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Human Computers as Instruments

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Abstract

Human computers and micro-observers 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 nineteenth and early to mid twentieth century. Despite their prevalence within big science, their epistemic roles remain unexamined by philosophy of science. This paper investigates the epistemic roles of the human computers at the Harvard College Observatory 1880–1920 and of micro-observers at the Bristol Nuclear Research Group 1935–1955; it argues that the instrumentalisation of human computers and micro-observers has negative epistemic consequences for the methodology of science.

1. Introduction

Human computers and micro-observers[1] 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) and nautical almanacs (Croarken 2003), calculated trajectories for space missions (Edwards and Duchess Harris 2017), and programmed the first (electronic) computers (Light 1999). Micro-observers were a staple of big physics laboratories during and after the Second World War (Galison 1997). Most of these scientific workers were professionally untrained[2] women, and most big science projects hired at least a dozen such scientific workers at any one time. Despite the prevalence of human computers or micro-observers within the context of scientific

[1] Micro-observers are known in historical and popular accounts as *scanners*. In employment records and laboratory notebooks, the Bristol workers were referred to as micro-observers, or simply observers. Since the word 'scanner' may be anachronistic, and has problematic connotations, the term 'micro-observer' is employed throughout.

[2] Most of the women did in fact have scientific training or prior scientific exposure, but their training did not have the relevant status to propel them to the higher echelons of 'professional' or even amateur scientists.

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projects that produced or processed large amounts of data during the nineteenth and early to mid twentieth centuries, 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.

Careful studies of scientific practices, and particularly of the roles and contributions of scientific workers, professional or not, invite myriad epistemic and normative questions. For instance, questions concerning under-valued epistemologies (who gets to contribute to scientific knowledge production and who does not?); questions regarding credit attribution (how is a fair distribution of credit in the context of scientific collaborations ensured?); or questions regarding the very nature of scientific institutions (are hierarchical scientific institutions fit for fairly representing the roles and epistemic contributions of a variety of workers?). Such questions have received scholarly attention in work by, for example, Massimi (2025) and Wylie (2015) in relation to under-valued epistemologies; Rossiter (1993) and Rubin and Schneider (2021) in relation to credit attribution; Wu (2023) and Ruetsche (2020) in relation to knowledge devaluation (especially in the context of minoritised scientific agents), and Fehr (2011) and Longino (1990) in relation to epistemic diversity in science.

This paper focuses on the epistemic roles of the human computers at the Harvard College Observatory 1880–1920 and of the micro-observers at the Bristol Nuclear Research Group 1935–1955 (known at the time as the Fourth Floor). This paper argues that both micro-observers and human computers were, by and large, *instrumentalised*, that is, they were treated as scientific instruments. Their instrumentalisation comes via two distinct paths. On the one hand, the assumptions inherent within their prescribed roles entail 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, micro-observers and human computers who transcended their prescribed roles were denied agency as knowledge producers.

This paper identifies, evaluates, and compares three frameworks that can help us understand the epistemic roles of micro-observers and human computers, their instumentalisation, and the consequences thereof. The first is what will be called *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 micro-observers and human computers and the institutional norms governing it. The second framework is what will be called *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.[3]

The third framework is *the epistemic injustice framework*, originally proposed by Fricker (2007). This framework is extended to include *knowledge production injustices*, which is taken to refer to the devaluation or discreditation of individuals or groups as

[3] 'Big science' refers to large-scale scientific collaborations organised around (usually) big and expensive instruments. 'Technician' represents a broader category of scientific workers that includes laboratory technicians and assistants, and thus can be said to include both micro-observers and human computers.

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knowledge producers. This paper argues that the epistemic injustice framework helps us conceptualise the consequences of the human computers' and micro-observers' intrumentalisation. In particular, it shows that, due to their gender and role identities, human computers and micro-observers were either discredited or not fully acknowledged as knowledge producers.

By combining the three frameworks, this paper ultimately shows that the instrumentalisation of micro-observers 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, this paper shows that denying a scientific worker their capacity to produce scientific knowledge or discrediting their scientific knowledge claims may: (i) stunt the advancement of scientific knowledge; (ii) discredit particular types of knowledge or methods; (iii) render invisible biases of both micro-observers/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 micro-observers and human computers that has not yet been undertaken in any field. Second, the paper investigates not only the neglected epistemic roles of micro-observers 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, it offers a novel comparative analysis of two foundational case studies in big science. And fourth, it identifies and synthesises three frameworks that can be used to understand the instrumentalisation of scientific workers within big science.

The paper is situated in the analytical landscape of scientific knowledge production and it will prove instructive for future philosophical and historical work that investigates how scientific knowledge is produced and legitimised in big science and beyond. It shows that a more complex understanding of the process and the people involved in knowledge production can allow us to isolate specific epistemic threats to scientific methodology that have previously gone unnoticed. The historical material is crucial for offering context and relevant details, yet it has been carefully balanced with philosophical analysis to draw out important philosophical arguments and lessons. It is worth highlighting that epistemology of science is here taken to mean 'the study of the formation, nature and justification of [scientific] knowledge' (Galison et al. 2023); and the epistemic consequences identified in the paper are concerned with these aspects of scientific knowledge production. The framework adopted in this paper is continuous with the New Experimentalism advocated by Laudan (1978), Hacking (1983), and Franklin (1989*a*, b). Concurrent with this tradition, this paper assumes that experimental knowledge is cumulative, particularly in terms of methods and heuristics, as well as in terms of domain-specific theories about empirical phenomena – for a recent discussion of experimental knowledge, see Boyd (2021).

The paper is structured as follows: section 2 focuses on the Harvard human computers; section 3 focuses on the Bristol micro-observers; section 4 explains the concept of 'instrumentalisation'; section 5 examines the institutional background; section 6 investigates the functional aspects of instrumentalisation; section 7 situates

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knowledge production injustice within the broader context of epistemic injustice; section 8 focuses on the pernicious epistemic consequences of instrumentalisation for the methodology of science; and section 9 concludes the paper.

2. The Harvard College Observatory 1880–1920

The last few decades of the nineteenth 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 and 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 their velocity and motions. All this required a large staff of trained scientific workers 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 and 1920.[4] This paper focuses 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 will be explained 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' (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 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

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

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spectra, her research further extended to the creation 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. 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' by the patron of the Henry Draper Catalogue, Anna Draper Palmer. Maury's scientific propensity and prowess, while 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 she was told and the relations between Fleming and Pickering remained cordial, something that 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 (Maury 1897). Besides width and strength, the main characteristics of spectra recognised by Fleming and Pickering, she identified additional noteworthy patterns such as flutedness 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 that 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

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computer. Defending her right to receive due recognition for her scientific work, she wrote to Pickering as follows:

> I do not think that it is fair to myself that I should pass the work onto other hands until it is [in] such shape that it can stand as work done by me.
>
> I do not mean that I need necessarily complete all the details of the classification, but that I should make a full statement of all the important results of the investigation[.] 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 in regard to β Lyrae. Would it not be fair that I should, at whatever time the results are published receive credit for whatever I have in writing in regard to these matters? Will you not please write me your opinions about the matter? (Maury to Pickering, May 7 1892, Harvard University Archives, UAV 630.14. Box 169, Folder 10. Antonia C. Maury, 1883–1903, Courtesy of the Harvard University Archives)

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 neither recognised nor followed up. According to astronomer Dorris Hoffleit, this episode had dramatic consequences for the development of astronomy. She noted 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)

Similarly, Philip C. Keenan, writing to Hoffleit, reconfirmed Maury's pioneering role in the creation of a luminosity classification:

> Bill Morgan and I are interested in trying to piece together a more complete account of the beginning of luminosity classification of the stars – and, of course, that means Antonia Maury. (Keenan to Hoffleit, 5 November 1980, Dorrit Hoffleit Papers, Schlesinger Library, Radcliffe Institute, Harvard University, Box 21, MC 529)

While 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 mis-recognition of Maury's scientific discoveries are not epistemic, but are instead related to institutional and functional aspects of scientific knowledge production, as will be explained below.

The contrast between Fleming and Maury and lesser 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 while some computers could break beyond their prescribed roles, most computers undertook routine work. Second, it shows that even the computers who transcended their prescribed roles did so within particular restrictions. Finally, even when computers produced important scientific

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knowledge, their contributions were not duly acknowledged or credited. These observations support the more general thesis that human computers were instrumentalised, and specifically denied agency as knowledge producers. This paper will further show that instrumentalisation 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 micro-observers.

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 among 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 in two ways. Emulsion plates were exposed for long periods of time at higher mountain altitudes and at balloon-flight altitudes (Lock 1997). Further, through government sponsored collaborations with Ilford Ltd. and Kodak, Powell obtained more sensitive and thicker emulsion plates that could record more detail and longer particle tracks. Powell found the method compelling for its 'extreme simplicity' (Powell 1987, 16).

By 1938, further advantages of the emulsion method had become apparent. The method was 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 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 micro-observers 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

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scanning work developed: the Second World War was impending and thus a male workforce was harder to come by. This was how the practice of employing women, known as 'micro-observers' 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 and analyse it while 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 micro-observer was hired in 1939, and more micro-observers were recruited among the wives of the physicists or technicians. Isobel Powell was amongst the first micro-observers (Fowler 1995), and Irene Roberts, the wife of Max Roberts, joined later. In 1949 the team of micro-observers was composed of Mary Cole, Margaret L. Andrews, Mary Merritt, Peggy Ford, Phyllis Dyer, June Cowie, Grace Hussey, Betty Moore, Winnie Van den Merwe, Mary Jones, Margaret Stott, and Isobel Powell. The micro-observers' work was minutious and repetitive, and could cause significant strain on the eyes. The micro-observers worked seven hours a day peering through their microscopes. Their work was interrupted by two twenty-minute breaks, one in the morning and one in the afternoon. 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 micro-observers numbered over a hundred over the course of the scientific investigations with nuclear emulsions.

Equipped with a microscope, the micro-observer's job was to examine the emulsion plates in search of 'specified topologies of events' (Galison 1997, 198). In a laboratory notebook, the micro-observer 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.[5] The micro-observers 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)

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

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In the early years, the micro-observers 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 micro-observers, 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, according to Galison (1997), 'demoted' from discovery to 'finding' and the micro-observers' names were removed from official publications. However, the 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, according to P.M.S. Blackett,

> 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. (Daston and Galison 2010, 344)

The identification of an event by a skilled observer, be it physicist or micro-observer, required the same skill, as the quote from Blackett demonstrates. However, one may insist that the micro-observer could identify the event *merely* as a novel phenomenon but not as a 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 micro-observer 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 micro-observers 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, this paper shows that the distinction marks the emergence of a double standard that distorted and minimised the micro-observers' work.

4. The instrumentalisation of scientific workers

The preceding historical analysis supports one of the central claims of the present paper: namely, that both human computers and micro-observers were *instrumentalised*, that is, they functioned as scientific instruments or extensions of scientific instruments. Scientific instruments, whether ordinary scientific instruments or revolutionary devices, can play substantial evidential roles and be deemed crucial for establishing particular knowledge claims. There is a sense in which both human computers and micro-observers were perceived in this evidential way. However, instruments do not give rise to theories or speculate about the data. It is this attribute

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of instruments that underpins the instrumentalisation of human computers and micro-observers.[6]

This paper contextualises four features of instrumentalisation: (i) *autonomy or self-determination*: human computers and micro-observers were denied autonomy or self-determination as they came to perform prescribed routinised tasks and were denied opportunities for other types of work; (ii) *fungibility*: both types of workers were fungible or interchangeable insofar as they were perceived as uniquely fit for the work, but no particular computer or micro-observer was regarded as irreplaceable; (iii) *denial of subjectivity*: they were denied subjectivity, manifested in the belief that specific types of workers, and specifically 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; and (iv) *denial of agency as knowledge producers*: the technician status, together with the feminised interpretation of their roles, deprived them of relevant agency and competencies – they were denied agency as knowledge producers. Both groups were perceived as lacking the hallmarks of a knowledge producer, such as insight, initiative, and originality; they 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 next three sections define three explanatory frameworks which, taken together, help us better understand the epistemic roles of micro-observers and human computers. The frameworks offer support to the instrumentalisation claim and help contextualise and explain how and why instrumentalisation occurs. This paper is primarily concerned with the epistemic, rather than the ethical, consequences of instrumentalisation. Thus, while the individual consequences of instrumentalisation are acknowledged as problematic injustices, the focus is on understanding how instrumentalisation impacts science beyond the individual. In particular, it will prove instructive, for the purposes of this paper, to separate the negative consequences of instrumentalisation from the consequences of credit (mis) allocation for at least two reasons: instrumentalised workers can, in principle, be given credit; and credit can be mis(allocated) without at least some of the consequences of instrumentalisation occurring. For instance, credit for a theory can be lacking, but the theory could gain uptake despite the individual injustice.

5. The institutional framework

The institutional framework is principally concerned with the socio-economic conditions that led to the development of a scientific labour market that both enhanced and restricted women's work in science in the United States (US) between the 1880s and the 1920s. Despite varying socio-economic and cultural disparities between the US during 1880–1920 and England in 1935–1955, the institutional framework can fruitfully be extended to the case of the Bristol micro-observers since both practices are similar in terms of the work undertaken by micro-observers and computers and the conditions that gave rise to these types of scientific employment.

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

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The practice of employing micro-observers 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 micro-observer 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 Nuclear Research Group, similarly, relied on its micro-observers 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 micro-observers or human computers even if they would often get stuck in such low-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).[7]

The 'proletarianisation' or deskilling phenomenon carries with it not only socio-economic implications, but also epistemic. The tedious and laborious work that micro-observers 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,[8] the skills involved in such work, and 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 routine tasks (Bergland 2008, xvi).

Feminised jobs not only referred 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 can be seen not only from the work of Rossiter (1982) on the history of women's work in science between 1880 and 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 sun-spots 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 labour market.

[7] Hicks (2018) has written at length about deskilling and feminisation of work.

[8] Male computers were unlikely to remain in such positions for long, leaving as soon as better opportunities arose.

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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'. 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 her 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. Despite their significant discoveries, they could not altogether transcend the prohibitive socio-hierarchical norms that devalued them as knowledge producers.

Daston and Galison (2010) offer a similar interpretation. 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 2010, 123); '[m]achines were [also] ignorant of theory and incapable of speculation' (123). Human computers, like machines, offered 'in their "emptiness" a transparency through which nature could speak' (341). In fact, 'women workers were

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presumed to offer a "natural" predilection away from the speculative tradition' (341), preexisting theoretical commitment, and interpretative temptation (Daston and Galison 2010).

Similar qualities were ascribed to the Bristol micro-observers, 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). Some of the anonymising language used to refer to micro-observers 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 helps us understand why micro-observers and human computers were devalued as knowledge producers. Yet they do not constitute a homogenous workforce and thus there are significant differences among human computers on the one hand, and between micro-observers and human computers on the other hand. First, not all human computers performed the same kind of work. Though, arguably, even in cases where the work was mechanical, epistemic gains associated with improvements in, for instance, mathematical methods can go unnoticed when the focus moves away from the workers.[9] Second, there are significant differences among 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 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). Third, while some human computers undertook work beyond routine tasks, most of the work performed by micro-observers was routine. Fourth, while at least some human computers enjoyed auctorial rights and recognition, micro-observers were briefly given quasi-auctorial rights only to have their names removed from official publications, according to Galison (1997). Thus, it is important to recognise that the instrumentalisation of micro-observers and human computers comes in different shapes and varieties, and for different reasons.

In the human computers' case, instrumentalisation was primarily the result of the prevailing socio-economic and cultural norms. The micro-observers' instrumentalisation cannot, however, be attributed solely to institutional factors. While 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-focused 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

[9] Thanks to James Weatherall for discussions on this point.

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epistemic authority commensurate with their work or findings. A better supported interpretation, which is the focus of the next section, is that the micro-observers were denied due credit and recognition not only because they were women, but also because of their functional roles within the economy of scientific work.

6. The functional framework

The functional framework, reconstructed from Shapin's (1995) historical work on Boyle's technicians, is concerned with the role of scientific workers within particular processes of scientific knowledge production. This paper argues that Shapin's remarks on the epistemic role of technicians and on their invisibility extend to micro-observers and human computers too. Furthermore, Shapin's framework helps us understand the reasons behind the minimisation of micro-observers' 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 focus on the 'epistemic role of support personnel' is geared towards (i) highlighting the historical invisibility of such workers, and (ii) demonstrating the 'collective nature of experimental knowledge'. 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 (Shapin 1995, 360). Micro-observers 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'. This is particularly true of the Bristol micro-observers, as information regarding their qualifications and education, and sometimes their full names, is difficult to retrieve due to lack of relevant 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 to 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' (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' (380). Technicians and other skilled labourers were fungible and interchangeable. Even technicians that 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

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distinguish phenomena from the idiosyncrasies of the experimental apparatus, their 'experience did not in itself confer epistemic value on technicians' understanding'. Whether or not a technician was given an identity and a voice was entirely at the disposal of the employer (Shapin 1995).

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 that, 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 micro-observers and human computers. In both cases, the precarity of their employment and their fungibility is evident. It is also evident, 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 micro-observers' 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 micro-observers, 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" ' (385).

7. The epistemic injustice framework

The epistemic injustice framework, originally developed by Fricker (2007), mapped the ethics of testimonial and hermeneutical interactions. Fricker argued that in either of these exchanges someone can be placed at a disadvantage because of an identity prejudice harboured against them. When someone is placed at a disadvantage in a testimonial exchange or in an epistemic exchange where they lack relevant interpretative resources through no fault of their own, Fricker argues that they have been the subject of an epistemic injustice. The subject of a testimonial injustice is 'wronged in their capacity as a knower', that is, they suffer a credibility deficit that places them at a disadvantage. Since Fricker's seminal work there has been a prolific expansion of the literature concerning epistemic injustice, focusing 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).

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 engaging in or contributing to cooperative inquiry, or more specifically being 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] 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

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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.

The case of human computers and micro-observers falls within 'participatory injustice', broadly construed. That is, they were not denied participation in science tout court, but they were obstructed from fully participating in science as inquirers and originators of scientific knowledge. This type of injustice is predicated on the instrumentalisation of scientific workers and results in harms not only to the scientific workers themselves, but also collective harms and harms to science itself.

In the epistemic injustice literature, it is common to distinguish between the primary and secondary harms of epistemic injustice. Fricker's original account identifies the 'primary' wrong of epistemic injustice with a form of epistemic objectification that 'involves the denial of someone's epistemic agency' (McGlynn 2021). The primary harms are distinctly epistemic and occur when one is 'degraded qua knower' Fricker (2007, 44), while the secondary harms need not be distinctly epistemic. In the case of knowledge production injustice, one is devalued as a knowledge producer and, ipso facto, as a knower, but, importantly, the secondary harms are also distinctly epistemic and have collective ramifications for the epistemology and methodology of science. For instance, one of the secondary consequences of knowledge production injustice is a curtailment of epistemic diversity via methods or theory devaluation. Lack of epistemic diversity has been shown to 'negatively affect the progress of epistemic communities' (Rubin and O'Connor 2018, 380). The consequences of knowledge production injustice are thus not only epistemic all the way down, but have a significant effect on the creation of scientific knowledge, and thus on the methodology and epistemology of science. In the case of micro-observers and human computers, both types of harms occur.

We have already argued in sections 4–6 that the instrumentalisation of micro-observers and human computers led to their diminished epistemic agency, especially in their capacity as knowledge producers. We have also shown in sections 2, 3, and 7 that in both cases their instrumentalisation had negative consequences on the individuals, such as lack of credit and recognition. These effects can be assessed as negative not only with the benefit of hindsight, but also from the viewpoint of the workers at the time, as seen in Maury's struggle for her auctorial rights.[10] In what follows, we combine the lessons from the three frameworks analysed above to examine the epistemic consequences of instrumentalised scientific labour for the methodology of science.

8. The epistemic consequences of instrumentalised scientific labour

This section argues that the instrumentalisation of micro-observers and human computers has negative effects not only for the individual scientific workers, but leads to particularly pernicious epistemic consequences for the methodology of science.

[10] Thanks to Dennis Lehmkuhl for discussions on this point.

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8.1. Stunted scientific progress

The first significant epistemic 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.

Instrumentalised micro-observers and computers had a close acquaintance with the data that presented them with the opportunity to discover more structure within the data, ranging from variations in the line spectra to new phenomena. At the same time, the instrumentalised micro-observers and computers faced the same predicament as Shapin's technicians, namely 'directness of experience did not in itself confer epistemic value on technician's understandings' (Shapin 1995, 381) and '[t]he plausibility of assistants' testimony might be set against credibility handicaps they might possess' (386). In other words, socio-institutional norms dictated, at the very least, a reluctant approach to any micro-observer's or computer's work that claimed to be of independent scientific merit. The presumption to theorise of the worker who operated beyond their station could deepen the 'credibility handicaps' dictated by the then extant socio-institutional norms (e.g., women computers/micro-observers should not speculate, infer, or theorise).

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 that 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.

8.2. Method devaluation

The second significant epistemic consequence of instrumentalising scientific labour for the methodology of science refers to situations in which the devaluation of human computers and micro-observers led to the discreditation of particular types of knowledge or methods. The case of Antonia Maury is once again telling. The instrumentalised workers were expected to work as per specific instructions. They were expected to analyse and reduce the data but not to interpret it. Antonia Maury was expected to classify the stars according to Fleming's criteria – according to the strength and width of the hydrogen line, in alphabetical order.

Maury, due to her scientific training and inquisitive mind, was able to distinguish further structure in the spectral lines and went on making inferences about the extra structure. In particular, she added flutedness and haziness of lines as relevant classificatory criteria. She designed methods for calibrating, standardising, and triangulating her findings, and she designed her own theories for interpreting them. However, the analysis of spectra was the work of computers. While such work was perceived as highly skilled, it was not associated with relevant epistemic authority qua interpreter or theoretician of the phenomena. The method of analysing and distinguishing structure in the spectral lines was perceived as routine and was rooted in the work of computers. Maury's work was specific to someone who was instrumentalised due to her status as a computer, her fungibility, and lack of control

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over her own work. Her 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.

8.3. Invisible biases

The third significant epistemic consequence of instrumentalising scientific labour for the methodology of science refers to cases in which biases of both micro-observers/human computers and knowledge validators are rendered invisible. In particular, if Daston and Galison (2010) are right, according to the dominant paradigm at the time, the instrumentalised scientific workers played an 'objectifying role'; the idea being that if even (professionally) untrained workers could identify particular events or empirically classify phenomena, certainly the phenomena were real/the classifications were robust. To ensure this objectifying role, some later employers of micro-observers, such as Walter Barkas, went as far as to not specify the goals of the research in the instructions to micro-observers in order to preserve their 'objectivity' towards the data. This was done in order to eliminate 'confirmation biases' that would tempt the micro-observers 'to obtain the answer that pleases' [sic] (Barkas 1964). Relatedly, the idea that scientific workers would 'mechanically' or 'mindlessly' analyse the data seems entirely inadequate, especially considering the fact that the workers (i) were in both cases analysed cognizant of the goals of the research; (ii) were involved in the analysis of data for prolonged periods of time; and (iii) had prior scientific exposure or training. The objectifying attitude thus hides different biases, this time on the side of the scientists, who sought to eliminate theory-ladenness not through epistemic means, but through socio-cultural and institutional biases. In particular, they believed that 'women's capacities' or 'technicians' skills' were such that they could not 'laden' the data with their inferential capacities as they were thought to lack such.

Another case in point is the way in which the artificial distinction between 'finding' and 'discovering' was introduced to delineate the 'mere' skill of the micro-observers 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 focused 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 that value observational/empirical novelty. The introduction of this distinction is particularly problematic, since the 'finding/discovering' distinction was not germane to the experimental process that led to the discovery of the pi meson (1947) and the tau (1949); it was the artefact of recollections and memoirs that post-dated contemporary appraisals of the contributions of micro-observers.[11]

[11] The distinction was based specifically on Tyndall's remarks with regard to the scanning process. Galison interpreted Tyndall's statements as a denigration of discovery and introduced the finding/discovering distinction to highlight the artificial credit economy created by privileging method

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8.4. Distortion of historical facts

Finally, the fourth significant epistemic consequence of instrumentalised scientific labour for the methodology of science refers to the distortion of historical facts. This paper has already shown that while the micro-observers played an invaluable role in the process of scientific knowledge production, their roles have not been adequately recognised in publicly disseminated documents. According to Galison (1997), their names have been erased from official documents such as the atlas published by Powell et al. (1959).[12] Such acts, in turn, gave rise to flawed narratives that distort our understanding of the process of scientific knowledge-making. When facts are skewed, incomplete, or highly selected to present a specific narrative, we do not gain an adequate understanding of how scientific knowledge is created; we do not gain adequate knowledge. We miss out on potentially valuable heuristics, discoveries, or opportunities to clarify and mitigate specific biases. For instance, when we talk about 'scanners', as opposed to 'observers' or 'micro-observers' as per the archival data, we imply a certain kind of worker and a certain kind of work: skilled or mechanical worker and routine work, as opposed to a knowing or knowledgeable worker and active exploratory work. Such issues are deeply epistemic in nature and can be detrimental to our understanding of science and scientific methodology if branded 'historical-corrective' or left unexamined. Mechanisms by which certain scientific contributors are minimised or actively removed from formal records result in scientific narratives that perpetuate simplistic myths about who produces scientific knowledge and how scientific knowledge is made.

9. Conclusion

This paper identifies, evaluates, and compares three frameworks for reconstituting and analysing the epistemic roles of human computers and micro-observers, their instrumentalisation, and consequences thereof. The institutional and functional framework helped us understand the conditions and motivations behind the human computers' and micro-observers' 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 micro-observers and human computers negatively impacts 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 micro-observers and human computers that is crucial in understanding the epistemic 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

stabilisation over the identification of novel observational results (Galison 1997, 200–201). In the context of this paper, the finding/discovering distinction is only a further example of the problematic epistemic consequences of unexamined biases. A full treatment of the historical adequacy of this distinction, or of its implications for accounts of scientific discovery and adjudications of credit is beyond the scope of the present paper.

[12] It is important to note that different kinds of records – official, formal, informal – can produce different kinds of scientific narratives, each of which may distort or simplify in distinct ways, with distinct consequences.

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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 that led to the under-appreciation of the epistemic roles of human computers and micro-observers, 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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