical path of his interferometer with a metal shell and performed the experiment in a basement. By following this procedure, Joos was in fact assuming that Miller's observations could have been compromised by local factors. Joos's instrument was designed to drastically reduce these factors in the act of observation itself. This method was radically different from that of Miller who, instead, tried to eliminate the background in the data analysis by increasing the number of observations.

Enclosing the optical path in a metallic shell was not the only novelty of Joos's interferometer. Joos also employed an electric motor to rotate the interferometer smoothly, greatly reducing the mechanical vibrations. The last essential difference between Joos's interferometer and Miller's was that Joos employed an automatic camera for acquiring data. Joos was fully conscious of the persuasive power of photographic plates as a way of making the data *objective*. Photos of observations could be checked by virtually everybody wanted to control the result of the experiment, as Joos explicitly stated in his paper (Joos 1930, p. 385; Sichau 2009, p. 85). Once registered on a photographic plate, fringe shifts became immutable object of public knowledge. By making explicit the supposedly intrinsic objectivity of photos, Joos was implicitly referring to the subjective character of Miller's data, which had been observed exclusively by Miller himself.

The demarcation objective/subjective that Joos seemed to refer to in his paper can be further explored by comparing the publications of Miller and Joos. There are indeed remarkable differences between the ways in which they reported their results. When he communicated his positive result to the scientific community, Miller (1925) started with the history of the MMT experiments. He maintained a colloquial style all along his paper, explaining with words both the experimental methodology followed and the results obtained. Miller, moreover, emphasized his personal role in the observations; namely, he explicitly discussed the skills, both technical and ethical, that the experimental physicist should possess in order to successfully perform such a kind of experiment, including the stiff resistance to efforts and the ability to stay focused under stressful conditions.[5]

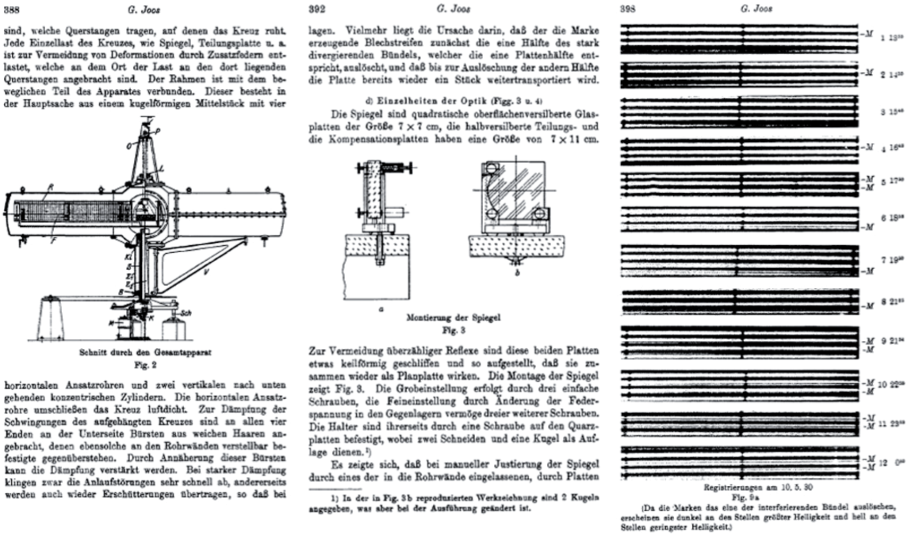
Joos, by contrast, tried to completely disappear from the scientific discourse. The persuasive power of his experiment is left to the instrument, intended as a precision machine. In the pages of his paper, Joos accurately described the instrument or, more precisely, every part of it. Photos of fringes and apparatus' sections became the central actors of the scientific discourse (Fig. 3). Joos never referred to his skills or to his role during the observations, because, it is implied, the machine could do the observations by itself. One can find signs of the experimenter's presence only in one picture, in which his role is to provide a measure of comparison to visualize the impressive dimensions of the apparatus. In Joos's intention the experimental persona becomes subordinate to the apparatus.

The comparison between the procedures employed by Miller and Joos, shows two different modalities of conceiving and practicing experimental physics. By the same token, their published reports expose two very different persuasion strategies. After Joos published his result confirming the null result in 1930, the experimental work aimed at testing Miller's positive result ended. The majority of physicists accepted Joos's results. The end of the controversy on Miller's experiments depended on many different factors, which I have investigated elsewhere (Lalli 2012). One of these factors, I here argue, was related to the changing way in which physicists judged the persuasive power of the arguments employed. In 1930, Joos's methodology and his attention to formulate objective criteria were preferred to Miller's focus on the subjective skills of the specific experimenter who actually performed the observations. This changing attitude toward argumentations was a reflection of the more general

---

[5] One might object that Miller and Joos published in two journals with very different styles and readerships. The former published his discovery in the journal *Science* of the American Association for the Advancement of Science – which was targeted to scientists of various disciplines – while the latter published his article in *Annalen der Physik*, which was one of the most authoritative journals read by physics specialists. The different styles of published reports was therefore also due to the specific features of the journals in which they chose to publish. This objection does not weaken my argument though. In the following years, Miller published only a few papers in *Science* and other journals maintaining the same style and never writing a different kind of report. In 1933, he published a long review of the entire work he had made on the MMT experiments in Reviews of Modern Physics (Miller 1933). In this paper, he clarified some details, but did not modify the general structure of the scientific discourse he had previously employed in his *Science* papers. I would suggest that the different styles of experimental physicists Miller and Joos were embodying also influenced the choice of the journals in which they actually published. I am grateful to Luca Guzzardi for his comment on this point.

historical process toward the depersonalization of physics experimental enterprises, which would shortly develop into those large-scale facility-focused activities that goes under the name of Big Science (de Solla Price 1963; Galison, Hevly 1992).



388 *G. Joos*

sind, welche Querstangen tragen, auf denen das Kreuz ruht. Jede Einzellast des Kreuzes, wie Spiegel, Teilungsplatte u. a. ist zur Vermeidung von Deformationen durch Zusatzfedern entlastet, welche an dem Ort der Last an den dort liegenden Querstangen angebracht sind. Der Rahmen ist mit dem beweglichen Teil des Apparates verbunden. Dieser besteht in der Hauptsache aus einem kugelförmigen Mittelstück mit vier

Schnitt durch den Gesamtapparat
Fig. 2

horizontalen Ansatzrohren und zwei vertikalen nach unten gehenden konzentrischen Zylindern. Die horizontalen Ansatzrohre umschließen das Kreuz luftdicht. Zur Dämpfung der Schwingungen des aufgehängten Kreuzes sind an allen vier Enden an der Unterseite Bürsten aus weichen Haaren angebracht, denen ebensolche an den Rohrwänden verstellbar befestigte gegenüberstehen. Durch Annäherung dieser Bürsten kann die Dämpfung verstärkt werden. Bei starker Dämpfung klingen zwar die Anlaufstörungen sehr schnell ab, andererseits werden auch wieder Erschütterungen übertragen, so daß bei

392 *G. Joos*

lagen. Vielmehr liegt die Ursache darin, daß der die Marke erzeugende Blechstreifen zunächst die eine Hälfte des stark divergierenden Bündels, welcher die eine Plattenhälfte entspricht, auslöscht, und daß bis zur Auslöschung der andern Hälfte die Platte bereits wieder ein Stück weitertransportiert wird.

d) **Einzelheiten der Optik** (Figg. 3 u. 4)

Die Spiegel sind quadratische oberflächenversilberte Glasplatten der Größe 7 × 7 cm, die halbversilberte Teilungs- und die Kompensationsplatten haben eine Größe von 7 × 11 cm.

Montierung der Spiegel
Fig. 3

Zur Vermeidung überzähliger Reflexe sind diese beiden Platten etwas keilförmig geschliffen und so aufgestellt, daß sie zusammen wieder als Planplatte wirken. Die Montage der Spiegel zeigt Fig. 3. Die Grobeinstellung erfolgt durch drei einfache Schrauben, die Feineinstellung durch Änderung der Federspannung in den Gegenlagern vermöge dreier weiterer Schrauben. Die Halter sind ihrerseits durch eine Schraube auf den Quarzplatten befestigt, wobei zwei Schneiden und eine Kugel als Auflage dienen.[1]

Es zeigte sich, daß bei manueller Justierung der Spiegel durch eines der in die Rohrwände eingelassenen, durch Platten

[1] In der in Fig. 3b reproduzierten Werkzeichnung sind 2 Kugeln angegeben, was aber bei der Ausführung geändert ist.

398 *G. Joos*

Registrierungen am 10. 5. 30
Fig. 9a
(Da die Marken das eine der interferierenden Bündel auslöschen, erscheinen sie dunkel an den Stellen größter Helligkeit und hell an den Stellen geringster Helligkeit.)

Fig. 3. Three pages from the paper (Joos 1930, pp. 388, 392, and 398) in which the author meticulously describes some parts of the apparatus and shows the photos of the interference fringes. Courtesy John Wiley and Sons

5. Concluding remarks

The comparison between Michelson's attitude and Miller's suggests that interpretative skills acquired through daily experience played a fundamental role in the experimenters' decision whether an effect was a signal or an artifact. Michelson's judgment on the relevance of the temperature effect did not depend on any detailed theoretical study. His opinion on both the effect observed by Miller and the smaller one emerged in his collaborators' observations was a consequence of tacit practical knowledge, given by his familiarity with specific instruments and experimental procedures.

The comparison between Miller's experiments and Joos's shows, instead, that different theoretical presuppositions concerning the drag of the ether led to two substantially different experimental procedures. Joos excluded some central hypotheses that had shaped Miller's experimental set up. In turn, this distinction depended on two different manners to intend the work of the experimental physicist. Miller pursued a purely inductive methodology in which the theory had to be extrapolated as a generalization of the experimental results. Joos, by contrast, followed a model in which the single experiment must be directly connected to the complex network of theoretical knowledge. The inclination for this more theory-laden approach to experimental work was natural for an expert of theoretical physics as Joos was.[6] In addition, it is fair to say that that Joos's view of experimental physics became largely predominant during the first decades of the 20th century (Galison 1997).

The Joos-Miller comparison also shows a deeper conflict within the evolution of experimental practices; namely, the contrast subjective/objective in the judgment about what counts as *good* experimental work. On the one hand, one finds Miller's experiments, in which the manual and mental skills of the experimenter is a fundamental ingredient for the success of the experiment; in which the qualities of the observer are clearly exposed in the public report of the experimental results, thereby becoming a central element of the argumentative structure of the scientific discourse; in which, then, the qualities of the subject are somewhat identified with the reliability of the experimental results. On the other hand, there is a physicist like Joos who is at ease with the recent advancements of theoretical physics; a physicist who conceived the instruments as a way to eliminate as much as possible of the subjective elements of the experiment; a physicist who worked in closely connection with experts of the Carl Zeiss – a leadership company in the production of precision instruments in optics and mechanics. Joos's experiment, I suggest, was a reification of what has been defined by historians of science Lorraine Daston and Peter Galison (2007) the "epistemic virtue" of "mechanical objectivity".

Such a dichotomy subjectivity/objectivity does not emerge only when one looks at this case through the glasses of historical epistemology. The physicists called to judge the validity of the experiments, implicitly employed such a distinction to explain why a result should be considered more reliable than another. The Mount Wilson Observatory astronomer C. St. John (1932), e.g., referred to several features of Joos's experiment, which I have above summarized, to persuade his readers of the validity of Joos's result and consequently, to close the controversy on Miller's ether-drift effect in the United States. That Joos's contemporaries shortly regarded his instrument as an embodiment of what Daston and Galison call mechanical objectivity seems to be confirmed also by the way in which it became a museum object. When it was donated to the Deutsches Museum in Munich in 1935, the apparatus assumed the name *Zeiss-Joos* interferometer. By giving this name to the apparatus, the curators of the museums were, *de facto*, putting the instrument makers on the same level of the experimenter as co-authors of the scientific enterprise (Sichau 2009).

The analysis here proposed seems, in conclusion, to confirm Galison's statement (1987) that different levels of presuppositions play complex roles in the experimental work. Only a part of these levels is directly related to theories, while others depend on the daily practice of the experimenters. The more tacit presuppositions are difficult to analyze. Nonetheless, they are necessary to understand the complex world of experimentation, from the design of the apparatus to the interpretation of data. This more nuanced view of experimental practice, as I have tried to argue, might increase our understanding of the dynamics of historical controversies on experimental results.

---

[6] Joos had recently published one of the most appreciated textbooks of theoretical physics (Joos 1932).

Bibliography

Beyerchen A.D. (1977). *Scientists under Hitler. Politics and the Physics Community in the Third Reich*. New Haven, CT: Yale University Press.

Colladay D., Kostelecký V.A. (1997). “CPT Violation and the Standard Model”. *Physical Review* D 55, pp. 6760-6774.

Collins H.M., Pinch T.J. (1993). *The Golem. What You Should Know about Science*. Cambridge: Cambridge University Press.

Darwin C.G. (1939). “Logic and Probability in Physics”. *Philosophy of Science*, 6, pp. 48-64.

Daston L., Galison P. (2007). *Objectivity*. New York: Zone Books.

De Solla Price D.J. (1963). *Little Science, Big Science*. New York: Columbia University Press.

Dolan J.F., Griffith B.C., Basile A.G., Kracher A., Burnley R.L., Collins H., Pinch T., Mermin N.D. (1997). “Discussion of Nature of Science Provokes Hit-or-Myth Debate”. *Physics Today*, 50 (1), pp. 11-15.

Dongen (van) J. (2009). “On the role of the Michelson-Morley Experiment. Einstein in Chicago”. *Archive for History of Exact Sciences*, 63, pp. 655-663.

Epstein P.S. (1925). Letter to A. Einstein, 19 September 1925. *Caltech Archives, Papers of Paul Sophus Epstein*, fol. 3.34.

Essen L. (1955). “A New Æther-Drift Experiment”. *Nature* 175, pp. 793-794.

Galison P. (1987). *How Experiments End*. Chicago: University of Chicago Press.

Galison P. (1997). *Image and Logic. A Material Culture of Microphysics*. Chicago: University of Chicago Press.

Galison P., Hevly B.W. (eds.) (1992). *Big Science. The Growth of Large-Scale Research*. Stanford: Stanford University Press.

Gottfried K., Wilson K.G. (1997). “Science as a Cultural Construct”. *Nature*, 386, pp. 545-547.

Grünbaum A. (1961). *The Genesis of the Special Theory of Relativity*, in Feigl H., Maxwell G. (eds.). *Current Issues in the Philosophy of Science. Symposia of scientists and philosophers. Proceedings of Section L of the American Association for the Advancement of Science, 1959*. New York: Rinehart and Winston, pp. 43-53.

Hacking I. (1983). *Representing and Intervening. Introductory Topics in the Philosophy of Natural Science*. Cambridge: Cambridge University Press.

Holton G. (1969). “Einstein, Michelson, and the ‘Crucial’ Experiment”. *Isis*, 60, pp. 133-197.

Janssen M. (1995). *A Comparison between Lorentz’s Ether Theory and Special Relativity in the Light of the Experiments of Trouton and Noble*. Ph.D. thesis, Pittsburgh University.

Jaseja T.S., Javan A., Murray J., Townes C.H. (1964). “Test of Special Relativity or of the Isotropy of Space by Use of Infrared Masers”. *Physical Review*, 133, pp. 1221-1225.

Joos G. (1930). “Die Jenaer Wiederholung des Michelsonversuchs”. *Annalen der Physik*, 399 (4), pp. 385-407.

Joos G. (1932). *Lehrbuch der theoretischen Physik*. Leipzig: Akademische Verlagsgesellschaft.

Joos G. (1934). “Note on the Repetition of the Michelson-Morley Experiment”. *Physical Review*, 45, p. 114.

Kennedy R.J., Thorndike E.M. (1932). “Experimental Establishment of the Relativity of Time”. *Physical Review*, 42 (3), pp. 400-418.

Lakatos I. (1970). “Falsification and the Methodology of Scientific Research Programme”. In Lakatos I., Musgrave A. (eds.). *Criticism and the Growth of Knowledge*. Cambridge: Cambridge University Press, pp. 91-195.

Lalli R. (2012). “The Reception of Miller's Ether-Drift Experiments in the USA. The History of a Controversy in Relativity Revolution”. *Annals of Science*, 69 (2), pp. 153-214.

Michelson A.A. (1897). “The Relative Motion of the Earth and the Ether”. *American Journal of Science*, 3 (18), pp. 475-478.

Michelson A.A. (1925). Undated note sent to the MWO director. Pasadena, CA, Huntington Library, *W.S. Adams Papers*, folder Michelson 1925.

Michelson A.A. (1928). Letter to F.G. Pease, 23 March 1928. Annapolis, MD, US Naval Academy, Nimitz Library Special Collection and Archives Division, *Albert A. Michelson Papers* MS 347, box 1.

Michelson A.A., Gale H.G. (1925). “The Effect of the Earth’s Rotation on the Velocity of Light.” *The Astrophysical Journal*, 61, pp. 140-145.

Michelson A.A., Pease F.G., Pearson F. (1929). “Repetition of the Michelson-Morley Experiment”. *Journal of the Optical Society of America*, 18, pp. 181-182.

Miller, D.C. (1916). “The Henrici Harmonic Analyzer and Devices for Extending and Facilitating its Use”. *Journal of the Franklin Institute*, 182 (3), pp. 285-322.

Miller, D.C. (1925). “Ether-drift Experiments at Mount Wilson”, *Science*, 61, pp. 617-621.

Miller, D.C. (1926). *The Science of Musical Sounds*, 2nd ed. New York: The Macmillan Company.

Miller, D.C. (1933). “The Ether-Drift Experiment and the Determination of the Absolute Motion of the Earth”, *Reviews of Modern Physics*, 5, pp. 203-242.

Miller A.I. (1981). *Albert Einstein’s Special Theory of Relativity. Emergency (1905) and early Interpretation (1905-1911)*. Reading: Addison-Wesley.

Morley E.W., Miller, D.C. (1905). “Report of an Experiment to Detect Change of Dimension of Matter Produced by its Drift through the Ether”, *Science*, 21, p. 339.

Morley E.W., Miller, D.C. (1907). “Final Report on Ether-Drift Experiments”, *Science*, 25, p. 525.

Polanyi M. (1958). *Personal Knowledge. Towards a Post-critical Philosophy*. London: Routledge & Kegan Paul.

Post E.J. (1967). “Sagnac Effect”. *Reviews of Modern Physics*, 39, pp. 475-493.

Sagnac G.M.M. (1914) “Effet tourbillonnaire optique. La circulation de l’éther lumineux dans un interférographe tournant”. *Journal de Physique Théorique et Appliquée*, 4, pp. 177-195.

Shamir J., Fox R. (1969). “A New Experimental Test of Special Relativity”. *Il Nuovo Cimento B*, 62 (2), pp. 258-264.

Shankland R.S., McCuskey S.W., Leone F.C., Kuerti G. (1955). “New Analysis of the Interferometer Observations of Dayton C. Miller”, *Reviews of Modern Physics*, 27, pp. 167-178.

Sichau C. (2010). “Hard Times: The Difficult Lives of Three Instruments in the Museum”. In Morris P.J.T., Stauberman K. (eds.). *Illuminating Instruments*. Washington DC: Smithsonian Institution Scholarly Press, pp. 73-94.

Stachel J. (1987). “Einstein and Ether Drift experiments”, *Physics Today*, 40 (5), pp. 45-47.

Staley R. (2008). *Einstein's Generation. The Origins of the Relativity Revolution*. Chicago: University of Chicago Press.

St. John C.E. (1932). “Observational Basis of General Relativity”. *Publications of the Astronomical Society of the Pacific*, 44 (261), pp. 277-295.

Swenson Jr. L.S. (1972). *The Ethereal Aether. A History of the Michelson-Morley Aether-Drift Experiments, 1880-1930.* Austin: Texas University Press.

Tobar M.E., Wolf P., Bize S., Santarelli G., Flambaum V. (2010). “Testing Local Lorentz and Position Invariance and Variation of Fundamental Constants by Searching the Derivative of the Comparison Frequency Between a Cryogenic Sapphire Oscillator and Hydrogen Maser.” *Physical Review D*, 81 (2), 022003.

Zhang Y.Z. (1997). *Special Relativity and its Experimental Foundations.* Singapore: Worlds Scientific.