HUMAN COMPUTERS AS INSTRUMENTS

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ABSTRACT. Human computers and scanners were scientific workers who performed calculations or reduced and analysed data before the advent of electronic computers. They were a staple of big science during the 19th century and early to mid 20th century. Yet, despite their prevalence within big science their *epistemic roles* remain virtually unexamined. This paper investigates the epistemic roles of the Harvard Human Computers at the Harvard College Observatory 1880-1920 and of the Bristol Scanners at the Bristol Nuclear Research Group 1935-1955. We identify, evaluate, and compare three frameworks which help us understand the *instrumentalisation* of human computers and scanners and the downstream negative consequences of their instrumentalisation for the methodology of science.
Keywords: *human computers, scanners, big science, methodology of science, epistemic injustice*

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1. INTRODUCTION

Human computers and scanners were scientific workers who performed calculations, reduced and analysed data, made scientific discoveries, and produced scientific knowledge before the advent of electronic computers (Light 1999; Croarken 2003). Their history spans many areas of science from astronomy, to particle physics, to computer science (Grier 2005; Galison 1997; Ceruzzi 1991). Human computers produced classifications of stars (Hoffleit 2002), Nautical Almanacs (Croarken 2003), calculated trajectories for space missions (Edwards and Duchess Harris 2017), and programmed the first (electronic) computers (Light 1999). Scanners were a staple of big data physics laboratories during and after the Second World War (WW2) (Galison 1997). Most of these scientific workers were professionally untrained women and most big data science projects hired at least a dozen such scientific workers at any one time. Yet, despite the prevalence of human computers or scanners within the context of scientific projects that produced or processed large amounts of data during the 19th century and early to mid-20th century, their epistemic roles within these projects remain virtually unexamined. Philosophers of science have paid little attention to how such practices influence the epistemology of instruments and experiments or the methodology of science more broadly. For instance, the question of whether using professionally unqualified women to analyse the data introduces or suppresses experimentation biases is yet to receive an answer.

This paper investigates the epistemic roles of the Harvard Human Computers at the Harvard College Observatory 1880-1920 and of the Bristol Scanners at the Bristol Nuclear Research Group 1935-1955. We argue that both scanners and human computers were, by and large, *instrumentalised*, by which we mean that they were treated as scientific instruments. Their instrumentalisation comes via two distinct paths. On the one hand, the assumptions inherent within their prescribed roles entails both a trivialisation of their cognitive and epistemic abilities and an underestimation of their important roles in knowledge production processes. On the other hand, their instrumentalisation is the result of epistemic oppression. In particular, scanners and human computers who transcended their prescribed roles were denied agency as knowledge producers.

This paper identifies, evaluates, and compares three frameworks which can helps us understand the epistemic roles of scanners and human computers, their instumentalisation, and the consequences thereof. The first is what we will call *the institutional framework*. This framework is reconstructed from the history of women in science by Rossiter (1982, 1993) and from primary sources that detail the work of scanners and human computers and the institutional norms governing it. The second framework is what we will call *the functional framework*. This framework is reconstructed from the work of Shapin (1995) on the history of technicians' work and their functional roles in big science.[1]

The institutional and functional frameworks are both concerned with the underlying conditions and the ensuing limitations of the creation and perpetuation of human computers and scanners roles. They each help us understand how such roles came to be and how they were legitimised within the relevant scientific activity. Furthermore, they help us understand how human computers and scanners came to be instrumentalised. The institutional framework helps us understand why the labour force was constituted by women and why their work was minimised and distorted by offering a gendered interpretation of the human computers' and scanners' instrumentalisation. The functional framework similarly helps us understand how the workforce came to be constituted by women, but, unlike the institutional framework offers a different interpretation of the scanners' and human computers' intrumentalisation; it locates their intrumentalisation within a practice of seeing technicians as skilled, yet lacking in epistemic authority qua interpreters or theoreticians of the phenomena they identified and analysed.

Finally, the third framework is *the epistemic injustice framework*, originally proposed by Fricker (2007). This framework is here extended to include *knowledge production injustices*, which we take to refer to the devaluation or discreditation of individuals or groups as knowledge producers. We argue that the epistemic injustice framework helps us conceptualise the consequences of the human computers' and scanners' intrumentalisation. In particular, we show that due to their gender and role identities, computers and scanners were either discredited or not fully acknowledged as knowledge producers.

By combining the three frameworks, we ultimately show that the instrumentalisation of scanners and human computers is significant, not only in terms of the negative consequences for the individuals, such as lack of credit and recognition, but more saliently in terms of the negative consequences for the methodology of science. With reference to specific examples, we show that denying a scientific worker their capacity to produce scientific knowledge or discrediting their scientific

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[1] 'Big science' refers to large scale-scientific collaborations organised around big and expensive instruments. Technician stands for a broader category of scientific workers which includes laborants and assistants and thus can be said to include both scanners and human computers.

knowledge claims may: i) stunt the advancement of scientific knowledge; ii) discredit particular types of knowledge or methods; iii) render invisible biases of both scanners/human computers and knowledge validators; and, finally; iv) lead to the distortion of historical facts.

This paper makes four novel contributions. First, it undertakes a systematic investigation of an epistemic nature of the role of scanners and human computers which has not yet been undertaken in any field. Second, the paper investigates not only the neglected epistemic roles of scanners and human computers and the consequences of such neglect for the individual scientific workers, but, more importantly, the downstream consequences of their neglect for the methodology of science. Third, the paper offers a novel comparative analysis of two foundational case studies in big data science. Fourth, we identifie and synthesise three frameworks that can be used to understand the instrumentalisation of scientific workers within big data science. The case of scanners and human computers is crucial for future philosophical and historical work which investigates how scientific knowledge is produced and legitimised in big data science and beyond.

The paper is structured as follows: Section 2 focusses on the Harvard Human Computers at the Harvard College Observatory 1880-1920. Section 3 focusses on the Bristol Scanners at the Bristol Nuclear Research Group 1935-1955. Section 4 examines the institutional background and broader socio-economic conditions that precipitated the creation of a human computers' and scanners' workforce and explores the circumstances that led to their instrumentalisation. Section 5 offers a more nuanced explanation of the instrumentalisation of the Bristol scanners by investigating the functional aspects of their instrumentalisation. Section 6 offers a conceptualisation of the scanners' and human computers' instrumentalisation which helps us identify not only the negative consequences experienced by the individuals, but, more significantly, pernicious downstream consequences for the methodology of science. Finally, section 7 concludes the paper.

2. THE HARVARD COLLEGE OBSERVATORY 1880-1920

The last few decades of the 19th century marked a turning point in astronomy. The development of modern astrophysics, spurred on by the discovery and improvement of spectral analysis and celestial photography, is intertwined with the development of ever more powerful instruments, as well as new methods of investigating celestial phenomena. Stellar spectroscopy, a method of photographing stars by attaching a prism onto the object-glass of a telescope which disperses the light coming from the stars into relevant component colours, enabled astronomers to gather a wealth of data, in the form of photographic plates. The plates were of not only the stars that could be seen through the (then) standard telescope but also of stars situated in some of the remotest parts of the galaxy that lay beyond the visible line of sight. Capturing numberless stars on photographs and having the photographs at hand for examination, re-examination, and classification, was an invaluable tool for discovering new stars, as well as for taking various measurements of stars that could hold the key not only to their constitution and origin, but would also enable the specification of the velocity of stars and their motions. All this required a large staff of trained computers who could take careful and detailed measurements of the brightness, position, and colour of stars which would ultimately provide modern astronomy with a trove of information with regard to the physical and chemical properties of stars. This painstaking work, was undertaken by human computers at the Harvard College Observatory between 1880-1920.[2] Here we focus on only two of the computers, Williamina Fleming and Antonia Maury for three reasons: i) they exemplify the painstaking work undertaken by computers; ii) they are paradigmatic examples of computers who transcended their roles and undertook independent original scientific work; and iii) their employment experience and relationship to authority was markedly different, a significant contrast as we will explain below.

Williamina Fleming joined the Harvard College Observatory as a computer in 1881 and her initial tasks involved "copying and computing" and "supplying copy for the Harvard photometry Catalogue" (Haley 2017, 3). When the work of the Henry Draper Memorial started in 1886, Fleming was put in charge of the "examination, physical care, classification, and indexing of thousands of glass plates" (id. 7), and as the work expanded she was further given managerial responsibilities such as recruiting, "training, monitoring and planning [the computer's] work schedules" (id. 8). Further, she was supporting the Observatory's director, Edward C. Pickering, with the Observatory's correspondence, and editorial work relating to the Observatory's publications. On top of her curatorial, managerial, secretarial, and editorial work, Fleming was also undertaking research work which consisted in the analysis of spectra, the identification of novel celestial denizens such as variable stars, gaseous nebulae, novae, and Wolf-Rayet Stars. Besides her discovery work based on the analysis of star spectra, her research further extended to the creation

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[2] This period constitutes the main focus of this paper; computers were hired at the Harvard College Observatory past the 1920 cutoff.

of an empirical classification system of the stars, in collaboration with Pickering, which was published as the Draper Catalogue of Stellar Spectra in 1890.

Fleming was not the only computer who transcended her role, as well as the dominant view regarding women's skills, powers, and capacities prevalent at the time. Annie J. Cannon, Antonia Maury, and Henrietta Leavitt Swan are some of the most well-known Harvard computers who came to be recognised, for their achievements not only with the benefit of hindsight, but also during their lifetime. Their recognition, was not, however, equally bestowed, nor was it unfraught.

Antonia Maury is perhaps one of the most unusual of the computers in that she not only undertook original work beyond discovery work, but she also fought for her auctorial rights. Antonia Maury joined the Harvard College Observatory as a human computer in 1888 after graduating her Vasaar B.A. with "honours in physics, astronomy, and philosophy" (Sobel 2016, 31). She was recommended to Pickering as "ha[ving] unusual ability in a scientific direction" (id.) by the patron of the Henry Draper Catalogue, Anna Draper Palmer. Maury's scientific propensity and prowess, whilst helping her penetrate beyond the surface of spectral analysis classification, also stood in the way of her performing her prescribed computer role, leading to a disruptive employment experience. Fleming too, complained about her employment conditions, but unlike Maury, she did what was told and the relations between Fleming and Pickering remained cordial, something which cannot be fully affirmed in Maury's case. This contrast is important as it points to the fact that the possibilities and restrictions related to undertaking original work as a computer and getting recognition for independent scientific work very much depended on the type of relationship established with the relevant authority.

Maury's first work at the observatory concerned the calculation of the orbit of the spectroscopic binary Zeta Ursae Majoris Mizar, dicovered by Pickering. She was further instructed to analyse and classify the spectra of the brightest stars in the northern hemisphere according to Fleming's criteria (Sobel 2016). Her analysis of 4800 photographs, particularly of the spectra of 681 stars, led her to discover a second spectroscopic binary, Beta Aurigae and to observe previously unrecognised details in the spectra of stars. Besides width and strength, the main characteristics of spectra recognised by Fleming and Pickering, she identified additional noteworthy patterns, such as fluteness and haziness. Observations of such patterns led her to suspect that further information about the constitution and evolution of stars may be obtained from further pursuing and systematising these new patterns alongside the patterns observed by Fleming. Maury proceeded to design a new classification system which incorporated these differences. The new system recognised "22 groups in a sequence of descending temperature with a concurrent scheme which also classified the spectra by the width and distinctness of line" (Vassar Encyclopaedia 2008, 4). Despite the fact that her work had a significant impact on further discoveries of the relationship between stellar luminosity and stellar temperature and was recognised at the time by astronomer Ejnar Hertzsprung as "the most important advancement in stellar classification since the travails of Vogel and Secchi" (Hoffleit 2002, 385), Maury's efforts were not appreciated and she was asked to hand over her work to another computer. This fact explains her fraught employment experience and her fight to assert her auctorial rights and also demonstrates her fungibility as a computer. Defending her right to receive due recognition for her scientific work, she writes to Pickering, as follows:

> I do not think it is fair that I should pass the work into other hands until it can stand as work done by me. I worked out the theory at the cost of much thought and elaborate comparison and I think that I should have full credit for my theory of the relations of the star spectra and also for my theories regarding Beta Lyrae. (Vassar Encyclopaedia 2008).

Maury's classification was eventually published in 1897 under her own name, but her two dimensional classification system was deemed too cumbersome and her theoretical insights were not followed up. According to astronomer Dorris Hoffleit, this episode had dramatic consequences for the development of astronomy. She notes that:

> [i]f only Pickering had appreciated Maury's conclusions and accepted and acted upon Hertzsprung's remarks, a two-dimensional system would have evolved at Harvard 30 years before the currently preferred MK system.(Hoffleit 2002, 386)

Whilst this may not be a significant time lag, and indeed rediscoveries are sometimes inevitable, delays such as this are not unavoidable. Furthermore, this case is particularly instructive because the reasons for the misrecognition of Maury's scientific discoveries are not epistemic, but are instead related to institutional and functional aspects of scientific knowledge production, as we will explain below.

The contrast between Fleming and Maury and less known computers such as Mary Wagner on the one hand, and between Fleming and Maury on the other hand, is telling for several reasons. First, it shows that whilst some computers could break beyond their prescribed roles, most computers undertook routine work.[3] Second, it shows that even the computers who transcended their prescribed roles, did so within particular restrictions. Finally, even when computers produced important scientific knowledge, their contributions were not duly acknowledged or credited. These observations support the more general thesis that human computers were denied agency as knowledge producers. We will further show that the denial of agency as knowledge producers has problematic consequences not only for the individuals involved, but, more importantly, for the methodology of science. To properly untangle such consequences, let us analyse further evidence in the context of the Bristol Scanners.

3. The Bristol Nuclear Research Group 1935-1955

The development of the nuclear emulsion method had a transformative effect on the methodology of nuclear and particle physics. A nuclear emulsion is a type of photographic plate that can function as a particle detector (Herz and Lock 1966). The nuclear emulsion method was pioneered by Marietta Blau, who was the first to study cosmic radiation via nuclear emulsion plates and obtain records of individual fast charged particles (Sime 2013). Of particular interest amongst the events recorded were 'stars' or disintegration events due to cosmic rays obtained from exposing emulsion plates at high mountain altitudes. The nuclear emulsion method was further improved by Cecil Powell. Powell adapted and improved the new method in two ways. Emulsion plates were exposed for long periods of time at higher mountain altitudes and at balloon flights altitudes (Lock 1997). Further, through government sponsored collaborations with Ilford Ltd. and Kodak, Powell obtained more sensitive and thicker emulsion plates which could record more detail and longer particle tracks. Powell found the method compelling first for its "extreme simplicity" (Powell 1987, 16).

By 1938, further advantages of the emulsion method besides its simplicity become apparent. The method was also versatile and efficient. As Powell notes, the method made it "extremely simple to make the exposures and an enormous amount of information can be contain[ed] in a single small piece of plate" and further it had "no associated gear", which meant it was "possible to make experiments at high

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[3] In some cases, assumptions about the computers/scanners were driven by epistemic reasons, in the sense that particular care was taken to ensure that individuals lack relevant knowledge or understanding in order to avoid the theory-ladeness of observation. In the cases we focus on here, most scanners and computers did have relevant knowledge and assumptions about their powers and capacities were not epistemically driven.

potential" (Galison 1997, 168). However, the method was also challenging in other respects. First, it had to be transformed from a qualitative method of identifying and measuring particle tracks to a reliable quantitative method for the systematic study and measurement of particle tracks and other nuclear events (Herz and Lock 1966). Second, since the nuclear emulsion plates contained an abundance of data, the data had to be reduced, analysed, and interpreted in a consistent way. Third, the reliability of the method had to be increased though independent checks. As Powell notes, "[t]he most important technical problem [...] is to establish a team of observers and a routine of measurements in order to increase the speed at which results can be obtained" (Galison 1997, 198).

Based on a previous experience of hiring "untrained observers" for making routine observations of seismic activity in Montserrat (Powell 1936), Powell made a case for hiring a team of scanners and for the acquisition of microscopes, without which it would have been impossible to sift through the rich nuclear emulsion data. Since the work was tedious and repetitive, "Powell ... convinced everyone that it was possible to train young women, with no formal knowledge of physics, to perform this exacting work with expertise and meticulous accuracy" (Frank and Perkins 1971, 549). Another reason for hiring women was also related to the conditions in which the scanning work developed: WW2 was impending and thus a male workforce was harder to come by. This was how the practice of employing women, known as 'scanners' or 'scanning girls' in big data physics laboratories became entrenched. Powell's biographers, Frank and Perkins (1971), describe this as a "vital innovation, necessary for the successful prosecution of the researches with the emulsion method" (549).

The nuclear emulsion method gave rise to so much data that it would have been nearly impossible even for a large team of physicists to reduce it and analyse it whilst also getting on with their manifold responsibilities. A single nuclear emulsion plate "4 square centimetres of Ilford halftone 100-micron-thick" contained approximately 1700 tracks and required "60 hours of scanning" (Galison 1997, 175). The first scanner was hired in 1939 and more scanners were recruited amongst the wives of the physicists. Isobel Powell was amongst the first scanners (Fowler 1995), and Irene Roberts, the wife of Max Roberts joined later. In 1949 the team of scanners was composed of Mary Cole, Mrs. M. L. Andrews, Mary Merritt, Peggy Ford, Miss P. Dyer, June Cowie, Grace Hussey, Mrs. B. Moore, Mrs. J. Van den Merwe, Mary Jones, Margaret Stott, as well as Isobel Powell. The scanners' work was minutious and repetitive and could cause significant strain on the eyes. The scanners worked seven hours a day peering through their microscopes. Their work was interrupted by two twenty minutes breaks in the morning and afternoon respectively. Due to the "nervous strain attached to this work", an observer would not be "kept too long on the job, at any one time" (Galison 1997, 198). The team of scanners numbered approximately two dozens over the course of the scientific investigations with nuclear emulsions, though not always the same two dozens (Galison 1997, 199).

Equipped with a microscope, the scanner's job was to examine the emulsion plates in search of "specified topologies of events" (Galison 1997, 198). In a laboratory notebook, the scanner would note the section of the plate assigned to them and record the events observed alongside their coordinates within the emulsion plate. The notebook entries feature, besides coordinates, a brief description and a drawing of the track or of any unusual event, a specification of whether secondary particles were present, and measurements of the track itself.[4] The scanners were trained to recognise particular tracks, such as proton tracks and later meson tracks. At first, any unusual event would be scrutinised by a physicist, but as Powell notes:

> The observers soon learned to recognise the tracks of mesons and we found many examples of similar disintegrations produced at the end of their range. Indeed the interest and liveliness of the observers was a crucial element in the progress of the work. They learned to distinguish by inspection the tracks of stopping mesons, of mass about two hundred times that of the electron, from those of protons and alpha-particles, for there are characteristic differences between them which an experienced observer soon recognises. And we took a good deal of trouble to help them to learn to interpret the events they found and to understand the significance of what they were doing. (Powell 1987, 21).

In the early years, the scanners not only joined fully in the investigations but "were also granted a kind of quasi-scientific authorship" (Galison 1997, 199), their names being attached to the plates featuring relevant discoveries. In fact, it was two of the scanners, Marietta Kurz and Irene Roberts, who first discovered in 1947, in succession, the disintegration events identified with the decay of a pion into a muon (Lock 1997). Later, as the work became more and more standardised, the identification of the events on the plates was 'demoted' from discovery to 'finding' and the scanner's names were removed from official publications. However, the

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[4] The descriptions of the notebooks are based on the author's own examination of the primary archival material.

identification of a novel event could only be done by a highly trained observer and required not only skill but also interpretation. An observer needed to have

> "the ability to recognize quickly many different types of sub-atomic events. To acquire skill in interpretation, a preliminary study must [have] be[en] made of many examples of photographs of the different kinds of known events. Only when all known types of event can be recognized will the hitherto unknown be detected" (Blackett, quoted in (Daston and Galison 2007, 344)).

The identification of an event by a skilled observer, be it physicist or scanner, required the same skill, as the quote from P.M.S. Blackett demonstrates. However, one may insist that the scanner could identify the event *merely* as a novel phenomenon but not as phenomenon of a particular type. That is, the physicist, based on specialist physics training had a much richer interpretation of the *nature* of the phenomena, which the scanner did not. Indeed, one of the prevailing views of scientific discovery requires that the discoverer has a correct or nearly correct theoretical interpretation for the identification of a phenomenon to count as a bone fide discovery (Kuhn 1962; Schindler 2015). On such views, the finding/discovering distinction may appear legitimate. On other views, however, the restrictions imposed on discoverers are less stringent. Such views require only that the discoverer be in a salient epistemic situation for making a relevant discovery (Achinstein 2001); which the scanners certainly were. Our aim here is not to offer a pronouncement on the logic of discovery, but merely to show that the finding/discovering distinction is artificial. With recourse to three explanatory frameworks we show that the distinction marks the emergence of a double standard that distorted and minimised the scanners' work. The three frameworks, taken together, will help us understand the epistemic roles of scanners and human computers, their instrumentalisation, and the consequences thereof.

4. THE INSTITUTIONAL FRAMEWORK

The institutional framework is principally concerned with the socio-economic conditions that led to the development of a scientific labour market which both enhanced and restricted women's work in science in the United States (US) between 1880s and 1920s. Despite varying socio-economic and cultural disparities between the US during 1880-1920 and England 1935-1955, the institutional framework can fruitfully be extended to the case of the Bristol Scanners since both practices are similar in terms of the work undertaken by scanners and computers and the conditions which gave rise to these types of scientific employment. The practice of employing scanners and human computers can be understood along three dimensions. First, there was more demand for employment opportunities in science due to increased educational opportunities for women, as well as more employment opportunities facilitated by "the changing structure of scientific work in the 1880s and after" (Rossiter 1982, 51). For instance, the "rise of "big science" or large budgets which could support staff or assistants at a few research centers" (Rossiter 1982, 53) was a major factor in the creation and perpetuation of human computer and scanner roles. This was particularly true of the Harvard Observatory, which relied on the work of human computers for fulfilling its major project of cataloguing the spectra of stars (Haley 2017). The Bristol Research Nuclear Group, similarly, relied on its scanners to identify novel nuclear phenomena in photographic emulsions. Second, there was still a "strong resistance to [the] female workforce entering traditional kinds of scientific employment (Rossiter 1982, 51), which restricted the kinds of roles and positions women could occupy. Due to the dearth of opportunities for remunerated scientific work, women accepted jobs as scanners or human computers even if they would often get stuck in such lowly paid jobs throughout their career. Third, both practices exemplify the "proletarianisation" phenomenon, according to which a job would be first downgraded and later feminised (Rossiter 1982, 56).[5]

The "proletarianisation" or deskilling phenomenon, carries with it not only socio-economic implications, but also epistemological. The tedious and laborious work that scanners and computers did "often required great docility or painstaking attention to detail" (Rossiter 1982, 53), and despite the fact that male assistants had done such work in the past[6], the skills involved in such work, as well as the work itself, came to be associated with "women's work". Even well-meaning supporters of women's work in science saw women as more qualified for particular tasks. For instance, in his address to a national convention, John Raymond, the first president of Vassar College, remarked that:

> In many of the processes of the laboratory, in the arrangement and care of great collections, in the keeping of minute and voluminous records, in difficult and delicate computations, and in like work of which there is so much to be done in chemistry, astronomy, and in

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[5] Hicks (2017) has written at length about deskilling and feminisation of work.

[6] Male assistants who carried out computer's work were unlikely to remain in such positions for long, leaving as soon as better opportunities arose.

> the whole range of natural history, and on the manner of doing which so much is often depending, one thoroughly trained woman is often worth any number of young men, who with rare and womanly exceptions, cannot do such work well if they try, and would not want to if they could. (Bergland 2008, xvi).

Feminised jobs did not only refer to subordinate, low paid positions, they also came to signify women's skills, capacities, and abilities. That this was the prevalent position of the time we can see not only from Rossiter (1982) history of women's work in science between 1880-1920, but also from popular articles regarding "women's powers" and work in science at the time. For instance, a Mrs. Buckler (1897), writing for the The North American Review in 1897 notes that although "women engaged in th[e] science [of astronomy] ... are doing good service in the study of photographs under the microscope or in the observation of sunspots and eclipses", "women as discoverers" and as inventors are "inferior to men" (306). In 1927, The Harvard Bulletin still proselytised the attitude according to which women were more competent than men in "work requiring infinite care and detail", as well as "generally more painstaking, more enthusiastic and less apt to grow tired, in meticulous, exacting labor" (Gordon 1978). This attitude is not surprising against the background of women's suffrage, theories regarding correlations between women's brain size and their cognitive capacities, and the formal limitations to women's acceptance into universities, learned societies and more generally the job labour market. In this broader context, it is not surprising that feminised jobs were not only subordinated in terms of institutional hierarchy as workers, but also in terms of knowledge hierarchy as knowledge producers. However, the socio-hierarchical norms that sieved into the knowledge making process did not accurately represent the women's actual skills, capacities, abilities, and accomplishments.

Applied specifically to the Harvard Observatory case, the institutional framework captures some of the primary motivations and justifications behind hiring women as human computers to do the exacting work required to classify the millions of stars captured on photographic plates. The reasons were not merely mercenary, but were also permeated by the then dominant attitude towards women's skills, capacities, and abilities as explained above. Writing to George Ellery Hale in 1901, Frank Schlesinger confides that he is "thoroughly in favour of employing women as measurers and computers" and he believes "their services might well be extended to other departments" (Hoffleit 2002, 370). Explicating his reasons he notes that: "[n]ot only are women available at smaller salaries than men, but for routine work they have important advantages. Men are more likely to grow impatient after the novelty of the work has worn off and would be harder to retain for that reason" (id.). Schlesinger's attitude was not in the minority. The same attitude was internalised by the computers themselves and was proselytised in popular magazines as detailed above. For instance, one of the most successful of the human computers, Williamina Fleming, writes the following about hers and other computers' work:

> March 1st 1900 in the Astrophotographic building of the Observatory 12 women, including myself, are engaged in the care of the photographs; identification, examination, and measurement of them; reduction of these measurements, and preparation of results for the printer. The measurements made with the meridian photometers are also reduced and prepared for publication in this department of the Observatory. From day to day, my duties at the Observatory are so nearly alike that there will be but little to describe outside ordinary routine work of measurement, examination of photographs, and work involved in the reduction of these observations. (Fleming 1900)

Beneath the veneer of routine with which Fleming characterises not only her work, but that of other computers, there are many interrelated tasks and responsibilities and a great deal of independent and original work as detailed in section 2. Both Fleming and Maury, and others undertook original independent work and transcended their prescribed roles throughout their employment. Yet, despite their significant discoveries, they could not altogether transcend the prohibitive sociohierarchical norms which devalued them as knowledge producers.

Daston and Galison (2007) offer a similar interpretation to the institutional framework. They argue that according to the prevalent norms of scientific objectivity at the time, the work of the human computers at the Harvard Observatory was perceived as mechanical work. Human computers came to embody the same virtues as machines, which "were paragons of certain virtues" such as "patien[ce], indefatigable[ness], ever-alert[ness]" (Daston and Galison 2007, 123); "[m]achines were [also] ignorant of theory and incapable of speculation" (id. 123). Human computers, like machines, offered "in their "emptiness" a transparency through which nature could speak" (id. 341). In fact, "women workers were presumed to offer a "natural" predilection away from the speculative tradition" (id. 341), preexisting theoretical commitment, and interpretative temptation (Daston and Galison 2007).

Similar qualities were ascribed to the Bristol Scanners, a group of "young women, with no formal knowledge of physics, [trained] to perform th[e] exacting work [of scanning emulsion plates] with expertise and meticulous accuracy" (Frank and Perkins 1971, 549). The scanners' skills were minimised and their discoveries were demoted to 'findings'. Some of the anonymising language used to refer to scanners further indicates that in some sense they were no different to machines or instruments. For instance there are references to the scanning work as done by "C.F.P., Fertel Stobbe and girl" (Frank and Perkins 1971, 546), or to Powell's need to request "three more microscopes and three girls" (Galison 1997, 176).

The institutional framework supports our view that both human computers and scanners were instrumentalised.[7] In particular, it shows that i) human computers and scanners were denied *autonomy or self-determination* as they came to perform prescribed routinised tasks and were denied opportunities for other types of work; ii) they were *fungible or interchangeable* insofar as they were perceived as uniquely fit for the work but no particular computer or scanner was regarded as irreplaceable; and iii) they were *denied subjectivity*, manifested in the belief that women could perform prescribed routine tasks without getting bored or fatigued which invited, at least to some extent, the suppression of their feelings and experiences. The feminised interpretation of their roles deprived them of relevant agency and competencies. Both scanners and computers were *denied agency as knowledge producers*. Furthermore, both groups were perceived as lacking the hallmarks of a knowledge producer, such as insight, initiative, and originality. Both groups were denied the chance to participate in activities deemed to denote the value of scientific contributions, such as "problem selection, instrument design, methodological approach and data interpretation" (Becker 2012, 187).

The institutional framework helps us understand why scanners and human computers were devalued as knowledge producers. Yet they do not constitute a homogenous workforce and thus there are significant differences amongst human computers on the one hand, and between scanners and human computers on the other hand. First, not all human computers performed the same kind of work. There are significant differences amongst the human computers in terms of their roles, responsibilities, and recognition. For instance, human computers such as Williamina Fleming, Annie J. Cannon, Antonia Maury, and Henrietta Swan Levitt received

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[7] Instrumentalisation, overlaps with, but it is not the same as objectification. For feminist theories of objectification see Nussbaum (1995) and Papadaki (2010).

recognition during their lifetimes, albeit not matched by their salaries which remained lower than those of male assistants. Others, such as Mary Wagner, remained anonymous and were omitted from Solon Bailey's (1931) "History and Work of the Harvard Observatory" (Zrull 2021). Second, whilst some human computers undertook work beyond routine tasks, most of the work performed by scanners was routine. Third, whilst at least some human computers enjoyed auctorial rights and recognition, scanners were briefly given quasi-auctorial rights only to have their names removed from official publications (Galison 1997). Thus, it is important to recognise that the instrumentalisation of scanners and human computers comes in different shapes and varieties.

The institutional framework offers only a partial explanation of the instrumentalisation of scanners and human computers. Their instrumentalisation served different purposes. In the case of human computers it was primarily the prevailing socio-economic and cultural norms and their meagre wages that contributed to their intrumentalisation. We cannot, however, attribute the scanner's instrumentalisation solely to institutional factors. Whilst, gender prejudices may have played a role in the suppression of their quasi-authorship roles, a more nuanced explanation is needed for their instrumentalisation. One reason to resist a gender focussed interpretation is that there were other women working in the Nuclear Research Group as physicists, such as Rosemary Brown and Connie Dilworth, who did receive credit for their contributions. A distinct, and more general, reason to resist a purely gendered interpretation consists in the fact that similarly placed workers encountered some of the same difficulties with their work or methods being adequately recognised or valued due to intersectional reasons that go beyond gender. For instance, black male technicians (Timmermans 2003) or even white, professionally untrained, male scientific workers (Becker 2012) were not given the epistemic authority commensurate with their work or findings. A more likely interpretation, to which we turn to in the next section, is that the scanners were denied due credit and recognition not only because they were women, but because of their functional roles within the economy of scientific work.

5. THE FUNCTIONAL FRAMEWORK

The functional framework, reconstructed from Shapin's historical work on Boyle's technicians, is concerned with the role of scientific workers within particular processes of scientific knowledge production. We argue that Shapin's remarks on the epistemic role of technicians and on their invisibility, extend to scanners and human computers too. Furthermore, Shapin's framework helps us understand the reasons behind the minimisation of scanners' work. In particular, the dichotomy between skill and epistemic authority or "knowledgeability", stripped of the social hierarchies of seventeenth century England, helps us better contextualise the dichotomy between 'finding' and 'discovering'.

Shapin's focuss on the "epistemic role of support personnel" (id.) is geared towards a) highlighting the historical invisibility of such workers, and b) demonstrating the "collective nature of experimental knowledge" (id.). He argues that support personnel have been "triply invisible" because i) they had always played at most a marginal role in sociological and historical accounts of science; ii) they are largely absent from formal records; and, finally iii) they were perceived as an interchangeable, fungible workforce (id., 360). Scanners and human computers too have been triply invisible for the same reasons. Moreover, as Shapin notes, "[e]ven when one is committed to doing so it is extremely difficult to retrieve information about who they were and what they did" (id.). This is particularly true of the Bristol scanners, as information regarding their qualifications and education, and sometimes their full names, is difficult to retrieve due to lack of relevant formal records. As regards the "collective nature of experimental knowledge", at least in the case of the Bristol Nuclear Research Group and the Harvard College Observatory, it is difficult to see how knowledge could have been created otherwise. Both projects amassed so much data, that its production, collection, analysis, and interpretation required a proportionate workforce. That said, the question of how credit and recognition should be apportioned in such cases remains open. Prima facie, everyone should be apportioned credit proportionate with their skill and the work undertaken. But, as Shapin shows, when the evaluation of skills and work is skewed by various socio-cultural biases, credit and recognition may not be bestowed where they are due.

What Shapin shows is that experimental skill or craft skill was defined "in practical opposition to notions like knowledgeability" (id., 361) or epistemic authority. Skill, which was associated with manual labour or repetitive activity was not considered genuine knowledge. A skilled labourer was subservient to their employer and was perceived as someone who could not produce work of independent scientific merit. The knowledge possessed by a skilled worker was "not thought to be marked by the individual signature of those who did it" (id., 380); it was thought as being pre-reflective. Technicians and other skilled labourers were fungible and interchangeable. Even those technicians which were perceived as having a higher degree of manual skill were not secure from being replaced. Despite the fact that they had direct contact with the raw data and that they learned how to distinguish phenomena from the idiosyncrasies of the experimental apparatus, their "experience did not in itself confer epistemic value on technicians' understanding" (id.). Whether or not a technician was given an identity and a voice was entirely at the disposal of the employer. As Shapin (1995) notes:

> Whether such direct and tacit experience was recognised by employers, how it was valued, whether it was encouraged or allowed to speak, and whether its voice was then represented, all depended upon the moral texture of social relations in the workplace (381)

The functional role of the technician bears two defining characteristics: 'dependence upon their employer' and a 'reputation' for their skills rather than their knowledge. It is these two characteristics, which, according to Shapin, act to discredit the technicians' claims on scientific matters. It is not difficult to see how Shapin's remarks on technicians extend to scanners and human computers. We have seen, in both cases, the precarity of their employment and their fungibility. We have also seen, in the case of Fleming and Maury, how their differential treatment and recognition depended on Pickering's authority. Further, in both cases it was amply demonstrated that the scanners' and computers' work, though in some cases recognised as requiring a good amount of skill, was nonetheless regarded as repetitive and automatic. So much so, at least in the case of scanners, that it was subsumed into the workings of the instruments. Or, as Shapin aptly puts it "[...] technicians's observational and representational labour was transparently subsumed into the workings of the instruments without attribution of assisting human agency: 'it was found'"(id., 385).

We set out to argue that scanners and human computers were denied agency as knowledge producers. The analysis of the institutional framework helped us pick out some of the gendered motivations behind the treatment of the human computers, yet this interpretation could not fully be extended to the denial of agency in the scanners' case. The functional framework adds to a gendered interpretation, a more nuanced interpretation of the scanners' predicament: it was their functional role within the knowledge production economy that precluded them from being recognised as genuine knowledge producers. In particular, their skills were subsumed to those of highly functioning instruments and their subjectivity became one with the instruments. As a consequence, it became immaterial which scanner 'found' which event, since any one of them could ‘happen’ upon it. Even if we accept that any skilled scientific worker can ‘find’ an event, this does not necessarily mean that their identity should not be acknowledged and that proportionate credit be bestowed. In the next section we introduce a final framework to help us identify the particularly pernicious consequences of the scanners’ and computer’s instrumentalisation.

6. The Epistemic Injustice Framework

The epistemic injustice framework, originally developed by Miranda Fricker in ‘Epistemic Injustice. Power and the Ethics of Knowing’ (2007) mapped the ethics of testimonial and hermeneutical interactions. Since Fricker’s seminal work, there has been a prolific expansion of the literature concerning epistemic injustice, focussing either on expanding the range of cases that qualify as epistemic injustices (Davis 2016; Kidd and Carel 2017; Lee 2021; Spencer 2023) or on clarifying the nature of the wrongs therein (McGlynn 2021). For instance, Davis (2016) shows that an epistemic injustice can occur not only as a result of credibility deficit, as per Fricker’s account, but also as a result of credibility excess. According to Davis, when one is the subject of “identity-prejudicial credibility excess’, they can be “treated as if he or she were fungible or interchangeable with others who share the same social identity” (Davis 2016, 487). For example, based on a stereotype that scientists are better informed than most people about scientific matters, we may give a scientist more credibility with regard to an issue on which they are not experts on. An example of this type of prejudicial credibility excess can be seen in cases where non-epidemiologists have given policy advice outside their remit on the recent COVID-19 pandemic (Oliver 2022).

Grasswick (2017) further extends the typology of epistemic injustice to science-related epistemic injustices such as “participatory and epistemic trust injustices” (315). Grasswick argues that participatory injustice occurs when various systems of oppression impede one to engage in or to contribute to cooperative inquiry or more specifically be involved in processes of scientific knowledge making. Epistemic trust injustices occur when subordinated groups cannot rely on expert knowledge as a result of systematic oppression. For instance, “the historical context of exploitation and differential rights documented [by Rebecca Tsosie, 2012] continues to impact [native] tribes’ ability to receive benefits from contemporary health care innovations, including genomic research and personalized medicine” (Tsosie 2012, 1170). A particularly relevant example here is that of the Havasupai tribe who consented to give blood samples for a diabetes study, but had their samples used for other purposes unspecified in the original consent forms. This has led the Havasupai tribe to file a lawsuit for the misuse of their blood samples (Tsosie 2012). Such misguided and objectionable practices lead to epistemic trust injustices and would entitle Havasupai tribe members, and others in similar situations, to reasonably be reluctant to participate in future medical research.

As regards the wrongs of epistemic injustice a distinction is usually made between the ‘primary wrong’ of epistemic injustice and the wrongs that accrue as consequences of an epistemic injustice. Fricker’s original account identifies the “primary” wrong of epistemic injustice with a form of epistemic objectification which “involves the denial of someone’s epistemic agency” (McGlynn 2021). In the case of scanners and human computers we encounter both types of harms.

We have already argued in sections 4 and 5 that the instrumentalisation of scanners and human computers led to their diminished epistemic agency, especially in their capacity as knowledge producers. We have also shown in section 2, 3, and 6 that in both cases their instrumentalisation had negative consequences on the individuals, such as lack of credit and recognition. In what follows, we combine the lessons from the three frameworks analysed above to examine the methodological consequences of instrumentalised scientific labour.

7. EPISTEMOLOGICAL CONSEQUENCES OF INSTRUMENTALISED SCIENTIFIC LABOUR

In this section we argue that the instrumentalisation of scanners and human computers has negative effects not only for the individual scientific workers, but leads to particularly pernicious consequences for the methodology of science. With reference to the examples discussed above, we show that denying a scientific worker their capacity to produce scientific knowledge or discrediting their scientific knowledge claims leads to four such pernicious consequences.

The first significant epistemological consequence of instrumentalised scientific labour for the methodology of science relates to situations in which the advancement of scientific knowledge is endangered by discrediting the knowledge produced by scientific workers based on an identity prejudice. The case of Antonia Maury is particularly telling. Maury’s ‘fine perceptual discriminations’ of the qualities of stellar spectra were disregarded by Pickering and others despite the fact that they pointed to a two dimensional classification of stars. Her discoveries further pointed to a significant relationship between stellar luminosity and stellar temperature, a relationship which became central to the successive classification of stars by Morgan, Keenan, and Kellman (Morgan et al. 1943). This is a stark example where the consequences of instrumentalising scientific labour have significantly delayed the progress of science thus adversely impacting the epistemology of science.

The second significant epistemological consequence of instrumentalising scientific labour for the methodology of science refers to situations in which the devaluation of human computers and scanners led to the discreditation of particular types of knowledge or methods. The case of Antonia Maury is once again telling. The methods by which she arrived at her significant discoveries were rooted in computer's work, that is, they were rooted in painstaking analysis of the spectra of stars. Whilst such work was perceived as highly skilled, it was not associated with relevant epistemic authority qua interpreter or theoretician of the phenomena. Despite Maury's intimate acquaintance with the spectra, her beyond routine work, such as designing methods for calibration, standardisation, and triangulation, was not fully appreciated. The tendency to "ferret... out all the details discernible in the spectra, [and] to ponder... what their significance might be" (Hoffleit 2002, 386), was linked not with epistemic authority but with skill and hard work. Her process lacked the relevant authority to propel her methods into legitimacy. Her instrumentalisation thus impeded the uptake of her methods within the scientific community.

The third significant epistemological consequence of instrumentalising scientific labour for the methodology of science refers to cases in which biases of both scanners/human computers and knowledge validators are rendered invisible. A case in point is the way in which the introduction of the artificial distinction between 'finding' and 'discovering' was introduced to delineate the 'mere' skill of the scanners from the epistemic authority of the physicist. This is not a moot point since this distinction problematises the way we conceptualise scientific discoveries and implies a hierarchy of discoveries that may discredit some discoveries based purely on methodology. To be exact, the 'finding/discovering' distinction implies that observational/empirical novelty is to be valued less than theoretical novelty. Such an incentive structure leads not only to the devaluation of work focussed on the former type of discovery, but could also lead to the prioritisation of one over the other which could result in negative consequences for experimental science or areas of science which value observational/empirical novelty.

Finally, the fourth significant epistemological consequence of instrumentalised scientific labour for the methodology of science refers to the distortion of historical facts. We have already shown that whilst the scanners played an invaluable role in the process of scientific knowledge production (Frank and Perkins 1971), their roles have not been recognised in publicly disseminated documents (Galison 1997). In fact, their names have been erased from such documents (Powell et al. 1959). Such acts, in turn, gave rise to flawed narratives that distort our understanding of the process of scientific knowledge making. Further, such mechanisms of erasure by which certain scientific contributors are actively removed from formal records result in scientific narratives which perpetuate simplistic myths about who produces scientific knowledge and how scientific knowledge is made.

8. CONCLUSION

This paper identifies, evaluates, and compares three frameworks for reconstituting and analysing the epistemic roles of human computers and scanners, their instrumentalisation, and consequences thereof. The institutional and functional framework helped us understand the conditions and motivations behind the human computers' and scanners' instrumentalisation and the epistemic injustice framework helped us examine the pernicious epistemic consequences of their instrumentalisation.

The central lesson of this paper is that the downstream epistemic consequences of the intrumentalisation of scanners and human computers negatively impact not only the individual, but also the methodology of science; an impact that has been underestimated. This paper constitutes an important first step in the development of an epistemology of scanners and human computers which is crucial in understanding the epistemological consequences of instrumentalising scientific workers more generally.

The significance of the paper cuts across two temporal dimensions. The analysis provided here helps us have a better historical understanding of scientific processes of knowledge making. This is the backward looking dimension. Looking forward, the analysis provided here can have a significant impact on the philosophical and normative assessment of epistemic work and epistemic injustice within large-scale collaborations which constitute a permanent fixture of contemporary science.

The present paper further examined mechanisms of denial, omission, and erasure which led to the underappreciation of the epistemic roles of human computers and scanners, their misrepresentation within the scientific record, and a skewed understanding of scientific knowledge making and progress. Thus, this paper constitutes a significant addition to the timely conversation about diversity in science and the historical invisibility of marginalised groups in the history of science.

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