THE CORDOBA OBSERVATORY AND THE HISTORY OF THE ʽPERSONAL EQUATION' (1871-1886)

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1. Introduction

The Córdoba Observatory in Argentina, founded in 1871 with state funds, was directed until 1886 by Benjamin Gould, an American trained by German astronomers.[1] His work there began at an interesting time for the basic aims of international astronomy, as astronomers sought to unify astronomical catalogues and maps, previously heterogeneous because they were used in more limited areas. There was, then, an endeavour to get on paper the entire firmament, and for all the world's observatory equipment to use the same technical criteria. This period included the introduction of the electric chronograph to standardize the measurement of time for astronomic observations, the construction of photometers in order to reach a consensus on measuring star brightness, international agreements on common units of measurement and reference points for use in astronomical calculations of terrestrial distances, and conventions regarding astronomical work regulation.[2] At the Córdoba Observatory, the working teams would participate in all these processes, forming a fundamental section in the construction of the "southern sky" scientific object.[3]

The increase in resources invested in astronomy during this process meant not only a proliferation of scientists dedicated to the activity, but also a transformation in their work. Astronomers no longer worked with their own instruments, and their schedules were set by a management hierarchy that organized the teams employed to examine the skies. Some of their skills– and with them the control over their work rate– became obsolete as new instruments were incorporated, while their findings were appropriated and continued by other astronomers. John Lankford shows how as labour was divided and new devices acquired, astronomers became less qualified.[4] Along the same lines, Simon Schaffer comments that in the late nineteenth century, at the Greenwich Observatory, the director sought to hire tireless, reliable, obedient and unqualified workers who were supervised with remarkable discipline.[5] This historical process led Lankford to consider that by the twentieth century even "factory observatories" could be found.[6] Lankford analyses the workings of a large observatory as if it were a company, where the astronomer who directed the institution would have become a manager of sorts, a kind of CEO.

Taking this historical context into account, the attempt by observatory directors to prevent certain errors made by observers included in the ʽpersonal equation' concept was redefined by some scholars as just another step in the drawing up of disciplinary rules for the new division of labour.[7] The tensions that ʽpersonal equations' led to in the observatories of the time have generated controversy among historians, who have either considered them a habitual problem that was standardized and absorbed as a probabilistic aspect in calculations, to which astronomers would never have paid much attention, or, in contrast, as an obstacle to be dealt with and resolved only through rigid discipline and control over observatory assistants. Hoffman, dealing with some aspects of the question raised by Jimena Canales, adds another nuance in pointing out the initial development of the issue[8]. He claims that it is true that, at some point in the early nineteenth century, the way observers made mistakes ceased to be self-evident and became a scientific problem that needed to be investigated. Previously, either the error was absorbed by probabilistic calculations, when it was not constant for each observer, or errors were prevented through training, self-control and better instruments that prevented constant errors –whether those were attributed to insufficient training, misinterpretation of data, or the lack of consideration of a certain anatomic structure of the senses that hindered perception in the recordings. In contrast, the difference that Friedrich Bessel started to detect was involuntary and could not be eliminated even when all precautions were taken. The observer appeared to be incapable of self-control, and this gave rise to a new question regarding the 'personal equation'. Meanwhile, others continued to maintain that experienced observers made fewer mistakes and that these could be adjusted through calculations. But Hoffman maintains that since it was assumed at the time that instruments had to be calibrated, not because of imperfections, but because they were only deemed completed after being put into use, so too were observers seen as something to be calibrated, evaluated, in the specific material conditions of their corporeity and functioning. This analysis treated observers and instruments as one and the same, and so the limits and errors produced by both were investigated at observatories in this period. Personal differences were considered as physical or cerebral differences in the same way that instruments are different in their specific material qualities. At first, Hoffman saw the attempt to define the 'personal equation' as akin to the attempt to determine instrument error. Although he acknowledges the attention Bessel gave to the problem, he argues with the idea that strict discipline was astronomers' response to the problem of the personal equation. He likewise argues with such authors as Peter Galison and Lorraine Daston, who consider that attempts to prevent the 'personal equation' were accompanied by an ideology regarding objectivity that shifted confidence away from observers and into machines and instruments.[9]

According to Hoffman, disciplinary systems are not a direct result of the emergence of the 'personal equation', but a correlation of the continuing division of labour in observatories in the late nineteenth century. By then, says Schaffer, among others, with the incorporation of new instruments, astronomers in the large observatories organized chronometric systems to monitor subordinate observers with the intention of eliminating the 'personal equation'.[10] In this respect, Schaffer notes that the study of the political economy of the great nineteenth century observatories pinpoints the material and social bases of an influential set of values associated with the technology necessary for exercising "moral control" over individual behaviour. In contrast, Hoffman points out that in the early nineteenth century, Bessel suggested that observer errors, the limits of which could not be absorbed by probabilistic calculation, were errors from a physical source, in the sense that they could not be eliminated from the observational work and their margins had to be determined empirically in each case, as was done with instrument errors. No 'moral surveillance' of the problem could solve it; analysis could detect errors, but not eliminate them. Moreover, Hoffman shows the lack of control Bessel has over the understanding of the problem. As he conducted further experiments with different observers, with the same observer at different times and with different instruments, he was perplexed because he had no way to reproduce what he found with final errors; the more he experimented, the less clear he was on what pattern the errors were following. Thus, often the astronomers who followed Bessel paid lip service to the problem, only to then silence it as soon as possible.

As regards the ʽpersonal equation', Hoffman returns to Bachelard´s idea that what occurs here is that the experiment becomes a ʽcomplication', contrary to the traditional notion of experimental results as disturbances of a general law.[11] ʽExperimental facts' reveal the complexity of the object of research. There are no simple phenomena, each phenomenon is a fabric of relations. Notwithstanding, Hoffman believes that, contrary to Bachelard's suggestion, complexity does not lead to a greater understanding of the phenomenon's characteristics, at least not in the sense of a more comprehensive knowledge. Complexity in this case is almost identical to incoherence and, from the point of view of the experimenter, equivalent to a loss of control.

In this article I argue that this problem pinpointed by Hoffman by the early nineteenth century continued in Córdoba as long as until the end of the century, and that the 'personal equation' perspective remained an issue in the involuntary, material and specific observation conditions. I also associate the definition of the 'personal equation' with the complexities of the historical process of the division of labour and changes in material labour conditions. Increases in instruments and resources were not enough to standardize all activities directly; the observer was not immediately replaced by mechanization; observatory practices were still heterogeneous and astronomer qualifications still varied widely. In this context, the sources analysed show how, for the observatory director, the 'personal equation' problem was solved in very different ways, not all of them related to the replacement of the observer by instruments or observer monitoring. In light of these ambiguous responses, the 'personal equation' came to be investigated and redefined. I will also return to Hoffman's questions regarding the history of the 'personal equation', with the positions of Schaffer, Canales, Daston and Galison on the role of the observer in relation to instruments in the organization of scientific work during the nineteenth century.

2. The attempt to homogenise the catalogues.

Five years before the founding of the Córdoba Observatory, most observations that were made for catalogues were not based on a common system of celestial coordinates. Observing the situation in 1883, Rogers presents four issues that contemporary astronomers considered important. First, in general, the tables only covered stars as far as the eighth magnitude, even though instruments already allowed observation of weaker stars. Second, in a large number of cases the same star was repeated in different catalogues, but the same stars of the sector were not repeated because a different selection criteria was used. Third, albeit with some exceptions, there were no common objectives behind the catalogues. Fourth, each catalogue had a particular system of errors according to the observer, the kind of instruments used, and the primary star system selected, but no general effort to homogenise the work done so far.[12]

In this context, in which the absence of homogeneous standards began to be detected as a problem in practices and to be mentioned as an obstacle in some writings, the *Astronomische Gesellschaft´s* proposal to compile a great unified catalogue can be understood. At the 1865 Leipzig meeting, the association's Bessel-trained president Friedrich Argelander conceived a plan to try to eliminate the aforementioned problems in future observations.[13] The proposal eventually became a program, which in its attempt to eliminate error recommended: establishing the initial reference stars distributed uniformly across the northern sky; observing each star two or three times; accepting that the different instruments used require contextual adjustments depending on the observer's criteria; not undertaking observation zones of over two hours, because mental and physical fatigue would lead to errors in the readings[14].

In Córdoba, Gould trained his team to take on the new research guidelines and collaborate with the adjustment of norms proposed for the creation of the southern catalogues. For example, in regarding the suitable width for the observation of the northernmost areas, after much practical experience he decided to preserve the same width as Lalande, Bessel and Argelander. However, as the declination increased, so too did the width have to be increased, otherwise it would diminish when there were too many stars, as in the case of the Milky Way. Regarding the length of the areas, he put forward that two hours was too long, that they should not exceed one hundred minutes, "because observers' eyes and nerves cannot tolerate any more time on the telescope or the microscope (…)Without exception, all of the few areas that were continued for two hours were bad for the observer." [15]

The development of these norms was related to the attempt to eliminate inaccuracies from catalogues. Errors were divided into accidental or periodical and only some of the latter, those that were manifested as deviations from the correct declination of the observed stars, were due to failings in the pivots of the instruments used. The detection and recording of these errors had been known for some time, but they only became a problem when the resources accumulated in observatories increased, the division of labour expanded, and the continuity of observatory work had to be ensured.[16] If errors could not be prevented, the way they were recorded had to be standardized, in order to share, simultaneously or diachronically, the data obtained from local work that were intended to be combined. However, no matter how much astronomers' work was regulated, the problem would persist for years after the *Gesselschaft* plan, and catalogues intended to eliminate all errors could not ultimately prevent them.

3. The 'personal equation', organization of labour and new instruments.

The observer that used the 'eye-and-ear' method recorded the passage of a star by listening to the pendulum of a clock and writing down the times at which he saw a given star pass through the fine vertical and equidistant wires placed on the telescope´s eyepiece.[17] Since its invention in 1849, the electro-chronograph was being used to slowly replace this method and Canales shows how Swiss astronomer Adolf Hirsh was one of the first to test the merits of the new instrument.[18] With this, from 1861 Hirsh tried to solve the problem of the errors of the observers through psychological experiments that could lead to regulating certain behaviour patterns suitable for astronomical practice. He indicated that since alterations came from records made by a single observer, it was not enough to alternate observers for each star or to create an average to obtain exact readings. As Jimena Canales argues, to these astronomers a profound knowledge of human psychology was essential to achieve objectivity in astronomy, which was the condition to share astronomic production and give it continuity.

Furthermore, different perceptions of the colour and brightness of the stars also became problems of ʽpersonal equations'. Since the brightness of stars was one of the fundamental variables in the construction of catalogues, image construction practices were used as photometric references. Like attempts to prevent errors in the definition of celestial coordinates, so too was there an attempt here to eliminate the heterogeneity of individual perceptions, to be able to share the readings obtained. In 1858, Karl Friedrich Zöllner designed an astrophotometer which would become one of the most important instruments of German astronomy in the nineteenth century.[19] The photometer would allow simultaneous observation of an artificial star and a real one through a translucent screen. The artificial star was formed by a light emitted by a flame that passed through a hole in a black cover in the base of the telescope. The cover had holes of different sizes to thus project a standard scale of magnitude, to compare against the brightness of the real stars. As a member of the *Astronomische Gesellschaft,* Zöllner used his own photometer and shared his findings with other world observatories, which sought to obtain the instrument. By 1871, at least half the observatories that participated in international programs had a Zöllner photometer, including the Córdoba Observatory.

The issue of personal equation also formed part of contemporary psychology analyses; in 1887 the American psychologist Edmund Sanford proposed the thesis that the ʿpersonal equation' of astronomers, that he considered the main cause of all errors in the coordinates of stars, should be controlled through knowledge of the laws of experimental psychology. Sanford expected to help astronomers to make use of psychological research, to avoid what he considered "unfounded hypotheses".[20] He maintained that there was a psychological cause for this phenomenon that they were attempting to solve: the impossibility of comparing the impressions of both senses, sight and hearing, which had to take place at exactly the same instant in theʿeye-and-ear' method. Thus, the two impressions had to be combined. In this process, perceiving one impression after the other produced the ʿpersonal equation'. Sanford admitted that it was Bessel who had noticed this before anybody else, and who had taken the first step towards measuring the time occupied by mental processes, in his classic *Memoir on 'Personal Equation'*, but Sanford maintained that Bessel's observation had had no consequences for astronomical work.

What is certain is that the 'personal equation' problem transcended the limits between disciplines, but for astronomers the question had two dimensions: on the one hand, the division of labour within the observatory itself; on the other, the astronomers had to manage to coordinate the task of keeping a record from different points around the world. Moreover, there existed concern that the 'personal equation' was not even constant for each observer. The same astronomer with one instrument could observe differently according to the zenith point the stars were on, and there were also variations depending to their brightness. Bessel, quoted by Sanford, says he first saw the star and then heard the clock, except for when the stars were very weak, so the relation was inverted. To complicate matters even further, this was not valid for all observers. At the Córdoba Observatory the attempt to solve these problems would occupy many hours of the observatory's work and Gould would refer permanently to Bessel's conclusions.

Astronomers, particularly Hirsh, supposed at this time that part of the solution for the variations in the observations lay in the elimination of the observer. The point is highlighted by scholars like Canales and agrees with other analyses for this period, such as those of Daston and Galison regarding the aforementioned change in the way the observer was considered during the nineteenth century.[21] In the case of astronomy, it was hoped that observers would be replaced with photometry, photography and micrometres that recorded and followed the transit of stars. The electric chronograph also encouraged them to think that special skills would no longer be required to achieve homogenous time consumption, in contrast with methods that relied on the senses of sight and hearing for measuring.[22] Those astronomers at the forefront of contemporary astronomical development hoped that the crystallization of human skills in various instruments that could give homogeneous readings would straighten out the problem of differences between observers.[23]

Nonetheless, some years later, in 1897, a veteran of the *U.S. Nautical Almanac* and astronomer at Williams College in Massachusetts called Truman Safford pointed out that the chronograph could not prevent 'personal equations'.[24] Something similar kept occurring, but was then carried over to the different time in which astronomers pressed a given button, which registered the passing of a star according to the chronograph. Safford brought to light the issue that sometimes the new machines produced ‘personal equations’ because of the lack of familiarity with them.[25] This had already been detected in Córdoba, even though Gould held the same hopes for the chronograph as his contemporaries.

It has already been pointed out that Lankford shows that with the accumulation of instruments and the subsequent division of labour in observatories, semi-qualified or unqualified jobs proliferated.[26] However, it was not just a question of devices, which crystallized and homogenised knowledge that was previously embodied in the astronomers, divesting scientists of all qualifications. Firstly, a new skill was necessary to accurately build the devices, and often the devices were built by astronomers or in collaboration with them. Secondly, the astronomer had to acquire the necessary ability to handle and calibrate the new device. Thirdly, and most importantly, some astronomers had to be qualified to coordinate the work of those who handled the devices. They also had to be able to coordinate the products of previous work, done without devices, with the new work and findings obtained. Consequently, the extension of the division of astronomical labour meant disqualification, as Lankford says, but also hierarchization and new qualifications.

The historical cases analysed here are intended to illustrate a chain of problems which is not broken with the introduction and accumulation of instruments and with the subsequent surveillance of astronomers. We will see that at Córdoba, certain problems tied to the lack of familiarity with new devices meant that, among other issues, Zöllner´s photometer was not considered a solution to ‘personal equations’ in the recording of magnitudes, and the introduction of the electric chronograph raised quite a few suspicions about the superiority of the new method in eliminating ‘personal equations’. Evidence against the supposed lineal nature of the process of accumulation of instruments and growing control of labour can be seen in: the many hours of investigation to implement adequate work norms; testing for the utilization of the new devices; modifications of some devices; the impossibility of using others and, above all, the publication of the uncertainties and ambiguities that proliferated in reports on errors.

4. 'Personal equation’, photometry and division of labour in the Córdoba Observatory.

Naked-eye observations for the *Uranometría Argentina* began at Córdoba when the observatory was still being built. The work followed lines Gould had learned while working with Argelander at Bonn. The model resembled his own work at Dudley observatory, which he managed from 1856 after his practice with German astronomers. In Albany, the observatory was still under construction when he had been training observers who assisted him in naked-eye observations. In addition, since 1859 he had been putting his skills into practice with the Coast Survey, in correcting many catalogues and working with modern devices while he made the longitude calculations required of him.

In Córdoba, the space covered by this early research was the part of the sky that was south of the + 10° parallel, covering the region within a radius of 100° from the South Pole. John Hodge states that for the standards of magnitude of this work to be the same as those applied in the northern skies, the first thing Gould had to solve was how to trust four different observers whose different ‘personal equations’ were bound to differ.[27] Referring to the procedure for observing and recording the magnitudes of the first stars and comparing them with the average brightness established by Argelander, Gould wrote:

> This procedure was done independently and separately by each of the four observers; furthermore, after completing each successive region of the aforementioned belt, they compared their results together, not just with each other, but with the sky. The discrepancies turned out to be notably few and minor, in some cases disappearing when reviewed together. Seldom did they go up to two tenths of a unit, nor was there in general a systematic variation. The cases with a regular excess or lack in appreciation were effectively limited mostly to stars with a pronounced colour or which were close to another, brighter star. (…) Only those stars on which the four observers agreed were adopted as magnitude types, discarding all others on the list.[28]

Later, Gould reiterated that the average brightness corresponding to each order of magnitude in *Uranometría Nova* had to be determined independently by each observer and proven by unanimous agreement between them. This meant that at the primary stage, which consisted of establishing the magnitude type stars, which would serve as a reference for establishing the rest of the degrees, the magnitude equation was solved by eliminating the stars which presented discrepancies in their records. Although Gould said the discordances were few and minor, this elimination was be no means marginal: of over 1800 stars noted, there was only absolute agreement between the four observers on 722, so a reading could not be agreed for most stars. The solution proposed by Gould only made it easier to establish some stars chosen for reference. However, the problem would resurface, because the long-term plan was to catalogue and map all stars visible to the naked eye, not simply to leave a map of the stars whose brightness had been agreed upon. The interesting thing about the previous quote is that it shows very clearly Gould´s tendency to attribute personal differences to accidental errors, which could be solved through re-examining that which had presented some sort of ambiguity in the readings.

Firstly, Gould supposed that the source of the error could derive from the proximity of bright stars immediately to the one being observed; in second place, he stated that it was more complicated to determine the brightness of stars of certain colours because it was more difficult for the human eye to perceive them. Though the former problem was more difficult to solve with the devices available at the time, the director counted on the use of telescopes to achieve a more precise record in re-observations. When doing the final revision of his work in 1874, Gould interpreted the solution in the same sense, arguing that: "(…) Thome compared all the work with the sky again, adding many stars, and discovering mistakes in the identification of several others."[29] In the fourth chapter, he explained how in this final part of the work several errors he deemed major were found and he indicated their origin and solutions, always in relation to new observations regarding stars on which assistants differed. The solution to overcome the errors was to leave this part of the work in the hands of the best-trained observer. This was done for all the errors Gould considered major, whether they stemmed from mistakes in readings of magnitudes in previous catalogues or mistakes in the reference catalogues themselves.

Nonetheless, and secondly, this was not considered a solution for red or pronounced orange stars: "The discrepancies of this last kind"–referring to errors due, according to him, to the star being near a brighter one–"could, in general, be solved with the use of the telescope; but those that were due to the colour of the star could not be solved so easily".[30] Gould attributed the error to a problem with the observers´ senses. When dealing with these stars he came close to the way other astronomers were discussing 'personal equations' as problems of observers´ senses. According to Gould the problem could be caused by "some tendency to what is called colour blindness, a phenomenon which I think is caused by (…) the lack of a complete sensitivity of the retina to certain luminous vibrations".[31] Thus, he made the distinction that only in these cases was no clear answer found to solve the question of discrepancies "they were often, even mostly, irreconcilable. Not just the red ones, but also those with a pronounced orange colour gave a lot more work for this reason."[32] And he added:

> Happily there were two observers, Mr Thome and Mr Hathaway, who almost constantly estimated the magnitudes of coloured stars much more highly than the another two men, Mr Rock and Mr Davis; while the judgement of one pair and the other were usually in agreement. (…) It so happens that we have relatively few coloured stars in our list of magnitude types. For the main *Uranometría* catalogue we have adopted, in such cases, the average between the highest and lowest estimates (…)[33]

It was decided that the problem of personal differences in the estimation of the magnitude of a red coloured star was to be solved not through the use of some instrumental or photometric auxiliary, nor by work discipline, but by calculating an average. That is to say, it was established that the magnitude of the star was such that, paradoxically, no one had ever seen it with precisely that intensity. Contemporary astronometrical conventions were followed regarding the resolution of magnitude equations for catalogues.

Thirdly, Gould would insist on the hypotheses that the most notable errors came neither from mistakes in the training of observers, nor from differences in perception, but from the very nature of the celestial bodies observed:

> Also, the frequent indications that the light from a very considerable portion of fixed stars is in no way constant, evidently showed the suitability of increasing where possible the number of those types of comparison.[34] (…) There have been cases in which, through common agreement, an adopted magnitude has varied by two and even three tenths; but I am convinced that, in most such cases, the change has been in the star itself, rather than in the estimation of its degree of light." [35]

Gould continued by saying that in the cases of a pronounced discrepancy between the estimations done by him and his helpers at Albany in 1858, and those done at Córdoba, it seemed highly likely that they were due to a real change in the brightness of the stars themselves, rather than any error in estimates. He considered such discrepancies "strong indications of variability."[36] His own experience told him that sometimes stars with indefinite or variable brightness would then be discovered as double stars by more powerful telescopes. Astronomy could not yet explain those cases in which the variation came from the fluctuation of brightness of just one star.

Fourthly, the error was tackled differently in the case of stars of very weak magnitude. Gould proposed to continue the work done by Argelander, which meant accepting the sort of convention regarding the scale adopted for different magnitudes, but he also wanted to make the grading of the scale even more accurate, down to the decimal fractions of the unit. Furthermore, Gould had compared the stars that had to be observed further north with the stars registered in Bessel's areas –after reducing their magnitudes to Argelander´s scales through previously published tables– not only with the *Durchmusterung*, but also with Lalande's and Taylor's catalogues, and had reached the following conclusion:

> (…) comparing all our estimates of the stars that are to the north of the equator, they indicated that what we had called $6^{M}5$ was really $6^{M}9$ or $7^{M}0$; and that many of the stars that had been seen here in Córdoba by the naked eye and put on our maps, were not really higher than 7¼. It seems beyond all doubt that, on the most favourable nights, stars of magnitude 7.0 can be seen easily in Córdoba by people with normal eyesight; while in Albany I determined $6^{M}2$ as the corresponding limit.[37]

In short, establishing the magnitude types demanded a re-adaptation from Gould; in the previous catalogues no one had gone to the depths he required, nor established degrees down to decimal fractions, nor broadened the scope of the scale to cover stars weaker than 6.5 magnitude. Gould explained that:

> (…) serious difficulties appeared in forming the scale for stars with a magnitude lower than the sixth degree. The work of extending a series accurately and precisely, beyond its extreme limit, is a problem that usually has serious obstacles; and for some time I was completely incapable of determining a type that represented the zero of the seventh degree of magnitude. (…) Faced with this emergency, I turned to Mr Argelander himself, asking him to designate several stars visible in Córdoba that he adjudged would serve as types to represent magnitude 7.0; but I was sorry to learn that this distinguished friend did not consider himself suitable for determining such types.[38]

As mentioned before, Zöllner had introduced a photometer that he claimed would solve this problem, and the device had been acquired in Córdoba.[39] However, Gould preferred to take for reference certain conventions regarding the denomination of the brightness of stars that were previous to the introduction of the photometer. However, in order to set a standard with which to compare the weak magnitudes that were observed and to determine the degree of brightness that should represent the zero of the seventh degree, Gould advanced in the same direction as Zöllner when it came to constructing types to broaden the scale. That is, he began to devise ways to crystallize in an instrument the knowledge regarding the definition of the magnitude of brightness of stars. Let us remember that the German had placed a cover with different diameters of apertures in front of a flame in such a way that the different apertures let through more or less light from the flame; he thus managed to project it onto a transparent screen that would serve as an artificial star to compare to the star observed in the telescope. Gould's method was similar:

> (...) a considerable number of photometric experiments were done through the method of minimum apertures (…) I performed a long series of tests through apertures of several diameters in the cover of a small telescope." (…) "Sliding plates were built, with a series of round holes whose successive diameters differed by a hundredth of an English inch, which is the same as a quarter of a millimetre. These were fitted on the cover of a small portable telescope of equatorial movement. Having chosen stars that represent the diverse variations

> of magnitude from 5.5 to 6.1, we looked for the smallest aperture with which each star was still visible.[40]

Gould continued to measure stars with holes pierced in a cover that covered the eyepiece of a telescope, but this time comparing them with stars that were called 6.0 in Albany, discovering that they needed a diameter of 0.18 of an inch, but judging that the same would apply to those weaker than that, he did not continue. Thus far he seemed to be on his way to reinventing Zöllner's photometer; but instead of passing artificial light from a flame through the hole, to project it onto a translucent screen and comparing the projection the light from the real star, Gould made the holes directly in the cover of the eyepiece so the light of the real star would go through it. However, he stopped early, considering it ineffective and came to the conclusion that the method of minimum apertures was "extremely illusory and uncertain."[41] Either way, Gould used the tests to give grounds to conventional agreements:

> The zero for the seventh magnitude, reached empirically through the common agreement of the four observers, was neatly compared to the average that resulted from several catalogues, especially with the Bonn Durchmusterung, and it was found to match these sufficiently to authorize its adoption without any change, all the more so as it also matched the results obtained through the minimum aperture method.[42]

The effectiveness of Gould's attempts or of Zöllner's invention does not concern us here; what matters is that all those hours of work spent on experiments and building instruments, were caused by practical problems that astronomers encountered in their everyday work compiling catalogues. Thus new areas of research opened up, though that did not ensure that their objectives met with success. Furthermore, Gould himself was conscious of the parallels between his attempts and the already invented photometer, which he had in his possession at the Córdoba observatory, which led him to clarify:

> It is true that I had a Zöllner-built photometer, which this eminent physicist, with great kindness, had had built and sent to Córdoba for me; but (…) I would have needed a number of preliminary observations and investigations for which no time was available, even if circumstances had been otherwise favourable. I thus concentrated my efforts on continuing and extending the best existing magnitude system, hoping at the same time to be able to later determine, through independent research, the reason for the brightness corresponding to this system.[43]

This case confirms something that was suggested when discussing Lankford´s interpretation of unqualified labour into astronomical work. It was not only a question of instruments crystallizing human knowledge and replacing qualified labour. The incorporation of the new instrument implied a restructuring of the work processes that would not be carried out immediately. This happened, among other reasons, because there was a need not only for new knowledge to build these devices, but for new abilities to manage them adequately and to be able to adapt the type of data produced to the type of data acquired previously. According to Gould, Zöllner's invention had to be postponed, because the convention of the scale of magnitudes incorporated into the instrument was not the same as that of the catalogues he used as guides.

In chapter six of *Uranometría* he came back to this issue and formulated a proposal regarding comparisons to relate the work he was carrying out with recent and older photometric readings:

> It has already been said that, according to my original plan, the relative brightness of a considerable number of stars had to be measured through Zöllner's astrophotometer, and the findings compared with our determinations of magnitudes, to establish a definite formula that expresses the relationship between our scale and the numerical value of brightness deduced from photometric observations. This has not been yet possible for me, due to the amount of work other studies have required of me; however, I do not abandon the hope of carrying out this idea.[44]

It must be taken into account that Zöllner's photometer was too recent to have been used in major catalogues as a reference. The fact is that Gould was not the first astronomer to attempt to use photometers in large scale surveys of star magnitudes. He insisted it would require great care to select a group of primary stars and establish their absolute magnitudes, so they could be used as reference stars for other magnitude measurements and for future catalogues. He indicated that he had reached the conclusion that the determination of magnitudes had to be done through comparative estimates of brightness, "without recurring to instrumental readings, nor employing any empirical reason for the relative amount of light corresponding to the successive degrees" and that this had made "much more notable the importance of adopting trustworthy types".[45]

Fifthly, the problem was also detected and used to organize ways of presenting observation work, although no univocal solution was found for this. Gould focused the problem on the different estimates of the brightness of stars according to the place in the sky where these were found. He pointed out that in such cases the same observer could assign different magnitudes to the same stars.[46] This fact, added to the importance given by Gould to determining the magnitude of the brightness of stars, established organizational criteria for work processes. Thus, the constellations were separated into each of the three books in which observations were registered, keeping each of them in the order of declination. Of these books, the first took note of the 27 southernmost constellations, the third the 22 most to the north, and the second the 17 that were between the first and third. It was noted there that not only were there 'personal equations', but that these varied according to the observations of the same astronomer in different parts of the sky.

In these five ways of tackling the problem, Gould knew he was following alternatives that international astrometry had already established. He would, however, note in regards to this inconvenience:

> But, notwithstanding all the influences that may have affected the consistency of the various discrepancies, I think the total sum of them is still extremely small, whether you consider the differences between the respective observers and the average of all of them, or the variation of each observer from his own mean.[47]

The problem was surely being diminished by the director. However, the issue was important enough in the practices of the Germans, to which Gould constantly referred, that many pages of *Uranometría*, finally published in 1879, would be dedicated to the 'personal equation'.[48]

Although it was sometimes supposed that errors were tied to lack of discipline, training or suitable instruments and, at other times, the problem of the ‘personal equation’ was diminished and did not seem to affect the readings of the observation except when an average was calculated to save the errors, the topic merited enough attention that it came to order the way observations were registered and filed. What Hoffman indicated for Bessel in the first half of the nineteenth century continued to occur for several decades, namely that the ‘personal equation’, though not solved and sometimes even ignored as a problem, started to become an object of research in itself at the Córdoba Observatory.

5. ‘Personal equation’, the chronograph and instrumental practices in the Córdoba Observatory.

For the next work, beginning in 1872, when the building was finished and the instruments installed, Gould´s team would incorporate several devices to register and observe the transit of stars. The catalogue observed with a meridian circle was published as *Zonas de Observación*. This was one of the main objectives of Gould´s initial plan.[49] This task was noted in the writings of the time as a continuation of the work began by Lacaille with his small telescope on the Cape between -23º and -80º declination and, in Gould´s words, its writing followed “the dearly expressed desire of the late Argelander”.[50] Regarding the instrumental practices involved in this work, the director pointed out that:

> (…) The instrument has seven groups of parallel spider’s threads on the eyepiece whose respective distances regarding the centre of the field of vision have been tidily determined through many observations (…) this way, whichever group of threads is observed to pass through the path of a star, it will be easy to take note of the time corresponding to the movement and of the centre of the instrument. [51]

The description of the threads on the instrument´s eyepieces showed how this obeyed the standards demanded in astronomical practices in that period: the stars were observed passing through several threads and the reduction was calculated to the central thread, which marked the transit of the star through the local meridian. Let us remember that ‘personal equations’ in the reading of star coordinates were produced when different times were perceived for the transit of the star through these threads. That is why precision in the distances between these allowed averaging successive ‘personal equations’ for the same observer on just one star. It must be taken into account that a mistake of one second in time of transit was equal to approximately half a kilometre in calculations of terrestrial longitude. Gould not only had a suitable instrument, he was also trained in body positions and work rates considered necessary for the correct usage of the instrument, as well as the conventions regarding how many measurements had to be determined for each star on each occasion.

> (…) The circle with the degrees marked for measuring declinations is read through microscopes that are on a frame which rests on one of the stone pillars that hold the instrument. There are four microscopes that are ninety degrees from each other and all of them are read and their mean is recorded for regular observations (…) but when in an area under observation stars go by too quickly to read all microscopes, only one must be used(…) In this way the observer at

> the microscope retains the same position in all observations of a form, and to prevent the heat of his body from eventually producing expansive action on the material (…) he sits at a certain distance (…)[52]

When speaking of the first observations made with the meridian circle for the areas, the director explained he had supposed that by following this dynamic and suitably alternating observers, it would not be hard to deduce their respective 'personal equations', so different results could be combined, without errors. "But a very short experience was enough to prove that such hopes were vain (…)" [53]

Furthermore, the electric chronograph had been introduced into international practices and a debate had begun on whether the device had the capacity to eliminate the problems arising from personal differences in the recording of the transit of the stars. As mentioned previously, the chronograph was noted by some scholars as one of the instruments in which astronomers placed their confidence as part of an attempt to eliminate the 'personal equation'. Gould described the chronograph with precision, making reference to the homogenous rotation speed of the device´s cylinder on which a pen marked the passing of time on paper and to the way astronomers registered right ascensions on said graph. Gould again showed his awareness, not only with the use of cutting-edge astrometry instruments, but also with the appropriate bodily positions for using the new apparatus. The work had to be coordinated by two astronomers in such a way as to achieve observational patterns suitable for contemporary practices:

> (…)a switch which can cut the power allows the observer to record time on the chronograph (…)"[54] "(…) The observer is reclined on a mechanical chair that can be adjusted (...) until he can support his head at a given height (…) in his right hand he has the switch for the telegraphic signals and in his left an iron lever for raising and lowering the telescope (…) he then tells the assistant at the microscope the magnitude of the star observed and the group of threads on which its passing is recorded.[55]

When operating the signalling switch, the observer would take the pen and record the time it took the star in question to complete its transit. This operation was supposed to be able to eliminate the error of the previous method in which the clock pendulum had to be heard to record the passing of time. However, let us remember that Safford would also later point out that hearing errors could be carried over to chronograph errors. These occurred because the button which was set to mark the exact moment on the cylinder´s paper strip was not always pushed at the exact same time by all observers. In fact, differences were even recorded by one same astronomer in recordings at different times.

The group at the Córdoba Observatory explicitly contributed data to international research into 'personal equations' in determining star coordinates as well as to research into the material conditions of observation.[56] This occurred at the same time that these catalogues were written, catalogues which were intended to be more accurate than the *Uranometría,* since they were compiled using telescopes and chronographs.

> Observations have been made (…) which are very thorough, with the objective of determining the differences between right ascensions that are measured with a chronograph and those that measured using the old 'eye-and-ear' observation method. Many observations have also been done to cover the effect that a star's

> brightness can have on the observation of its position. At the same time re-observation has been followed systematically for all cases of discrepancies detected in the areas, in order to observe and correct errors. (…) Several secondary investigations are also being done in regards to the instrument, on the fundamental stars, on the refraction constants employed, on the scales employed by the different observers in their estimates of the magnitude of telescopic stars, on the personal differences with the same observations done by eye-and-ear, etc. [57]

The work here at Córdoba was advancing in the direction of the attempts carried out by international astrometry, experimenting with both methods –'eye-and-ear' and chronograph– that still overlapped in practice. These notes tried to determine new forms of organising work that could, if not solve the problem, at least give a detailed account of its existence. Furthermore, an important part of the volumes of the final publication would be dedicated to detailing the evolution of this new field of research. As in other observatories of the world, the difference between methods had become an object of research.[58] Gould shows how errors became the object of these new research areas:

> it is necessary to commence three different investigations: 1. The 'personal equations' in chronographic observations; 2. The 'personal equations' in the passages observed by eye-and-ear, which in the current case have only been for stars of a relatively high declination; 3. The reductions that must be applied to the right ascensions determined by eye-and-ear, to make these homogeneous with the whole sum of chronographic determinations. [59]

The new research had its method, and it was claimed that if this was not followed, the subjectivities would reappear, bringing in through the window what had gone out through the door. Gould explained that most of the comparisons done to reduce 'personal equations' had been done with readings that were taken from astronomers without awareness of the observations they would have to be submitted to, the confrontations and special tests, and thus would seem to not have inadvertently influenced the quality of the observations. However since in some cases this condition was not met, the results seemed to indicate "the existence of such an influence".[60] Here, for the first time something of a 'psychological' type is noted that would alter the record of the observations; the observer who knew he was being observed would produce different results, which was not the focal point of the problem. The truth is that Gould had the skies observed regularly by both methods and prepared tables for registering stars in columns where the position in which they were found would be noted, and in files, where the observers would be noted by their initials. When the data was cross-referenced he obtained maximum and minimum differences between the positions found by the different observers. He also evaluated if these differences increased or diminished according to the declination and if the magnitude of the brightness of stars influenced them.

In the case of 'personal equations' of eye-and-ear observations, Gould stated that personal differences were so great that they necessarily had to be taken into account, sometimes being of more than half a second. On the other hand, as Safford would later point out, Gould maintained that chronographic observations were particularly susceptible to certain sources of errors. He indicated particularly that the determination of right ascension was influenced by the magnitude of the star. He referred to the verification in telegraphic determinations of longitude done in the United States in the years 1852-1865 and the determination of longitude between Europe and America in 1866. He cited the words of the astronomer Gill "it seems there is an influence involved here that does not exist in the eye-and-ear observation method; that is to say, an effort (generally unnoticed) of judgement, according to which many, and possibly most, observers signal with the switch not in the instant they see the star on the thread, but at a prior moment, so that according to their appreciation, the effect happens in the precise instant they want to register, and after an interval of decision and another interval of muscular contraction …"[61] Gould tried to get out of the bind swiftly, saying that it was clear that the true method that had to be followed was to give the signal in that "instant in which the star is actually divided by the thread". [62] He ventured that then, however great the difference with other observers, the 'personal equation' would be constant and unaffected by many other strange influences that otherwise would not stop having disruptive effects.

However, the solution was not as simple as Gould presented it; he himself acknowledged that the erroneous observation system, which manifested itself in the differences in right ascension, became more complex among stars of different declinations or dissimilar magnitudes. On the one hand, much greater 'personal equations' were verified for stars that moved slowly (below 60º) and thus made it necessary to apply the 'personal equations' that were already calculated only to those cases. On the other hand, a tendency to observe the brightest star early was verified. This means that all right ascensions were augmented, since the fundamental stars were generally greater than those whose positions had to be determined. He even accepted that there were indications that some of his own observations had been particularly affected, since, when confronted with the general catalogue, they ended up needing negative corrections.[63] This problem meant that in the work programs the readings were examined separately according to the difference in brightness of stars; these were divided into three categories to allow independent research of each group.[64] To decipher the existing relationship between brightness and the 'personal equation', Gould proposed a program that he would not ultimately carry out: either compare the 'personal equations' of several observers for stars of different brightness, or have observers diminish the apparent magnitude of the star "artificially, closing down the aperture of the telescope", thus comparing the ascension obtained with the whole eyepiece and with the diminished one. [65]

In any case, due to all the uncertainties that the use of the chronograph entailed, Gould took care to record which stars were observed with which methods. Depending on the declination of the star, it was registered chronographically or with the 'eye-and-ear' method. Since 'personal equations' still differed for the same observer in either method, the necessary corrections had to be determined so the necessary passages matched. Gould stated that this was not a simple equation and that it changed for different observers. Since the same star was observed over three years by both methods, Gould´s team proceeded to compare the readings and established the differences that corresponded to each of the observers. The average difference was 0.114 seconds. Gould presented the smallest difference at 0.077 seconds followed by Thome at 0.085.[66] This average was used to correct all the passages observed by 'eye-and-ear', independently of who registered them. As Gould put it:"It has seemed best to not apply any personal correction, but a general one to convert to the chronographic scale all passages that were observed by 'eye-and-ear'[67]. Regarding the coherence of the values obtained thus by the two methods, Gould warned: "generally modifications that are almost arbitrary have been necessary with this object. Notwithstanding, I am fairly confident that the corrections adopted are very close to the truth; and in those cases in which modifications of some importance have been made, there is almost always an independent testimony in its favour".[68] Despite all the objections, in the following volume, he committed to the implementation of the new method: "A careful study of all the data provided by the observations of the year makes it evident that the 'personal equations', in those done by chronograph, were in all cases small and disposable".[69] As Hoffman points out, sometimes the problem was detected, only to be ignored shortly after.

If the determination of right ascensions and declinations entailed all this work, less effort was expended on the accuracy of the determination of the magnitude of stars. In this respect the opposite was done as in the *Uranometría*, where precision was sought in the measurement of brightness down to one decimal. Gould admitted that the magnitudes of the Catalogue had in no way been deduced from the estimates made in the moment of observation, that a great number of them had been defined for the *Uranometría*, that others were estimated with less dedication and not always during its passage over the meridian, and that when done later through the telescope, they had been determined only down to the quarter of the unit.[70] He concluded that:

> The part of the determinations, in the areas, that I have less confidence in is in the estimates of the magnitudes. No one who has not learned from experience will be able to fully understand how difficult it is for the observer to assign values that are satisfactory even to himself. For all his attention is necessary to register the passing through the various threads, to verify the direction of the telescope in declination, and to choose the group in which the star being followed will be observed, or to prepare to move the telescope in search of another, after having noted the declination of the latter. In such circumstances, I have rarely attempted to make magnitude estimates of an approximation greater than the half unit; nor have I been able to have any confidence whatsoever even in that limit (…) The excessive influence of subjective conditions has caught my attention many times. In several occasions when observing areas while I suffered from a headache, I overestimated the brightness of all stars systematically, almost totally or totally by one unit of magnitude.[71]

Either way, Gould knew he was moving within the limits of the astronomical practice of his time and justified himself by saying that Argelander had already said that appreciating the magnitudes of stars seen through a telescope was one of the most difficult problems for astronomy:

> The impression made on the eye by their light is in such a way influenced by the state of the atmosphere, by the greater or lesser illumination of the field of vision, by the degree of tiredness of the eye, and by other incidental circumstances, that all determination becomes very uncertain because of it. To this must be added the fact that the image of the previous star still stays in sight exercising an involuntary influence on the appreciation of the following one, so when a brighter one follows a weak one we consider it too bright, and vice-versa.[72]

While these dissimilarities continued to exist, Gould proposed an average for coordinates and an approximation for the orders of magnitude that would finally be recorded in catalogues. The productivity index of this work would be given by the amount of stars observed: according to Gould "(…) more or less 180 are observed per hour, three per minute, determining time of transit, magnitude and declination". [73] They thus achieved international standards for scientific work in this area. Finally, the work was published as *Catálogos de las Zonas Estelares* and contained the determination of the position of 73,160 stars. At the same time, a series of independent observations was carried out: the total was 155,000. The series was completed in 1885 and published in 1886, for the first *Catálogo General Argentino,* containing 32,448 stars.[74]

6. Perspectives for debate

Hoffman claims that Sanford was right in 1888 when he warned that astronomers had paid scant attention to this aspect of the problem despite Bessel's notes, but that he was wrong to say that this had no consequences. In fact, says Hoffman, the frequent clarification in the footnotes that the observations were made by the same astronomer was a way of indicating the personal differences between different observers, and then, in Paris or Greenwich, when there were four assistants, a column was added to the coordinates to indicate which observer had noted which data. These were consequences of Bessel's arguments, although the problem would only reappear decades later under the title ʿpersonal equation', Hoffman writes, when rather than a response to an urgent problem, it was a reaction to the mess generated by this ʿwork style' that alternated different observers for the same series of observations, although Bessel had always advised against doing this.

In the Córdoba Observatory the topic existed as ʿpersonal equation' was effectively a response to a mess of its own making, due to the scale of the work, rather than a ʿwork style'. Notwithstanding, Bessel's model was seen here too: sometimes understanding of the phenomenon was superficial, but the attempt to understand it established observation and recording guidelines that affected the way the rest of the work was organised and guided the ways in which obtained results were conveyed. It is true that Gould barely advanced in the understanding of the ʿpersonal equation' problem, that his answers were erratic and ambiguous –if not contradictory–, that he constantly struggled against procedures which he appeared to follow, and that in many passages he suggested that he did not agree with the remedies he himself proposed. This course of action, where the obstacle is tackled from different perspectives, including attempts to adapt it to work regulations, replace it with instruments, write it off because of agreements on averages, or directly ignore it despite having been alerted to its presence, shows the discomfort of observatory directors in the late nineteenth century in the search for solutions to these inconveniences that they have agreed to call ʿpersonal equations'. However, it does not appear as if the director was losing control of this phenomenon, but rather that he gradually found varied ways of controlling the problem. In his attempts to eliminate errors, attitudes towards observers alternated between attempts to calibrate them and describe them as just another instrument –or as parts of one–, replace them through the incorporation of new instruments, or place confidence in the qualifications of one of them, who then oversaw the others. What Hoffman and others point out as distinct from the act of disciplining, i.e., classifying the observers´ errors as instrument errors, also allows for discipline, in the sense that it enables working times and rates to be controlled outside of the awareness of whoever was exerting that control directly. With this clarification, Schaffer and others make a valid point on the relationship between strict discipline, instrument replacements and ʿpersonal equation' in the organization of the work of the major observatories. Following these lines, the team of the Cordoba Observatory, distant from Europe and the United Estates but in very close contact with its most advanced astronomical techniques, were able to generate highly accurate data without possessing complete understanding of the phenomena of error and variation in observation.

REFERENCES

1. For historical analyses of the observatory since its foundation see: Omar Bernaola, *Enrique Gaviola y el Observatorio Astronómico de Córdoba. Su impacto en el desarrollo de la ciencia argentina* (Buenos Aires, 2001); Enrique Chaudet, "Benjamin Apthorp Gould", *Revista de la Universidad de Córdoba*, (1924); *La evolución de la Astronomía durante los últimos cincuenta años (1872-1922). Evolución de las ciencias en la República Argentina*, (Buenos Aires, 1926); George Comstock, *Biographical memoir Benjamin Apthorp Gould 1824-1896* (Washington, 1922); Robeto Ferrari, "Carl Schultz-Sellack (1844-1879) y los orígenes de la fotografía astronómica en la Argentina", *Saber y Tiempo*, Nº 11 (2001), 71–101;John Hodge, "Benjamin Apthorp Gould and the Founding of the Argentine National Observatory", *The Americas*, Vol. 28, Nº 2 (1971), 152–75; Philip Keenan, "The Earliest National Observatories in Latin America", *Journal for the History of Astronomy*, Vol. 22, Part 1, Nº 67 (1991), 21–31; Wayne Orchiston, "Southern hemisphere astronomy", *Journal for the History of Astronomy*, Vol. 21, Part 3, Nº 65, (Cambridge, 1990), 305–7; Santiago Paolantonio, Santiago and Edgardo Minniti, *Córdoba Estelar. De los sueños a la Astrofísica. Historia del Observatorio Nacional Argentino* (Córdoba, 2009); Santiago Paolantonio and Edgardo Minniti, *Uranometría Argentina. 2000. Historia del Observatorio Nacional Argentino*, (Córdoba, 2000); Lewis Pyenson; *Cultural Imperialism and exact sciences: german expansion overseas 1900-1930* (New York and Berne, 1985); Marina Rieznik, *Los cielos del Sur. Los observatorios astronómicos de Córdoba y de La Plata (1871-1920)* (Rosario, 2011); Sociedad Científica Argentina, *La evolución de las Ciencias en La República Argentina 1923-1972*, Tomo VII, Astronomía (1979).
2. Jimena Canales, "Exit the frog, enter the human: physiology and experimental psychology in nineteenth-century astronomy", *The British Journal for the History of Science*, Vol. 34, Part 2, Nº 121 (2001), 171–97 ; William Rogers, "The german survey of the northern heaven", *Science*, Vol. 2, Nº 29, (1883), 229–37; Edmund Sanford, "Personal Equation", *The American Journal of Psychology*, Vol. 2, No. 1 (Nov., 1888), 3–38; Edmund Sanford, "Personal Equation", *The American Journal of Psychology*, Vol. 2, No. 3 (May, 1889) 403–30 and Edmund Sanford, "Personal Equation", *The American Journal of Psychology*, Vol. 2, No. 2 (Feb., 1889), 271–98; Klaus Staubermann, "Making stars: projection culture in nineteenth-century German astronomy", *The British Journal for the History of Science*, Vol. 34, Part 4, Nº 123 (2001), 439–51.
3. Marina Rieznik, *op.cit* (ref.1), in regards to the notion of the construction of scientific objects see Hans-Jörg Rheinberger, "A reply to David Bloor: 'Toward a Sociology of Epistemic Things'", *Perspectias on Science*, Vol. 13, Nº 2, (2005), 406–10.
4. John Lankford, *American Astronomy: Community, Careers, and Power, 1859-1940* (Chicago, 1997); Simon Schaffer, "Astronomers mark time: discipline and personal equation", *Science in Context* 2, (1988), 115–45.
5. Simon Schaffer, *op. cit*. (ref. 4).
6. John Lankford, *op. cit*. (ref. 4).
7. Simon Schaffer, *op. cit*. (ref. 4); In contrast to Schaffer, Canales pointed out that for the astronomers on which she focused, education and discipline were not a solution since the personal equation varied according to the observer's physical fatigue. Cf. Jimena Canales, *op.cit* (ref. 2) Christoph Hoffman, "Constant differences: Friedrich Wilhelm Bessel, the concept of the observer in early nineteenth-century practical astronomy and the history of personal equation", *The British Journal for the History of Science*, Vol. 40 (2007), 333–66.
8. Christoph Hoffman, *op.cit.* (ref. 7).
9. Lorraine Daston and Peter Galison, *Objetivity* (Nueva York, 2007) Cfr. Jimena Canales, *op.cit.* (ref. 2). The nineteenth century, as a stage in the history of modern ʽobjectivity', was, according to Daston and Galison, marked by the attempt to leave to one side the subjective, emotional intervention, that stems from individual characters, in scientific records. To this end an attempt was made to control the observer to avoid his judgement from intervening in the record. The acceptance of the enormous importance of a trained judgment in the constitution of current objectivity would belong to a later stage.
10. Cfr. Christoph Hoffman, *op.cit.* (ref. 7); Simon Schaffer, *op. cit*. (ref. 4); Jimena Canales, *op. cit*. (ref. 2).
11. Gaston Bachelard,, *The New Scientific Spirit* (Boston, 1984).
12. William Rogers, *op. cit*. (ref. 2).
13. Christoph Hoffman, *op.cit.* (ref. 7).

14. The attempt was not exclusive to astronomy, already in the eighteenth century diverse European societies and academies, as recounted by Podgorny and Schäffner, produced innumerable guides to educate the sight and gestures of the traveller-gatherer of a distant wilderness, which consisted in the procedures which had to be followed in the recollection of plants, animals and minerals or in the use of instruments. Like the travelling naturalists in unexplored territories, the Germans in their instructions hoped to ensure the flow of a huge mass of data from new stellar regions and, in the same process, advance in the forms of recording, processing and storing the same. Podgorny and Schäffner point out that the tendency to control the sources of observational errors remit to the problem of finding the proper language that is inseparable from the need for statistical precision, which surfaced in the eighteenth century and was expressed through the measurements and quantification of nature and society. At first these ʽinstructions' were set to guarantee the uniformity of the data gathered to the effects that they were not lost in a collection of heterogeneities. The German astronomers also considered that many hours of work already carried out at that point were becoming a waste because that unity was not assured. Cfr. Irina Podgorny, and Wolfgang Schäffner, "'La intención de observar abre los ojos'. Narraciones, datos y medios técnicos en las empresas humboldtianas del siglo XIX", *Prismas*, Nº 4, (2000), 217–27.
15. *"no pudiendo soportarlo bien por más tiempo ni los ojos ni los nervios de los observadores sea al telescopio ó al microscopio.(…) Todas las pocas zonas, sin escepcion, que se cointinuaron por dos horas, resultaron perjudiciales al observador."* Gould, Benjamin Apthorp, "Observaciones del año 1872", in *Resultados del Observatorio Nacional Argentino*, Vol. II, (Buenos Aires, 1884), 47
16. Christoph Hoffman, *op.cit.*, (ref. 7).
17. Allan Chapman, "The accuracy of angular measuring instruments used in astronomy between 1500 and 1850", *Journal for the History of Astronomy*, Vol. 14, (1983), 133–7.
18. Jimena Canales, *op.cit.*(ref.2).
19. Klaus Staubermann, *op.cit.* (ref. 2).
20. Jimena Canales, *op. cit*. (ref. 2), Truman Safford, "The psychology of the personal equation", *Science*, New Series, Vo.6, Nº 152, (1897), 784–788; Truman Sanfford, "The psychology of the personal equation", *Science*, New Series, Vo.4, Nº 84, (1896), 170–1.
21. Lorraine Daston and Peter Galison, *op cit* (ref.9); Cfr. Jimena Canales, *op.cit.* (ref. 2).
22. Hugh Slotten, " The dilemas of science in the United Status: Alexander Dallas Bache and the U.S. coast survey" *Isis*, 84 (1993), 26–49, 41–2.
23. Jimena Canales, *op. cit.* (ref. 2).
24. Simon Schaffer, *op. cit* (ref. 4).
25. Truman Safford, *op. cit.*(ref. 20) (1896, 1897).
26. John Lankford, *op. cit*.(ref. 4).
27. John Hodge, *op.cit*. (ref. 1). However, let us point out that, even before that, Gould had to make sure that his assistants were transformed into observers with the capacity to produce errors based on their ʽpersonal equations', and not because of a lack of assimilation of the basic rules of work.(Cfr, Marina Rieznik 2011)
28. "Este procedimiento, se hizo independiente y separadamente por cada uno de los cuatro observadores; además, después de concluida para cada región sucesiva de la faja mencionada, comparaban juntamente los resultados, no solo uno con otro, sino también con el cielo. Las discordancias resultaron ser notablemente pocas y ligeras, desapareciendo en algunos casos con la revisión común. Raras veces ascendieron a dos décimas partes de una unidad, ni había por lo general variación sistemática. Efectivamente los casos de un exceso o falta regular en la apreciación se limitaron en su mayor parte a estrellas de un color pronunciado o que se hallan próximas a otra más brillante. (…) No se adoptó como tipos de magnitud sino aquellas estrellas sobre las que los cuatro observadores estuvieron de acuerdo, desechando las demás de la lista.", Benjamin Apthorp Gould, "Uranometría Argentina. Brillantez y posición de las estrellas fijas, hasta la séptima magnitud", *Resultados del Observatorio Nacional Argentino en Córdoba* (1879), chapter one, no page number.
29. "Thome comparó nuevamente toda la obra con el cielo, agregando muchas estrellas, y descubriendo equivocaciones en la identificación de varias otras.", Benjamin Apthorp Gould, *op.cit* (ref.28). chapter one, no page number.
30. "Las discordancias de esta última clase pudieron, por regla general, subsanarse con el uso del telescopio; pero las debidas al color de la estrella no pudieron resolverse tan fácilmente", Benjamin Apthorp Gould, *op.cit* (ref.28) chapter one, no page number.

31. "alguna tendencia a lo que se llama ceguedad de color, fenómeno que me parece debido (…) a la carencia de una completa susceptibilidad de la retina para ciertas vibraciones luminosas", Benjamin Apthorp Gould, *op.cit* (ref.28) chapter three, no page number.

32 ."ellas fueron a menudo aún en su mayor parte, irreconciliables. No solamente las rojas, sino también las del color naranja pronunciado, dieron mucho trabajo por esta razón." Benjamin Apthorp Gould, *op.cit* (ref.28) chapter one, no page number.

33. "Felizmente hubo dos de los observadores, los señores Thome y Hathaway, quienes casi constantemente apreciaron las magnitudes de estrellas coloradas muy arriba de lo que las juzgaban los otros dos señores Rock y Davis; mientras que los juicios de unos y otros se hallaban generalmente acordes. (…) Así sucede que hay relativamente pocas estrellas de color en nuestra lista de tipos de magnitud. Para el catálogo principal de la Uranometría ha sido adoptado, en tales casos, el promedio entre las mayores y menores apreciaciones", Benjamin Apthorp Gould, *op.cit* (ref.28) chapter three, no page number.

34. "Además los frecuentes indicios de que la luz de una porción muy considerable de las estrellas fijas no es de ninguna manera constante, manifestó evidentemente la conveniencia de que se multiplicasen en lo posible el número de estos tipos de comparación.", Benjamin Apthorp Gould, *op.cit* (ref.28) chapter two, no page number.

35. "Ha habido casos en que, por acuerdo común, una magnitud ya adoptada ha sido variada de dos y aún de tres décimos; pero estoy convencido de que, en la gran mayoría de tales casos, el cambio ha sido en la estrella misma, más bien que en la apreciación del grado de su luz.", Benjamin Apthorp Gould, *op.cit* (ref.28) chapter three, no page number.

36. "indicaciones fuertes de variabilidad", Benjamin Apthorp Gould, *op.cit* (ref.28) chapter three, no page number

37. "comparados todos con nuestras apreciaciones de las estrellas que están al norte del ecuador, indicaban que lo que habíamos llamado 6M5 era realmente 6M9 o 7M0; y que muchas de las estrellas que se habían apreciado aquí en Córdoba a la simple vista y puesto en nuestros mapas, no eran en realidad superiores a 7¼. Parece fuera de toda duda que, en las noches más favorables, las estrellas de la magnitud 7.0 pueden verse fácilmente en Córdoba por personas de una vista regular; mientras que en Albany determiné 6M2 para el límite correspondiente." Benjamin Apthorp Gould, *op.cit* (ref.28) chapter one, no page number.

38. "(…) se presentaron graves dificultades en la formación de la escala para las estrellas inferiores al sexto grado de magnitud. La obra de extender con exactitud y precisión una serie, más allá de su límite extremo, es un problema que suele presentar serios obstáculos; y por algún tiempo me hallé completamente imposibilitado para fijar un tipo que representase el cero del grado séptimo de magnitud. (…) En tal emergencia, acudí al señor Argelander mismo, pidiéndole me designase unas cuantas estrellas visibles en Córdoba, que a su juicio pudiesen servir de tipos para representar la magnitud 7.0; pero tuve el pesar de saber que este distinguido amigo no se consideraba en aptitud de fijar tales tipos.", Benjamin Apthorp Gould, *op.cit* (ref.28) chapter one, no page number.

39. Klaus Staubermann, *op.cit* (ref. 2).

40. "se practicaron un número considerable de experiencias fotométricas por medio del método de aberturas mínimas (…) hice una larga serie de pruebas por medio de aberturas de varios diámetros hechas en la tapa de un pequeño telescopio." (Gould, 1879: chapter one) (…) "Se construyeron planchas corredizas, en las que había una serie de agujeros redondos cuyos diámetros sucesivos diferían de la centésima parte de una pulgada inglesa, o sea la cuarta parte de un milímetro. Estas se ajustaron a la tapa de un pequeño telescopio portátil de movimiento ecuatorial. Habiéndose elegido estrellas que representasen las varias gradaciones de magnitud desde 5.5 hasta 6.1, se buscó para cada estrella la mínima abertura que permitiese verla claramente.", Gould, op.cit, (ref.28) chapter three, no page number

41. "(…)sumamente ilusorio e incierto.", Benjamin Apthorp Gould, *op.cit* (ref.28) chapter one, no page number.

42. "El cero para la séptima magnitud, conseguido así empíricamente por el acuerdo común de los cuatro observadores, fue prolijamente comparado con el término medio que resultaba de varios catálogos, especialmente con el Durchmusterung de Bonn, y se halló suficientemente conforme con estos para autorizar su adopción sin cambio ninguno, y tanto más cuanto se conformaba también con los resultados obtenidos por el método de aberturas mínimas", Benjamin Apthorp Gould, *op.cit* (ref.28) chapter three, no page number.

43. "Es verdad que tenia un fotómetro de la construcción Zoellner, que este físico eminente, con la mayor amabilidad, había mandado hacer para mí, y enviar a Córdoba; pero (…) habría necesitado una

cantidad de observaciones e investigaciones preliminares para la cual no había tiempo disponible, aun cuando las circunstancias hubiesen sido propicias bajo otros aspectos. Por lo tanto, dirigí mis esfuerzos a la continuación y extensión del mejor sistema de magnitudes que existía, esperando a la vez poder determinar más tarde, por medio de indagaciones independientes, la razón de la brillantez correspondiente a este sistema.", Benjamin Apthorp Gould, *op.cit* (ref.28) chapter one, no page number.

44. "Ya se ha dicho que según mi plan originario debían hacerse medidas del brillo relativo de un número considerable de estrellas por medio del astrofotómetro de Zöellner, y una comparación de los resultados con nuestras determinaciones de magnitud, para establecer definitivamente alguna fórmula que exprese la relación entre nuestra escala y el valor numérico del brillo deducido de las observaciones fotométricas. Esto no me ha sido posible todavía, debido al mucho trabajo que han requerido otros estudios; sin embargo, no abandono aún la esperanza de llevar a cabo esta idea.", Benjamin Apthorp Gould, *op.cit* (ref.28) chapter five, no page number.
45. "sin recurrir a medidas instrumentales, ni emplear ninguna razón empírica para la cantidad relativa de luz correspondiente a los grados sucesivos", "mucho más notable la importancia de adoptar tipos fidedignos", Benjamin Apthorp Gould, *op.cit* (ref.28) chapter no page number.
46. Benjamin Apthorp Gould, *op.cit* (ref.28) chapter four, no page number.
47. "Pero, no obstante todas las influencias que pueden haber afectado la constancia de las varias discordancias, me parece sumamente pequeño el importe total de ellas, sea que se consideren las diferencias entre los respectivos observadores y el promedio de todos, o las variaciones de cualquier observador desde su propio término medio", Benjamin Apthorp Gould, *op.cit* (ref.28) chapter four, no page number.
48. Benjamin Apthorp Gould, *op.cit* (ref.28) chapter four, no page number.
49. Seth Chandler, "The Life and Work of Dr. Gould", *Science*, Vol. 4, Nº 103, (1896), 885–890; Benjamin Apthorp Gould, "Observaciones del año 1874", *Resultados del Observatorio Nacional Argentino, Vol. V* (1886).
50. "al deseo encarecidamente espresado del finado Argelander", Gould, Benjamin, "Observaciones del año 1872", in *Resultados del Observatorio Nacional Argentino*, Vol. II (1884), 15.
51. "El instrumento tiene el campo de visión de siete grupos de hilos de araña paralelos cuyas respectivas distancias del centro del campo han sido prolijamente determinadas por medio de numerosas observaciones (…) de esta manera, cualquiera que sea el grupo de los hilos en el que se observa el tránsito de una estrella, podrá ser fácilmente computado el tiempo correspondiente de tránsito y el centro del instrumento.", Benjamin Apthorp Gould, *Copiador 3*, manuscript, (Córdoba, 1878-1888), 458.
52. "El círculo graduado, destinado a la medición de las declinaciones es leído por medio de microscopios colocados en un marco que descansa sobre uno de los pilares de piedra que sostienen el instrumento. Hay cuatro microscopios que distan noventa grados el uno del otro y todos ellos son leídos y tomado su término medio en las observaciones regulares (…) pero cuando en la observación de una zona se suceden las estrellas con demasiada rapidez para que sea posible la lectura de todos lo microscopios, hay que servirse de uno solo (…) De esta manera el observador que se halla delante del microscopio retiene la misma posición en todo el tiempo de las observaciones de una forma y para evitar que el calor del cuerpo produzca a la larga alguna acción expansiva sobre el material (…) se sienta a cierta distancia", *Ibid.*, 51.
53. "Pero muy poca esperiencia bastó para demostrar que semejante esperanza era vana", Benjamin Apthorp Gould , *op.cit* (ref. 50), 43
54. "(…)una llave con la que puede interrumpirse la corriente facilita al observador el registro del tiempo sobre el cronógrafo (…)", Benjamin Apthorp Gould, *op.cit* (ref. 51), 458.
55. "(…) El observador está reclinado sobre una silla mecánica que puede acomodarse (...) hasta que le permita apoyar la cabeza a una altura cualquiera (…) tiene en su mano derecha la llave de las señales telegráficas y en la izquierda el mango de una pieza de hierro destinada a elevar o deprimir el telescopio (…) él dice entonces al asistente que se halla en el microscopio la magnitud de la estrella observada y el grupo de hilos sobre el que se anota el tránsito.", Benjamin Apthorp Gould, *op.cit* (ref. 51), 458.
56. Christoph Hoffman, *op.cit*. (ref. 7).
57. "Se han hecho observaciones (…) prolijas con el objeto de averiguar las diferencias entre las ascensiones rectas que se determinan con empleo del cronógrafo y aquéllas que resultan del antiguo método de observación á ojo-y-oído. Igualmente se han hecho muchas para cubrir el efecto que el brillo de una estrella pueda producir sobre observaciones de su posición. Al mismo tiempo se ha seguido sistemáticamente a reobservación de todos los casos de discordancia

descubiertos en las zonas, para poder resolverlos y corregir errores.(…) También están haciéndose varias investigaciones colaterales respecto al instrumento, á las estrellas fundamentales, á las constantes de la refracción que se han empleado, á las escalas empleadas por los diferentes observadores en sus apreciaciones de la magnitud de las estrellas telescópicas, á las diferencias personales de los mismos en las observaciones hechas á ojo-y-oído, etc", Benjamin Apthorp Gould, *op.cit*. (ref. 51), 86.

58. Christoph Hoffman, op.cit. (ref. 7).
59. "se hace menester entrar en tres investigaciones distintas: 1. Las ecuaciones personales en las observaciones cronográficas; 2. Las mismas en los pasages observados á ojo-y-oido, los que en el caso actual han sido solamente para estrellas de declinacion relativamente alta; 3. Las reducciones que deben aplicarse á las ascenciones rectas determinadas por ojo-y-oido , para que estas se hagan homogéneas con el gran total de las determinaciones cronográficas.", Benjamin Apthorp Gould, *op.cit* (ref. 49), 14.
60. "la existencia de tal influencia", Benjamin Apthorp Gould, *op.cit*. (ref. 49), 15.
61. "parece que tenemos implicada aquí una influencia que no existe en el método de observación á ojo-y-oído; es decir, un esfuerzo (generalmente desapercibido) del juicio, según el cual muchos, y quizás la mayor parte, de los observadores hacen la señal en la llave manipuladora, no en el instante en que ven la estrella sobre el hilo, sino en un momento anterior tal que, según su apreciación, el efecto se reproduzca en el instante mismo que quieren registrar, y después del trascurso de un intervalo de volición y otro intervalo de contracción muscular(…)",Benjamin Apthorp Gould, *op.cit*. (ref. 49), 141.
62. "instante en que la estrella se ve realmente dividida por el hilo", Gould, op.cit, (ref. 49) p.141
63. Benjamin Apthorp Gould, *op.cit.* (ref. 49), 46.
64. Benjamin Apthorp Gould, *op.cit.* (ref. 49), 141.
65. "artificialmente, disminuyendo la abertura del telescopio", Benjamin Apthorp Gould, *op.cit*. (ref. 49) p. 165
66. Benjamin Apthorp Gould, *op.cit* (ref. 50), 44.
67. "Así ha parecido que lo mejor es no aplicar ninguna corrección personal, sinó solamente una general para convertir a la escala cronográfica todos los pasages que se observaron á ojo-y-oido", Benjamin Apthorp Gould, *op.cit*. (ref. 49), 18.
68. "generalmente han sido necesarias modificaciones casi arbitrarias con este objeto. No obstante esto, me hallo bastante confiado en que las correcciones adoptadas se acercan mucho á la verdad; y en aquellos casos en que se han hecho modificaciones de alguna importancia, existe casi siempre testimonio independiente en su favor", Benjamin Apthorp Gould, *op.cit*. (ref. 49), 19.
69. "Un estudio esmerado de todos los datos proporcionados por las observaciones del año pone de manifiesto que las ecuaciones personales, en las hechas cronográficamente, eran en todos casos pequeñas y, desechables", Gould, Benjamin Apthorp, "Observaciones del año 1875", in *Resultados del Observatorio Nacional Argentino*, Vol. IX, (1887), 19.
70. Benjamin Apthorp Gould, *op.cit* (ref. 49), 60.
71. "La parte de las determinaciones, en las zonas, en la cual tengo menos confianza es en las apreciaciones de las magnitudes. Nadie que no haya sido enseñado por la esperiencia, podrá comprender debidamente cuán difícil es para el observador asignar valores que satisfagan aun á él mismo. Pues toda su atención le es necesaria para registrar los paasges por los varios hilos, para verificar la dirección del telescopio en declinación, y para elegir el grupo en que ha de observarse la estrella que sigue, ó prepararse á mover el telescopio en busca de otra, luego que se hay anotado la declinación de la última. En tales circunstancias, raras veces he procurado hacer las apreciaciones de magnitud con mayor aproximación que hasta medias unidades; ni he podido tener confianza ninguna en la seguridad de ellas aun hasta este límite (…) La influencia excesiva de las condiciones subjetivas me ha llamado la atención repetidas veces. En varias ocasiones al observar zonas mientras padecia de dolor de cabeza, sobre-estimé el brillo de todas las estrellas sistemáticamente, casi ó totalmente por una unidad de magnitud", Benjamin Apthorp Gould, *op.cit*. (ref. 49), 60.
72. "La impresión hecha en el ojo por la luz de ellas es de tal manera influenciada por el estado de la atmósfera, por el mayor ó menor alumbramiento del campo de vista, por el grado de cansancio del ojo, y por otras circunstancias accidentales, que toda determinación se hace muy incierta á causa de esto. Además debe agregarse que la imagen de la estrella anterior queda aun delante de la vista ejerciendo influencia involuntaria sobre la apreciación de la que sigue, así cuando una más brillante sigue á una más débil, la apreciamos demasiado brillante, y vice-versa", Benjamin Apthorp Gould, *op.cit*. (ref. 49), 60.

73. “(…) observan más o menos 180 por hora, tres por minuto determinando momentos del tránsito, magnitud y declinación”, Benjamin Apthorp Gould, *op.cit* . (ref. 51), 458.

74. Seth Chandler, *op. cit*. (ref.49).