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Mediating Environments and Objects as Knowledge Infrastructure

Götz Hoeppe*
*Department of Anthropology, University of Waterloo, 200 University Avenue West, Waterloo, ON, N2L 3G1, Canada (Email: ghoeppe@uwaterloo.ca)

Abstract. The knowledge infrastructures of the sciences have been considered as human-made networks or ecologies of people, artifacts, and institutions that enable the production, calibration, storage, dissemination and re-use of data. Complementing these studies, this paper examines how scientists use the digitally mediated, shared availability of "natural" environments and objects for infrastructural purposes. Drawing on ethnography and informed by ethnomethodology, I focus on the uses of the sky in astronomical observation. Astronomical research is oriented to observing, and re-observing, sources on the sky, making it its topic. Yet, the sky is also an infrastructural resource, as it provides stable saliences that can be used alongside existing records for ordering work, diagnosing trouble with artifacts in data, and repairing data across diverse sites of practice. I consider a case in which such uses of the sky were new to researchers working in a novel domain, and one in which such uses were already established, but new to a student being inducted to its work. In both cases properties of the sky became salient through being mediated digitally. As existing records and new observations were made available to a single computational order, these data became accessible to what Melvin Pollner called mundane reason, wherein ceteris paribus clauses are used reflexively to maintain a world in common. Although the sky may appear to be an extreme case, I argue that other mediated environments and objects, and the reflexive practices through which these are engaged, have similar infrastructural uses in other disciplines.

Keywords: Knowledge infrastructure, Calculative practices, Digital data, Repair, Ethnomethodology, Mundane reason, Reflexivity, Boundary objects, Astronomy

1. Introduction

Setting out from studies of large technological systems (Hughes 1983), much recent work in Computer Supported Cooperative Work (CSCW) and Science and Technology Studies (STS) has considered the constitution, history and impact of infrastructures, with particular interest in knowledge infrastructures for scientific and technical work (e.g. Bowker 1994; Star and Ruhleder 1996; Bowker and Star 1999; Star and Bowker 2006; Edwards 2006, 2010; Karasti et al. 2010, 2016; Ribes and Lee 2010; Edwards et al. 2013; Jirotka et al. 2013; Mayernik et al. 2013; Monteiro et al. 2013; Borgman 2015; Parmiggiani et al. 2015). Paul Edwards defines knowledge infrastructures as "robust networks of people, artifacts, and institutions that generate, share, and maintain specific knowledge about the human and natural worlds" (Edwards 2010, p. 17). In his important historical study of research on climate change, Edwards demonstrates that this phenomenon becomes recognizable to scientists through the stable background that the knowledge infrastructures of climate science provide. These include networks for standardizing and calibrating instruments as well as for measuring, distributing, sharing and repairing meteorological data. Focusing on data use and re-use in the sciences more broadly, Christine Borgman (2015, p. 4) writes that data "exist within knowledge infrastructure – an ecology of people, practices, technologies, institutions, material objects, and relationships. All parts of the infrastructure are in flux with shifts in stakeholders, technologies, policies, and power." In the studies of Edwards (2010), Mayernik et al. (2013) and Borgman (2015), the elements of knowledge infrastructure are deemed to be distinct from the objects of inquiry.

In this paper, I propose a complementary perspective on knowledge infrastructures in the sciences by inquiring into how scientists use the digitally mediated, shared availability of "natural"[1] environments and objects for the diagnosis of troubles, the repair of an observing system and inventive re-uses of data – infrastructural uses in Edwards' (2010) sense. Focusing on astronomy, I contend that it is useful to consider the sky not only as a topic of astronomical work, but also as a resource for its conduct beyond the accounts of their work that astronomers themselves typically give.[2] The distinction of topic and resource is inevitably contingent on the projects that scientists pursue.[3] One may feel tempted to say that, as representations, some digital scientific data – for example, a satellite image of a desert or an image of a rock, or, perhaps more plausibly, models built from such images, − are themselves a "mediated environment" or a "mediated object." However, this leaves unspecified how scientists recognize and use them as such in methodical ways. I suggest that these practices are best revealed by ethnographically witnessing scientific work in its orientation toward producing and assessing specific results.

Some recent studies in CSCW and STS have focused on standards and ontologies – "formal, explicit specification of shared conceptualizations" (Guarino et al. 2009) –

[1] Here I use the adjective "natural" as a placeholder that inappropriately includes *Drosophila* fruit flies, laboratory mice and other animals and plants whose genetic material has been significantly transformed through human action.

[2] Given its ethnomethodological orientation, my concern in this text is to specify what scientists do to accomplish the diagnosis and repair of data as methodical and ordered actions. This work is infrastructural in Edwards' (2010) sense. In both of my empirical cases (described in sections 3 and 4) the mediated sky, in the form of catalogues of stars or maps of the microwave background, is important, but – critically – it is engaged through practices that dwell on assumptions about the order of the environment (sky) and its objects (stars and their spectral energy distributions, for example). I prefer referring to the mediated sky as an "infrastructural resource," seeking not to lose these practices in my account and to avoid promoting ready-made and fixed things as knowledge infrastructures without the practices through which these are engaged. Borgman (2015, p. 4) perceptively includes "practices" in her account of knowledge infrastructure. While I could presumably replace "infrastructural resources" by knowledge infrastructure, this would gloss over my point of describing and situating the scientists' practices.

[3] See section 5.1. for a discussion on how the ethnomethodological distinction of topic and resource pertains to the study of knowledge infrastructures.

as key elements of knowledge infrastructures (Borgman 2015) and scientific databases (Leonelli 2016). Edwards et al. (2013, p. 8) note the tension between the "desire for universality and the need for change" as the available knowledge and the modes of ordering it change. Informed by Kuhn's (1962) *Structure of Scientific Revolutions*, they insist that the objects of a science are defined in respect to specific dominant paradigms, which, following Kuhn, they understand to be mutually incommensurable. Data ontologies would then be bound to specific paradigms as well. Since Kuhnian paradigms are subject to being revised, "object-oriented" ontologies could seem to be unstable, and therefore not be viable in the long run. Edwards et al. (2013, p. 9–10) argue that this problem is aggravated by working across disciplinary boundaries, since either discipline's paradigms are subject to revision and transformation. In this paper, I adopt a post-Kuhnian position and argue that many scientists routinely solve these problems by using the resources that they have to organize and order their affairs (cf. Liberman 2013). In many scientific disciplines these resources include the objects of investigation and the practices by which these are engaged.

That the objects of scientific research can be used for work that is infrastructural in broad terms has been argued before. Thus, biological materials (Clarke and Fujimura 1992), *Drosophila* fruit flies (Kohler 1994) and laboratory mice (Rader 2004) became not only topics of research, but also resources for its conduct, such as by providing (and embodying) standards for comparison. A variety of West African insects became part of an infrastructure for ecotoxicological assessments by exposing them to different locales and serving as markers of levels of pollution (Tousignant 2013). Studies of the uses of these organisms adhere to the notion that infrastructures are human-made and have network structures (of exchange, for example). A case where "nature itself" has been deemed an infrastructure, although not for scientific work, is the Panama Canal watershed, a landscape developed to support appropriate water levels in the canal (Carse 2012). While this is another example of an infrastructure managed by humans, it is possible to conceive of the environment in an even broader, or deeper, sense as being infrastructural. John Durham Peters (2015) argues in an intriguing philosophical meditation for considering water, air, fire, and sky as infrastructural media. Following Edwards (2002), Peters considers nature to be the ultimate, and original, infrastructure, remarking poetically that "ontology, whatever else it is, is usually just forgotten infrastructure" (Peters 2015, p. 38). Here, then, we are back to ontologies.

A prominent illustration of Peters's view is the night sky with its constellations, which provide navigators in diverse cultures with what Edwin Hutchins (1999) calls "cognitive artifacts." Concluding a discussion of the techniques of Micronesian navigators in the Pacific, Hutchins writes: "There is a continuum from the case in which a cognitive artifact is used as designed, to cases of cognitive uses of artifacts that were made for other purposes, to completely opportunistic uses of natural structure" (Hutchins 1999, p. 127; see also Hutchins 2005, p. 1574). Read in the context of studies of knowledge infrastructure, Peters and Hutchins arguably invite us to consider the diverse novel and imaginative uses of scientific data that (mediated) environments and objects afford.

Focusing on the practical action and reasoning of scientists, my aim is to present an ethnomethodologically informed ethnography toward this end. I aim to show how astronomers make their work account-able (with the hyphen), that is, "reportable-and-witnesseable" (Garfinkel 1967, p. 1), to fellow practitioners. This work is typically reflexive, that is, descriptions of a setting become a part of it (Lynch 1993, p. 17). What is said, done or seen reflects back on what has previously been said, done or seen and can show it in a new light (see also Garfinkel 2002; Rawls 2008; Liberman 2013). Thus conceived, reflexivity is invariably linked to temporal sequences of action.[4] Ethnomethodology is respected and established in studies of CSCW (e.g. Crabtree 2003; Randall et al. 2007; Button et al. 2015) and it has already inspired some work on knowledge infrastructures in scientific practice (Randall et al. 2011). By attending to the lived, sequential, and reflexive work of scientists I focus on what studies of knowledge infrastructures drawing on historical material (Edwards 2010) as well as on ethnography and interviews analyzed with Grounded Theory (Borgman et al. 2012; Mayernik et al. 2013) could not attend to.

Inasmuch as ethnomethodological reflexivity matters to practices of repair in social interaction, such as in conversation, its study resonates with work that considers "infrastructure" as a verb (Star and Bowker 2006). This work attends to the foregrounding of otherwise backgrounded elements of technological work practices in what Bowker (1994) called "infrastructural inversion" (see also Sims 2007; Edwards 2010; Karasti et al. 2010; Mayernik et al. 2013; Tousignant 2013; Parmiggiani et al. 2015). As Edwards (2010) points out with respect to research on the Earth's climate, scientists themselves commonly do this work, turning the climate record upside down and re-examining its elements down to individual measurements. Such ongoing work of repairing data or elements of infrastructure is not the exception but the rule (Graham and Thrift 2007; Schaffer 2011). In order to account for the ordinariness of repair, Henke (2000) and Sims and Henke (2012) turn to ethnomethodology and conversation analysis for the insight that repair is critical to any social interaction and ordinary talk, including turn-taking (Schegloff 2006). This repair is inevitably sequential and embedded in streams of activity, yet often contingent on diagnostic work (cf. Büscher et al. 2009).

This paper is based on 18 months of ethnography of a research group in Heidelberg (Germany) and at two observatories.[5] My aim was to witness everyday work with digital data and to learn how problems were recognized and resolved in the process of producing a scientific result. I focused on settings where graduate students were instructed by senior staff members, but also succeeded in resolving problems

[4] For the contrast between the notion of reflexivity employed in ethnomethodological studies and that common to other approaches in the social sciences, see Kim (1999) and Lynch (1993, 2000).

[5] I conducted my fieldwork in 2007–10, followed by re-visits in 2010–17.

among themselves (Hoeppe 2014). A major fraction of astronomical data is analyzed in the course of instructing graduate students (as presentations at conferences and a stroll through a research institute illustrate), and I believe that what I witnessed is typical for work in the discipline more generally. I also attended conferences, seminar talks and collaboration meetings. I supplement this material by a video recording of an academic talk as available on the Internet (section 3). Garfinkel and Wieder (1992) called for ethnomethodological analysts to acquire "unique adequacy", that is, at least a "vulgar competence" in the work studied. Although I have not participated in the work that I describe in this paper, I had acquired practice in analyzing digital astronomical data 15 years prior to my study by completing a MSc degree in astrophysics.

In section 2, I will begin with discussing a moment in the work of the Heidelberg research group in which the infrastructural uses of the sky became remarkable to these astronomers themselves. I turn next to the role of digital mediation for astronomical work and the uses of mundane reason. In section 3 I discuss a case of diagnosis in which "having-a-sky-to-work-with-together" is not something that astronomers working in a novel domain of observation are used to. I use this case to illustrate how digital mediation makes the objectual sky, digital data, and existing astronomical records accessible to mundane reason. In section 4, I return to the Heidelberg group and present a case of a self-initiated repair of their observing system in which they use the existing record of astronomical data along with new observations. In the Discussion I will consider how one could generalize from these examples and suggest how their interpretation may benefit studying other domains of digital data use.

2. Mediated environments and mundane reason

2.1. The immutability of the sky as a backgrounded assumption

In November 2007, I sat in on the weekly meeting of a research group of astronomers in Heidelberg, Germany. Its members made observations of distant galaxies and clusters of galaxies for the purpose of studying their evolution. Some of their observations had been scheduled at a telescope in Spain over the past 3 years, but due to recurrent bad weather only a small fraction of the data required for the project had been gathered. This was much to the frustration of Nancy, a graduate student whose PhD work critically depended on the data. Having received an email from the observatory earlier in the day, Ken, a senior researcher and Nancy's supervisor, was to report on the status of new observations of two galaxy clusters, called A226 and A901.[6] Besides Ken, Jim and Owen (two other senior astronomers), several PhD students (including Nancy), and post-doctoral scholars were present.

[6] These acronyms stand for entries in the Abell catalogue of galaxy clusters (Abell 1958).

The following exchange occurred at the beginning of the meeting. I transcribe my recording of it using elements of Gail Jefferson's (2004) transcription scheme. In this scheme, underscoring indicates emphasis, parenthesized "h" – such as (hhh) – signifies an outbreath characteristic of chuckling or giggling, and degree signs bracketing an utterance – such as °these two° – indicate that it is spoken more softly than the surrounding talk. In the following, (HA-HA-HA-HA) represents loud laughter. "MANOS Deep" and "COMBO-17 + 4" are acronyms of the group's observing projects. All personal names are pseudonyms.

Transcript 1

1 Ken: Okay … so ehm … maybe it would be good to give a brief … eh … account where we … where we stand at the moment with MANOS Deep or COMBO-17+4 ... I want to mention before that … from my side we have … new observations in A226 … ehh … we had new observations in A226 already a week ago …. or a bit more than a week ago .... and tonight (hhhh) the first observations of A901 for more than a year (hhhhh) have happened (hhhhh)

2 Owen: It is still … it is still there?

3 Ken: (hhh) ((*chuckles*))

4 (hhhh HA-HA-HA-HA-HA-HA-HA-HA) *((collective laughter))*
(3 seconds)

5 Ken: So … it seems that we make some progress … I am pretty (sure) …

6 *((collective laughter continues))*

7 Jim: °It has drifted away°
(0.5 seconds)

8 *((laughter ends abruptly as Ken continues to talk))*

9 Ken: I … I think we have a good chance to get … a very good … to decent data base for A226 in this year … we have already collected quite a bit

Arguably, Owen's question (in line 2) of whether the galaxy cluster A901 was "still there" was rhetorical. Astronomers do not expect a galaxy cluster to disappear or to "drift away" in the sky, as Jim jokes (in line 7), certainly not after a year or so of bad weather. However, despite this being implausible, Owen's question did elicit collective laughter (line 4). Ken can be heard as having invited this laughter as his chuckling (toward the end of line 1) opened a slot for Owen to position his question (cf. Jefferson 1984). Yet the laughter that followed was contained. It ended abruptly (line 8) and Ken went on to give his account on the current state of observations for the project (line 9). One may hear him calling for the group to continue going on with business as usual.

Members of the group may have heard Owen's interjection as a quip on their notorious bad luck with the weather. Yet they may have also heard it as suspending the backgrounded assumption of the stability of the night sky – commonplace for astronomers since Antiquity (Aristotle 1939; Evans 1998; Trimble 2000). If galaxies and galaxy clusters were to "drift away," the sky, and astronomical work practice, would literally be "out of order." Anthropologist Mary Douglas writes that a joke "affords the opportunity for realizing that an accepted pattern has no necessity" (Douglas 1975, p. 96). The laughter that a joke elicits illuminates a social world held in common with others (Critchley 2002, p. 86). Participating in the laughter may affirm one's membership in it (Coleman 2012, p. 104). People hold worlds in common with others through sharing classifications and methods of sense-making and ordering. Thus, Harold Garfinkel begins his *Studies in Ethnomethodology* by insisting that "in doing sociology, lay and professional, every reference to the 'real world,' even where the reference is to physical or biological events, is a reference to the organized activities of everyday life" (Garfinkel 1967, p. vii). If this is the case, Owen's quip suggests that there ought to be shared practices of achieving reference which themselves are generally unproblematic to these researchers. The sudden end of laughter (in line 8) may well mark that these researchers cannot afford to be skeptics. Their work inhabits a space of non-skepticism.[7]

Several infrastructures are routinely used by observational astronomers, including the members of the Heidelberg group. Their work depends on standards of time-keeping, coordinate systems, units of measurement, and the calibration of equipment, as Frederick Chromey (2010) and John Hearnshaw (1996) describe, as well as on standard formats and protocols for transporting and sharing data, as Robert Hanisch et al. (2001) discuss. Yet neither of these astronomers, nor Christine Borgman (2015) in her useful account of astronomy's knowledge infrastructures, includes the sky itself as an infrastructure. Owen's quip draws attention to what these astronomers usually rely on (Graham and Thrift 2007). It affords them with an imagined instance of infrastructural inversion.

2.2. Mediation as a condition for astronomical work

Unlike, say, Pacific navigators who use the sky by night (Hutchins 1999, 2005), the research astronomers whose work I witnessed are usually removed from the phenomenal night sky, and rarely look at it directly. They work in the daytime, using computers and digital data. Cosmic radiation becomes available and perceptible to astronomers only through affecting intervening media – malleable "substrates," including air, photographic emulsions, or digital detectors. Yet, as astronomical work unfolds, numbers, gestures and language may be considered its media as well (Heider 1959; Krämer 2015; Luhmann 2012). In this way, media often appear to

[7] I thank Douglas Macbeth for this formulation.

be transparent – one sees a flower through the medium of air, one hears a story through the medium of language (cf. Krämer 2015, p. 31), except when troubles (say, in seeing or hearing) draw attention to media and their constitution. This is a characteristic that infrastructures and media share. It is not by chance that Peters (2015) writes about "infrastructural media." As astronomer Pierre Léna defines it, the "astronomical sky is a two-dimensional distribution of intensity of electromagnetic radiation" (Léna 1989, p. 245).[8] But it only becomes a two-dimensional distribution when thus represented using paper, photographs or digital technologies.

Taking a photographic image of the sky would usually be called an act of representation (Coopmans et al. 2014), so why do I prefer to focus on mediation instead, including in the title of this paper? The difference between "representation" and "mediation" is not critical for my argument, but in preferring to highlight the latter I am informed by philosopher Sybille Krämer (2015, pp. 34–35) who reflects on the difference between signs and media. She argues that a sign itself must be perceptible while its meaning, that which the sign "refers to," is invariably absent and perhaps invisible or immaterial. Much the same can be said about a representation. In contrast, attention to a medium emphasizes its "sensibility, materiality, and corporeality" (ibid., p. 35), and ethnographers can witness the practices involving these more easily than inferring how the referent of a sign is understood. Krämer insists that "there are not simply signs and in addition also media" (ibid., p. 34), but that making this distinction means to choose one of two perspectives.

Making representations is central to what scientists do, but in doing so they cannot but attend to their specific properties of media, for example, that digital images are "arrays of numbers" – two dimensional structures of non-overlapping, overwriteable square picture elements (pixels) – which can be added, subtracted or divided pixel-by-pixel, stored and retrieved. These specific properties of a medium become salient in the course of its use.[9] That the members of the Heidelberg research group can "collect" digital photographic exposures of the galaxy clusters A226 and A901 over several observing seasons, as Ken reports above (Transcript 1, line 9), and add them to increase the sensitivity and reduce the noise is just one example. As it encompasses all observers on Earth I consider the sky an environment; since it is only available to astronomical work through media I shall call it a mediated environment.

2.3. Mundane reason in the collaborative use of digital data

Astronomers are in a position reminiscent of that of chemist-philosopher Michael Polanyi, who observed: "In my laboratory I find the laws of nature formally

[8] For researchers studying the cosmic microwave background the sky is a two-dimensional distribution not only of intensity but also of polarization.

[9] Goodwin (2018) describes a girl's hopscotch game and a Munsell color chart as two kinds of graphic fields on which co-operative action is routinely built.

contradicted at every hour, but I explain this away by the assumption of experimental error" (Polanyi 1964, p. 31). Arguably, at the end of Polanyi's days in his laboratory, the laws of nature were always again back in place. He appears to consider the laws of nature as incorrigible, at least for his ordinary lab work, and in doing so he relies on what Melvin Pollner (1974, 1987) later came to call mundane reason. It is because of their mutual orientation to the assumption of an "incorrigibly objective and commonly shared world" (Pollner 1974, p. 53), that members of a practice are able to recognize and resolve disjunctive experiences using shared practices of sense-making, presuming that "reality is coherent, determinate and intersubjectively accessible" (Pollner 1987, p. 47). Mundane reasoners commonly rely on ceteris paribus clauses: the (often tacit) assumption that a law only holds if "other things are equal." It provides members with a resource to account for disjunctive experiences by attributing them to "things not having been equal" (cf. Cartwright 1983; Earman et al. 2002).[10] Embedded in members' reasoning, such clauses are resources for reflexively preserving their own validity as incorrigible propositions. Pollner claims that "mundane reason is not an empirical version of reality but an *a priori* specification of its features in terms of which empirical claims are reviewed for their adequacy" (Pollner 1987, p. 18).

Stable features of the material world can be resources for mundane reason. Pollner (1987, pp. 40–45) draws on Merleau-Ponty's (1968, p. 15) notion of the world as the "Great Object", whose presumed incorrigibility is a resource for resolving reality disjunctures. Kenneth Liberman (2013) demonstrates in intriguing case studies of navigating with sketched maps and playing board games that people use various embodied, material and representational means to organize and order their affairs, often utilizing features of a setting "opportunistically" (cf. Hutchins 1999).[11] Also focusing on board games, Livingston (2008, p. 140) argues that the stability of the practices of playing checkers lies in the materiality of its culture, and he insists that different materialities encountered in, say, laboratory chemistry or mathematical theorem proving, implicate different, specific forms of reasoning (Livingston 2006, p. 424). Drawing on studies of girls' hopscotch play and archaeologists' uses of a Munsell color chart, Goodwin (2018) comes to a similar conclusion. CSCW studies have likewise addressed the shared access of organized worlds of artefacts and objects, natural and artificial, which people can engage in sequences of work (Suchman 1987; Moran and Anderson 1990; Robertson 2002).

It is specific to much contemporary scientific work that objects and environments are available mostly in digitally mediated form. If digital pixel images are constitutively "arrays of numbers" (Lynch 1991) that astronomers work with computationally then the images' calculative properties must matter to mundane reason. Douglas

[10] See http://plato.stanford.edu/entries/ceteris-paribus/. Accessed 30 May 2016.

[11] Following phenomenologist Aron Gurwitsch (1964), Garfinkel (2002, p. 246) refers to this self-emergence as being "autochthonous."

Gasking (1955) takes the use of ceteris paribus clauses into the medium of numbers and addresses practices of counting, an elementary form of measurement in science (see also Warwick 1995; Martin and Lynch 2009). As a student of Ludwig Wittgenstein, Gasking is concerned with the relation of mathematics to the world. As it pertains to this paper, the gist of Gasking's argument is to make mundane reason available for discussing calculation, measurement, and the uses of digital data. Performing and interpreting a calculation like "7 + 5 = 12" is not troublesome for most quotidian uses, Gasking argues, but if one decides to experimentally align such a calculation with real worldly materials one may be challenged. For example, if one tries to add 7 drops of mercury to 5 drops of mercury in a bowl, one may count less than 12 drops of mercury in the end. Two or more of the drops may have merged while counting was in progress. Likewise, making calculations with observed data coherent may force one to invoke ceteris paribus clauses (Cartwright 1983; Warwick 1995), as the following two cases illustrate.

3. The mediated sky as a resource for diagnostic work

Sharing communal access to a stable object or environment is not something that all astronomers are used to, as the following episode illustrates. It is a case of diagnosis and critique in which one astronomer argues that his peers have not made proper use of mundane reasoning about the sky in their work.

In late March 2014, at a press conference at Harvard University, scientists of BICEP, an international research collaboration, announced a spectacular discovery about the early universe.[12] For 3 years, they had operated a small telescope (BICEP2) and a detector array (the *Keck Array*) at the geographic South Pole to make sensitive measurements of the cosmic microwave radiation of the southern sky, and prepare maps of its intensity and polarization. The polarization map had a greater sensitivity than any other such map made before. It revealed a swirl pattern that BICEP team members attributed to so-called B mode polarization, finding it to agree with theoretical predictions from models of inflation, a theorized brief moment of rapid expansion in the early history of the universe. Interpreted as observational evidence for inflation, this was a spectacular claim. However, despite the team's good reputation for its earlier work and their members' institutional affiliation with renowned universities (including Harvard University, Stanford University, the California Institute of Technology, and the University of Toronto), this interpretation soon attracted the skeptical attention of many experts in cosmology, who discussed it at meetings and in social media such as blogs, Facebook and on Twitter.[13]

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[12] The acronym BICEP stands for Background Imaging of Cosmic Extragalactic Polarization, see http://bicepkeck.org. Accessed 20 May 2016.

[13] Examples are: http://www.math.columbia.edu/~woit/wordpress/?p=6865, http://resonaances.blogspot.ca/2014/05/follow-up-on-bicep.html, and https://telescoper.wordpress.com/2014/05/14/that-bicep-rumour/. Accessed 25 May 2016.

3.1. Expectations of sameness

Ten days after the Harvard press conference, Princeton University astrophysicist David Spergel gave a physics colloquium at New York University in which he was to talk about his own studies of the cosmic microwave background.[14] However, given the current interest in the BICEP announcement, much of his talk was to be a commentary on his first inspection of their work. Spergel is a member of the *Atacama Cosmology Telescope* (ACT) collaboration, a competitor of the BICEP team. He introduced his talk with reiterating a scene from the movie *Bill Durham*[15] in which a baseball catcher teaches a young player useful phrases to know. A phrase recommended when talking about an exciting new scientific result would be, Spergel adds: "Important, if true." Evoking the audience's laughter, Spergel kept this critical tone when discussing the BICEP analysis.

Early in his talk, Spergel showed a slide of two greyscale pixel maps of the fluctuation pattern of the microwave background of a patch in the sky. One was based on measurements taken with the ACT in Chile using a very sensitive semi-conducting detector (a so-called Transition Edge Sensor, TES), the other made using a different detector design (a bolometer) onboard the European Space Agency's *Planck* satellite (Figure 1). Using a laser pointer to highlight similarities in the greyscale patterns, Spergel explains:

> *Transcript 2*
> These are completely different experimental set-ups … and you see the <u>same</u> <u>thing</u> … and this is true with a host of experiments … One of the things I want you to take away from this is the remarkable <u>agreement</u> we have between independent experiments at this point … making these measurements. (…) So if you actually look at the same part of the sky the agreement here is really remarkably good.[16]

Mapping the cosmic microwave background is a relatively new and small field in which members often speak of their work as "doing experiments" that are not used to seeing "the same thing."[17] Commenting on these different maps and their similarities in his talk, Spergel never bothered to consider the positional agreement of the patterns in the two greyscale maps that he presented on his slide. As he confidently talked about "the same part of the sky" to his non-expert audience, Spergel did not topicalize whether

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[14] David Spergel, "Cosmology after Planck", lecture at New York University (NYU), 27 March 2014, https://www.youtube.com/watch?v=j3fHkQa6818. Accessed 6 June 2018.

[15] See: http://www.imdb.com/title/tt0094812/. Accessed 7 June 2017.

[16] David Spergel, lecture at NYU, 27 March 2014, see endnote 14, ca. minute 6 and 10.

[17] For example, the fields observed in the sky by BICEP2 and the *Atacama Cosmology Telescope* (ACT) do not overlap.

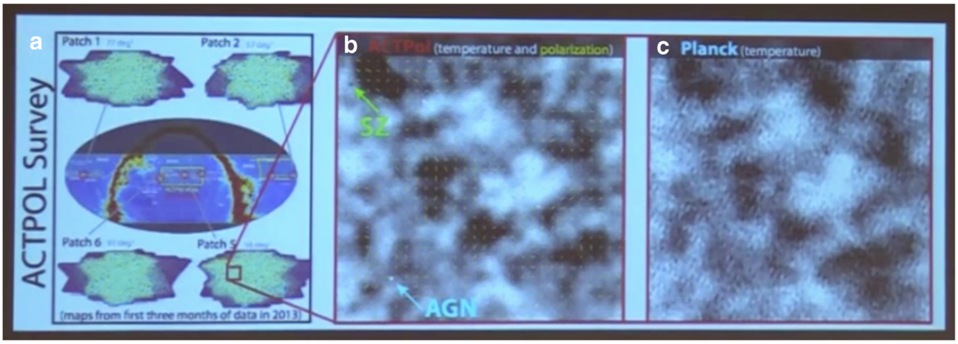

Figure 1. Presentation slide from David Spergel's lecture at New York University (27 March 2014), showing two greyscale pixel maps of the fluctuation pattern of the microwave background of a patch in the sky. One is based on measurements taken with the Atacama Cosmology Telescope (ACT) in Chile using a TES, a very sensitive quantum detector (center). The other map was made using a different detector design (a bolometer) onboard the European Space Agency's *Planck* satellite (right). The diagram on the left shows where in the sky these patches are (taken from https://www.youtube.com/watch?v=j3fHkQa6818; accessed June 6, 2018).

the coordinate positions of the features on the map agreed with one another. The maps he showed lacked labels indicating the celestial coordinates of the fields. Spergel thus apparently deems their positional agreement to be unproblematic. Of interest to him is the shape of the intensity pattern. It is not surprising that scientists check observations produced with one "instrumental complex" with those obtained using independent instrumentation. Spergel's trust appears to emerge from *comparing* these independently made maps and finding them to agree. This trust does not seem to reside in either one of these maps alone.

3.2. Raising doubt with mundane reason

The BICEP map, of greater sensitivity than previous measurements of cosmic microwave background polarization, could not be compared meaningfully with existing data, and so Spergel turned to internal comparisons of the data set and statistical tests as provided by the BICEP team in its discovery paper (Table I in Ade et al. 2014). These tests were calculated using pixel maps of data generated from scanning the sky with the BICEP telescope. A particularly notorious source of trouble for sensitive astronomical measurements is scattered light that has reached the detector and causes artifacts in the data. Such artifacts are usually specific to the orientation of the telescope during the measurements. To recognize them, the BICEP team had designed its telescope to be movable around three axes, allowing it to observe a given position in the sky (defined by its celestial coordinates) in four distinct orientations. The four resulting sub-sets of data could then be scrutinized for their consistency. The BICEP team had used the so-called jackknife resampling technique[18] and published its test statistic, presumably with the intention to demonstrate the reliability of its findings and the consistency of its sub-samples. Spergel, however, read these statistics as hinting at problems in the BICEP analysis:

Transcript 3
You can look at the sky … same part of the sky … at four different orientations and make four independent maps … and ask … Do I see the same sky at four different orientations? And that doesn't test all systematics … but if there was something going on where scattered light was getting in you might expect to see something different. Well … what's a little worrying is if they do that … *((points with laser pointer to a projected slide of Table I of Ade* et al. 2014*))* here is their EE signal at four different orientations [of the telescope] … the probability of finding that much difference between the two is at the 0.4 per cent level.[19]

Attending to statistical confidence levels, Spergel reads this test as pointing to a large probability that the observations in the four configurations were inconsistent and did not show the "same sky." The BICEP team had allegedly missed this.

Given how important comparisons of pixel maps are for Spergel as a fellow investigator of the cosmic microwave background, it is not surprising that cross-correlation, a mathematical technique suited for assessing the similarity of series of numbers, is among the tests performed by the BICEP team that Spergel pays particular attention to.[20] Thus, he next turns to the cross-correlations (referred to as "cross" in the following transcript) that the BICEP team had computed using the maps made with BICEP2 and the Keck detector array:

Transcript 4
The amplitude of the B mean modes here ((*points at a diagram on a slide*)) are much higher than the theoretical prediction. And this is supposed to be due to [gravitational] lensing. Now some people say … Oh don't worry, the Keck array … the numbers get better. … It's not really fair to play that game. I think it is better to ask … Look at the consistency test and say … they are looking at the same part of the sky. Why does the point … why … if I take Keck minus BICEP2 … which should have no signal … cross BICEP2 … I see shifts … of more than two sigma on most points … which suggests they are not seeing a consistent sky between the two experiments.[21]

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[18] The jackknife resampling technique is a method that is commonly used to estimate the variance and bias of samples (Tukey 1959).

[19] David Spergel, lecture at NYU, 27 March 2014, see endnote 14, ca. minute 49.

[20] Cross-correlations have been used also to calibrate observations of the cosmic microwave background, including data from the Atacama Cosmology Telescope (ACT; Hajian et al. 2011).

Interpreting differences of "more than two sigma" (standard deviations) as "not the same," Spergel concludes that the BICEP team has not used the sky as an organizational resource to recognize its own measurement uncertainties or, worse, its inconsistencies. The difference between measurements taken by the same team with its apparatus appears to be in stark contrast to the agreement between measurements taken by the presumably independent ACT and *Planck* telescopes that Spergel dwelt on at the beginning of his talk.

One ought to notice that the statistical tests that Spergel dwelt on did leave room for interpretation. They did not themselves invalidate the BICEP team's claim to the discovery of inflationary B modes at this point in time. Perhaps its members considered differences of two sigma as "same enough." One may have argued as to how much agreement between distinct measurements is required for them to be "good enough" to let the discovery claim stand. Nevertheless, Spergel's critique did mark the beginning of growing doubts about the BICEP interpretation. Eventually, the BICEP team retracted its claim to the discovery of inflationary B modes after a consensus had stabilized in the community that the entire measured polarization signal could have been due to foreground dust in the Milky Way galaxy.[22]

That Spergel's demand for the BICEP sub-samples to exhibit the "same sky" was worth pointing out to fellow scientists is suggested by New York University astronomer David W. Hogg, who attended Spergel's talk and wrote about it in his blog:

> One amusing thing about Spergel's talk was the repeated point (obvious, but often overlooked) that because all CMB [cosmic microwave background] experiments are observing the *same, single sky*, they ought to agree to better than one-sigma, especially on large scales where cosmic variance dominates.[23] (emphasis in original)

3.3. Discussion of this case

Working in a domain that is removed from any human's senses, David Spergel is persistently attentive to the sky and its uses as a diagnostic tool. The sky became available to Spergel's critique only through digital data, pixelized and amenable to calculative uses.[24] Thus, he could compare pixel greyscale maps of the *Atacama Cosmology Telescope* (ACT) in Chile with those of the *Planck* satellite and consider a table of statistical tests. Spergel points out (in Transcript 2) that ACT and Planck are

[22] Spergel is a co-author of the most widely cited critique of the BICEP2 interpretation (Flauger et al. 2014).

[23] http://hoggresearch.blogspot.ca/2014/03/spergel-and-bicep2.html. Accessed 19 May 2016.

[24] Of course, even prior to the introduction of digital data, observations were mediated. For example, the work of the astronomers who discovered the optical pulsar (Garfinkel et al. 1981) was mediated by a telescope and oscilloscope electronics.

instrumental set-ups that "see" the "same sky" even though they draw on independent physical processes. In doing so he is attentive to what philosophers of science have called robustness reasoning. Robustness here refers to the convergence of "multiple means of determination" (Wimsatt 2012 [1981], p. 61) toward one result. Consider, for example, Ian Hacking's (1983, pp. 186–203) account of what trust in seeing, and recognizing, the "same" thing through different kinds of (optical, acoustical, scanning) microscopes entails. This trust emerges not only from understanding the function of these instruments as relying on different established physical principles, but also on the implausibility that observations with technologies that rely on distinct principles should converge spuriously (see also Chang 2004; Wylie 2017).[25]

What is at issue for Spergel, and echoed by Hogg, is what "the same" could mean for the BICEP analysis. Spergel clearly disagrees with the BICEP team's claim of its sub-datasets showing the same sky. In positing that the sub-datasets ought to be "the same" within the margins of significance of statistical tests he orients himself to the assumption of an "incorrigibly objective and commonly shared world" (Pollner 1974, p. 53). Spergel could have found flaws in other data that he considered in his talk (and indeed, he also criticized data reductions of the *Planck* satellite team). What provoked his scrutiny of the BICEP results was the importance of the claimed discovery and its statistical significance. The resources he used were available to all astronomers: published statistical test results and shared assumptions about the sky.

4. Using the mediated sky as a resource for the repair of data

While Spergel's aim was to critically assess the work of his peers in respect to their claim to a significant discovery, I shall now consider a case in which researchers used mundane reason and public datasets to repair their observing system and make new observations consistent with already existing ones. This is infrastructural work in Edwards's (2010) terms. I consider it by returning to the Heidelberg research group that I introduced in section 2.1.

For over 2 years, I witnessed how Nancy, the PhD student, was instructed to add images, calibrate them, use algorithms for finding objects in the exposures, measure their position and brightness, and classify them by automatically matching observations to spectral template models to estimate the redshifts (a measure for the distance of galaxies) and their physical parameters (mass, luminosity, etc.). This was to yield physical information on individual galaxies and, in sum, of the distant galaxy population. In their publications, astronomers typically describe data reductions as a single linear sequence of reductions and calculations. However, I witnessed that these practices were reflexive and involved prospective and retrospective reasoning (Hoeppe 2014). Nancy's task was to combine circa 550 exposures taken over 3 years

[25] Analysts of social interaction may feel reminded of Schütz's (1962, p. 11–13) notion of the reciprocity of perspectives in the constitution of intersubjectivity.

with a wide-field near-infrared camera attached to the 3.5-m telescope at Calar Alto Observatory in Spain. These exposures were taken through four color filters transparent to certain wavelength-ranges of infrared light. They were to be analyzed along with data taken in visible light using the 2.2-m telescope at the European Southern Observatory on La Silla in Chile. Besides the near-infrared detector itself being fairly new, the filters used in this project had not been employed before at the precision that Nancy's project required. This situation is common in the inductive research of astronomy.

4.1. A moment of trouble

Nancy discussed each step in the course of her work with Ken, her supervisor. Whenever he considered her results "good enough for now", she went on to the next step. But at some points, the smooth flow of Nancy's work was suspended. In the following, I focus on one such moment. When Nancy got to the last step in the sequence of work, the template fitting, she became excited. The galaxy parameters she obtained from the template-fitting algorithm seemed to reveal a large number of surprisingly bright, massive galaxies in the distant universe. Senior group members considered this to be possibly true, but surprising (remember Spergel's "important, if true"). Ken was most overtly critical and suspected that these are "objects that do not exist." As such, he made Nancy's findings accountable to astronomical discourse. Ken decided to re-consider the earlier steps of data analysis with Nancy. One of the first steps was to divide the science exposures (recorded at night) by so-called flatfield frames (recorded at the beginning or end of the night before stars appear in the sky).

As images of the twilight sky the flatfield exposures only record the pixel-to-pixel variations in the sensitivity of camera's pixels. Dividing the science frames (which contain the same variations but also stars, etc.) by the flatfields is meant to "cancel out" these artifactual variations and yield science exposures with desired "flat" backgrounds, that is, uniform noise levels. Ken wondered whether the flatfields that Nancy worked with were "flat enough" for the uses of their project. He had encountered problems with flatfield frames in his earlier work, particularly when scattered light had apparently entered the telescope during exposures at twilight. Lacking previous experience of working with such data and being new to the group's analysis techniques, Nancy could not possibly assess this herself. Ken explained to her that there might be artifacts in the flatfield frames that had yet to be corrected for. Like other senior scientists, he insisted that such artifacts were either additive or multiplicative. Scattered light recorded in the flatfield frames during twilight, emerging perhaps from reflections in the dome, the telescope, or the camera are considered additive, and must be subtracted. Artifacts in scientific exposures are considered multiplicative and have to be removed by dividing them by flatfield exposures.

Figure 2 shows a false-color image of one of Nancy's flatfields. The most conspicuous features in the flatfield are visible as a brightening toward the lower

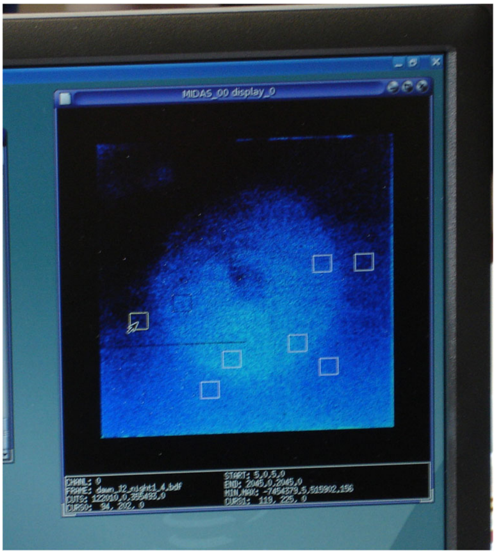

Figure 2. False-color image of one of Nancy's flatfields. The most conspicuous features in the flatfield are visible as a brightening toward the lower edge of the exposure and a roundish shape near its center. The white rectangles denote areas of the pixel image in which she had measured the noise level.

edge of the exposure and a roundish structure at its center. At this point, it was unclear if these features had been removed well enough. In conversation with Nancy, Ken wondered about the geometric shape of these presumed artifacts, their magnitude, and whether they were "additive" or "multiplicative." But how should one assess this?

4.2. Using the digital astronomical record

At a group meeting, Jim, a senior staff astronomer and group member like Ken, remarked that Nancy's field was included in 2MASS, the Two-Micron All Sky Survey (Skrutskie Skrutskie and 30 2006), which Borgman (2015) considers a part of astronomy's knowledge infrastructure. Widely known and considered a trustworthy source that is openly available on the Internet, 2MASS is a catalogue of 471 million celestial objects, most of which are stars.[26] Jim explained that this catalogue contains stellar positions as well as brightness measurements at wavelengths similar, although not identical, to those of Nancy's data set. Implying that this difference is "small enough" to not render the comparison useless, Jim argued that it may be useful to characterize the alleged artifacts in her flatfield frames. He asked Nancy to download the catalogue entries pertaining to "her field" (as specified by its celestial coordinates) from the 2MASS website and see whether there was a sufficient number

[26] See http://www.ipac.caltech.edu/2mass/. Accessed 19 May 2016.

of stars to cover the field evenly. If this was the case, she was meant to subtract these stars' catalogued brightness measurements (in astronomical magnitudes) from the "instrumental magnitudes" (brightness measurements not transformed to the magnitude scale of astronomical photometry) that she measured at the appertaining positions in a science frame resulting from her existing, possibly troublesome, flat field. As Jim explained, this would allow her to assess the shape of the presumed artifacts and to quantify their impact on the photometric measurements. Ken agreed that this was useful and asked Nancy to follow Jim's suggestions.

Within a few days, Nancy had succeeded in completing these tasks with some help from Jim. Her search of the 2MASS catalogue database, constrained with the celestial coordinates of the field of her exposures, yielded 31 stars. Jim deemed this "not many" but "sufficient to try this." Working with an ASCII file as generated through the 2MASS website, Nancy then calculated the brightness differences that Jim had specified, as well as the variations in the flatfield frame. Once she was finished, Ken asked her to summarize her measurements on a paper printout of the field. Thus equipped, she went to see him (Figure 3).

The following is a transcript of a part of their conversation. Nancy and Ken keep pointing to positions of 2MASS stars in the field and compare differences in the 2MASS photometry (in magnitudes, the unit of astronomical brightness) with the "instrumental magnitudes" (see above) as measured by Nancy in the science frame (using the "statistic cursor" routine; cf. line 1). She compares these numbers with the variation of the flatfield exposure (in per cent). As my transcript is based on an audio recording only, I am unable to relate the deictics ("here", "this", "that") to locations in her plot, but this does not affect my interpretation. At this point in their conversation, the "ring" had become an established topic of concern for them; both Nancy and Ken refer to positions of stars with respect to it.

Figure 3. Nancy briefing Ken about her assessment of artifacts in the flatfield exposures, using paper printouts to summarize her findings.

Transcript 5

1 Nancy: … those stars here … and this is the difference in magnitude … the white numbers … so … so I wanted to see where they're positioned in the flatfield … and then I calculate with statistic cursor what is the difference in the background for … between in and outside the ring.
2 Ken: ((*approving*)) Hmm.
3 Nancy: So … what I see … depending on position … ((*points at the plot*)) around here I have like basically the same background around 4 per cent in magnitude difference … between inside and outside the ring … here I have 7 per cent difference in the background but 12 in magnitude ... here I have 10 per cent in background … 7 per cent in magnitude, and here like 3 per cent background and 5 … 4.5 in magnitude … so … this is the same kind of tendency that … when the background difference is low then the difference in magnitude is low also ... but it's not the same numbers exactly … but the statistics are really low … also … you know … I have one star … two stars.
4 Ken: Yeah … the statistic is low … what was the number here? ((*points at the plot*)) Here it's very difficult … because you don't know where to see the outside.
5 Nancy: Well ... I have …
6 Ken: Maybe we ignore the … also this is the…
7 Nancy: I have the colour one … but
8 Ken: ((*points at the plot*)) … this is the … area where we found the problem … because of the very rapidly changing flatfield … it might be wiser to ignore this for the moment … for the diagnosis whether the ring is responsible too … for part of the problem … and I think therefore we should look at positions where the ring is seen more clearly … here … down … it's very difficult … the ring is best seen outside here.
9 Nancy: ((*points at the plot*)) Like here … and on (screen) I see it better … also … with the cursor I can really see if this dot is inside or outside ... so I know that this one is inside and this one is outside ... and the same for here … that is inside…
10 Ken: But that might change for some cases.
11 Nancy: Might change in this?
12 Ken: For stars … which are close to the border this might change ... ah no! You have single exposures … then it does not change … I thought the average over all measurements can change ... but

that thing is the main problem for large errors … okay… what is your conclusion?

13 Nancy: My conclusion is … that that can be because of the ring … the problem … but it's hard to see … if it's the only problem … because the difference is not exactly the same in the background and the magnitude ... but also … we have to take into account that it's only one star and … no statistics.

14 Ken: Sure … the magnitude of the stars or the magnitude difference of the stars is certainly not better than 0.03 or so ... it might depend on the stars ... I think differences which are only 3 per cent are not really relevant

(…)

Here, Nancy recounts what she had done (line 1). Following Ken's approval (in line 2) she reports on her measurements (line 3). Ken again approves of her work (line 4) and subsequently directs her attention to the ring feature, prioritizing its repair (line 8). Shortly thereafter, in what may be heard as an "exam question", Ken asks Nancy to assess her measurements. Nancy demonstrates that she recognizes the importance of the ring feature, but she is unable to tell which differences and variations are small enough to go on (line 13). It is Ken who (in line 14) draws on his experience to suggest that differences of 3% are tolerable, or "same enough" to go on. What "sameness" could mean in this particular case is a practical problem for Nancy, who cannot know the answer by herself, but has to be guided to recognize it.

4.3. Formulating calculative things for repair

Later in this conversation, Ken went to the blackboard in his office to sketch what he believed to be the structure of the artifacts in the flatfield: a circular ring in the center of the image and a more or less continuous gradient increasing from its top to its bottom (Figure 4). While Nancy and Ken had hitherto considered differences between published measurements from the 2MASS catalogue and their own instrumental magnitudes, Ken now produces a characterization of artifacts as geometric and calculable things that can be quantified.

Transcript 6

Ken: This would give a consistent picture … if we have something which is … the ring itself is only on the order of the lower value … maybe 5 per cent or whatever. And we add up … a problem which increases from 0 per cent here to 10 per cent here. ((*points at the plot*)) That means … another flatfield problem which increases form the upper part to the lower part. Then I think all your numbers would be roughly

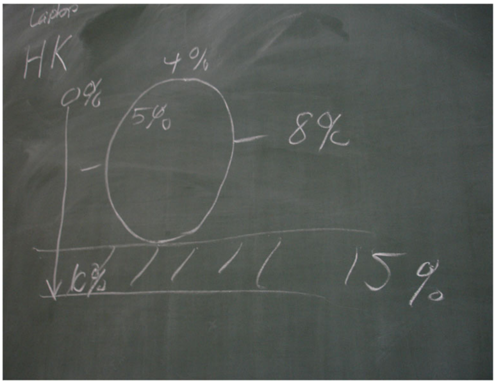

Figure 4. Ken's schematic characterization, and quantification, of artifacts in the flatfield frames on his office blackboard.

> consistent … or? What do you think? We can normalize this also somehow and say that … the ring problem is 8 per cent and you have an addition from minus 5 per cent to plus 5 per cent problem. I don't know. It seems that there is a gradient. And I don't know what the reason of the gradient is. The ring problem I can understand. But the gradient problem is much more difficult.

Noticing its radial symmetry centered on the "optical axis" (the center of the exposures), Ken accepts the ring as an "optics problem," but is unconcerned with its exact cause. He finds the gradient (to which he had referred in line 8 in Transcript 5) more puzzling, and muses without conclusion about its possible origin. Lacking his experience, Nancy has little to contribute other than to consider the flatfield frames, her measurements, and Ken's sketch. She seeks clues in the observing situation at the observatory that she had witnessed in person, remembering that moonlight may have entered the telescope:

Transcript 7

Nancy: But I have a question … if the straylight come from basically outside the field … then if the moon is close … you don't have any more (…) I remember that there is some run that the moon was close … if it's wider …

Ken: That gives you additional effects … but the straylight at night is not affecting the photometry … because you subtract it as a sky subtraction … EVALUATE [a software routine] looks at the local background around your objects and subtracts that. Any straylight during the night is not of interest to us … it is taken out by the full photometric pipeline anyway. What affects us is straylight in the flatfields. Because staylight in the flatfields … we divide everything

> by this wrong flatfield. And not only the sky we divide by this wrong flatfield … but all the stars we divide as well. All the star images we divide as well. And … therefore … the photometry is flawed. And therefore the only thing you have to do is to get rid of the straylight in the flatfields. The moon might produce somehow tilted backgrounds and things like that … but it doesn't matter. It looks not very nice … but the photometry doesn't see it … because it will look for the local background anyway. But the stray light in the flatfields we have to remove.

Ken dismisses Nancy's speculation about the effects of moonlight as irrelevant, since "the photometry does not see it," referring to the algorithm used to calculate with the pixel images. What mattered to him was the calculability of the image and of the artifacts in the calculus that constituted the group's numerical work. In their subsequent work, Ken guided Nancy to make a "model" that consisted of a "ring" and a gradient that was to be subtracted from the flatfield frames, which in turn were used to divide all science exposures anew. This model later proved "good enough" for Nancy's data to show "the same sky" as the measurements of the 2MASS survey. No further repair was considered necessary.

4.4. Discussion of this case

Prior to this episode, Nancy had calibrated her data in a standard fashion.[27] However, this calibration and standard flatfielding alone did not yield data that satisfied senior team members. It resulted, as Ken worried, in "objects that do not exist." Having thus reached a dead end (for the time being), Nancy had to be instructed about the resources and methods that she could legitimately use. After achieving to produce an improved flatfield image she had to return to earlier steps of her reductions and re-do them. She had to go "back and forth" like this at several stages in her work. Where to enforce sameness, where to allow difference, what to consider as the background of work, and what to foreground had to be decided at each turn. In this respect, her work was reflexive (Hoeppe 2014). As Livingston insists, if reflexive phenomena "are missing from a study [of practical action], we know that we're in trouble" (Livingston 2008, p. 204). Although essential, this reflexivity was so "uninteresting" (Garfinkel 1967, p. 7) to these astronomers that they did not mention the prospective-retrospective character of this work in the publication describing it. Neither did they

[27] Photometric calibrations in astronomy have been propagated by beginning with a single standard star, the primary calibrator (most often Vega, α Lyrae), to calibrate the brightness of secondary standard stars around the sky by comparison with Vega, and tertiary standard stars (calibrated in respect to the secondary standards, usually with declining precision; Hearnshaw 1996). This is what the Heidelberg group had done prior to arriving at the problems that I described, using one of the stars in the field as a so-called spectrophotometric standard star.

mention the reflexive, infrastructural uses of the sky. One may read this as implying the authors' understanding of their readers as members in the community of those who do not need such instruction. It spares practitioners the tedious task of providing lengthy descriptions of the work done, which would be inevitably incomplete at some point. This is where data users and those who do not belong to this disciplinary "form of life," are inevitably challenged. Nancy's reflexive use of the mediated sky was not made explicit in her published work, but it was essential to its success nevertheless.

These astronomers did work not only with digital media, but also with other media, including printouts on paper for making visual assessments. As Jim and Ken guided Nancy in removing possible artifacts, they first considered algorithmically derived parameters which they deemed implausible. They continued to look at flatfield exposures as false-color images on Nancy's computer terminal. After specifying possible artifacts using 2MASS data, Nancy was to summarize her findings on paper – a fixation in a medium that lacks the inherent calculability of the digital, but arguably gives more room for approximations and schematic assessments (Figure 3).[28] Ken went yet further by moving to a blackboard to schematically specify and quantify the "models" of the "numerical things" (the gradient and the ring) that were to become part of their calculations (Figure 4). It was by engaging these non-calculable media that specifying what was "good enough" to go on (or "same enough") could be achieved, constituting a moment of learning for Nancy. This moment was specific to the technique these astronomers used. That variations of 3% were "good enough", as Ken mused (Transcript 5, line 14), got inscribed into the data set they produced, but it was nowhere explicated in their publications.

5. Discussion

The scientists involved in these two cases made ample use of what has been called the knowledge infrastructures of astronomy, including standard data formats, calibration sources and public datasets. However, these resource alone were insufficient to resolve their practical problems of diagnosis and repair. For these researchers, the (mediated) sky was not only the topic of their work, but also an infrastructural resource, which they employed through specifically situated practices. Drawing on my description of the cases, I begin the following Discussion by returning to the notions of topic and resource that I introduced early in this paper (Section 5.1.). Then I consider the stability of reference to celestial objects across changes of scientific paradigms (Section 5.2.). Subsequently, I compare the infrastructural uses of mediated environments with those of mediated objects (Section 5.3.). Finally, I discuss the

[28] This resembles an argument that Gaston Bachelard (2002, Chapter 11) makes for the history of science more generally.

infrastructural uses of "boundary object" in the light of my observations (Section 5.4.).

5.1. Topic, resource, and the infinite task of knowledge infrastructures

Many ethnomethodologists have encountered the distinction of topic and resource through two early classic papers in the field, Zimmerman and Pollner (1970) as well as Garfinkel and Sacks (1970). Both are pertinent to my argument. Zimmerman and Pollner criticize sociology as a folk discipline that shares its terms with the ones employed by the members of the settings studied. Not only does sociological knowledge stand in competition with that of informants. What is more, in generating it, the topic of research and language as a resource for its study are confused. Zimmerman and Pollner recommend to analysts of the everyday social world to avoid this confusion by assembling the "occasioned corpus of setting features", the set of methods that members use to produce social phenomena. Focusing on conversation and social action, Garfinkel and Sacks (1970) appreciate the turn to member's methods but insist that there cannot be a position external to the "field of action" studied (Lynch 1993, p. 146). Topic and resource remain tied to each other as the definiteness of sense, of what is "good enough" for the time being, is achieved reflexively: descriptions of a setting become a part of it (Lynch 1993, p. 17). This, I argue, pertains to scientific practice as much as to ordinary social interaction.

Scientists' work with digital data may not correspond to social or conversational interaction in every detail, but both involve practices of sense-making that are unavoidably reflexive (Hoeppe 2014). It is common to much (inductive) scientific work that new observations go beyond established calibrations, such as when new detectors are being used, new wavelength ranges are considered, and novel precision is called for. This was the case both in respect to the measurements of the BICEP team and in the work of the Heidelberg research group. Standards and calibrations provided by knowledge infrastructures in the sciences are typically challenged as scientific work progresses. This is one sense in which the task of knowledge infrastructures is infinite. The mediated availability of environments and objects whose order and stability scientists agree upon, is a resource for dealing with such challenges.

In his critique of the BICEP analysis, David Spergel formulated sameness as an expectation. He did not point to any specific cartographic detail of the microwave sky but used statistical tests to assess BICEP data in this light. Spergel had, however, pointed to the data of two other projects, ACT and Planck, to demonstrate that observations of the cosmic microwave background do, by now, "see" the same, specific structures in the sky – and that the BICEP telescope ought to see "the same sky" in its different configurations as well. The members of the Heidelberg group achieved sameness by generating a model of the flatfield artifact to make measured magnitudes of stars in the field agree with those of the 2MASS catalogue. In both of these cases, the scientists' methods of mundane astronomical reasoning made use of incorrigible propositions about the sky, such as the continuity and uniqueness of the world. These scientists cannot reasonably doubt such propositions from within their practices without contesting the shared agreements on which these practices dwell (Pollner 1987; Wittgenstein 1969).

5.2. Ontologies, paradigms and the reflexive uses of the mediated sky

As mentioned in the Introduction, Edwards et al. (2013) argue in a Kuhnian spirit that the use of "object-oriented" ontologies in knowledge infrastructures is problematic because of their reliance on paradigms of scientific disciplines that are subject to revision and transformation. Moving away from the hegemony of theory and strong notions of the theory-ladenness of observation, post-Kuhnian studies in the history and philosophy of science have pointed out that not all scientific observations are affected by paradigm changes.[29] Astronomy is a pertinent example. Indeed, many astronomical observations have remained usable across some of the most famous historical shifts of scientific paradigms, including the Copernican revolution that Kuhn discussed.[30] This was largely possible because of the "cartographic" organization of astronomical data that has remained largely unaffected by shifts in paradigm. Thus, Bruno Latour comments on Elizabeth Eisenstein's (1979) study of the impact of printing on astronomical research:

> The hagiographers say that [astronomer Tycho Brahe] is the first to look at planetary motion, with a mind freed of the prejudices of the darker ages. No, says Eisenstein, he is the first not to look at the sky, but to look simultaneously to all the former predictions and his own, written down together in the same form. (…) The discrepancies proliferate, not by looking at the sky, but by carefully superimposing columns of angles and azimuths. (Latour 1986, p. 20).

Here the visual observations of planetary movements, mediated on paper as printed documents, by Tycho Brahe and his predecessors provide for a perspicuously simple case: a few planets moving in respect to the background of the starry night sky that can be represented using celestial coordinates. Coordinates given in "angles and azimuths" allude to the cartographic organization of astronomical data. Different cosmologies make different predictions of planetary movements that can be compared to this record. Much of this simplicity then rests in the mappings (using numbers or visual media) of the sky that have been maintained across distinct cosmologies, from those of Ptolemy to those of Copernicus, Tycho

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[29] Examples are Feigl (1974), Galison (1997) and Chang (2004).

[30] Another prominent example is the discovery of precession (Evans 1998).

and Kepler. Celestial coordinates are decisive in any modern documentation of the astronomical record.

Positing the sameness of distinct records of the sky through mundane reason draws on beliefs that astronomers cannot meaningfully question, as Owen's pun (Transcript 1) illustrates (Pollner 1987; Wittgenstein 1969). In this sense the use of incorrigible propositions does indeed resemble popular understandings of Kuhnian paradigms, where, following a paradigm shift, scientists live in a new world. However, scientists routinely draw on practices that are mundane or ordinary and prior to elaborate theorizing, as philosophers Edmund Husserl, Ludwig Wittgenstein and Gilbert Ryle have insisted (cf. Lynch 1993). While technological mediations characteristic of scientific observation may appear to challenge this mundaneity, the two cases described above illustrate its enduring pervasiveness.

I have pointed out above that the distinction of topic and resource is inevitably contingent on the project that scientists pursue. In addition, the stability of the sky and its objects, which the astronomers dwelt on in the two cases that I described, is itself good until further notice only. Refinements in the precision of coordinate systems, and a growing record of observations have made it possible to describe objects in the sky as moving in respect to a static background and discover variations in the radiation flux from cosmic sources, giving rise to a "dynamic sky" (e.g. Cordes 2012). The distinction between a "static sky" and a "dynamic sky" – which has grown in importance as the precision of observations has increased – is contingent upon definitions of coordinate systems, the available record of earlier observations, and technical developments of observing (Desai et al. 2016).

Astronomy provides an intriguing contrast to the infrastructures of Earth observation in meteorology and the climate sciences. Work in both meteorology and astronomy is arguably characterized by the reflexive engagement of physical totalities. However, one may notice that the infrastructures engaged in the respective disciplinary work stand in distinctly different ways toward the objects of inquiry. While scientists can document the increase in global atmospheric temperatures only through refined human-made infrastructures (Edwards 2010), for astronomers it is the sky that they can observe and re-observe, and can, thereby, not only refine and adjust their calibrations, but extend them reflexively to new contexts. The sky is the most stable element of their practice – Owen and his colleagues imagined its infrastructural inversion but could not perform it (see section 2.1.). This does not denigrate the importance of standards, classifications, and coordinate systems for astronomical work. In his study of meteorology's "infrastructural globalism," Edwards 2006, 2010) argues that "'the world' as a whole is produced and maintained—as both object of knowledge and unified arena of human action—through global infrastructures" (Edwards 2006, p. 230). In astronomy, by contrast, it appears that the shared availability of the sky precipitates a sort of "objectual globalism" with infrastructural uses.

5.3. From mediated environments to mediated objects

As Pollner remarks in view of Merleau-Ponty's (1968, p. 15) notion of the world as the "Great Object", "[f]rom the viewpoint of mundane reason, the world, as the over-arching context of lesser objects and thus an object itself, is idealized as a 'finished explicit totality in which the relations are those of reciprocal determination'" (Pollner 1987, p. 41). While I have considered the sky as a mediated *environment* in astronomical work since it encompasses all observers on Earth, Pollner's understanding of "lesser objects" suggests that there is no essential distinction between the reflexive, infrastructural uses of mediated environments and mediated objects. Mundane reason applies to practices of engaging either of them. Astronomers' open-ended reflexive uses of the sky benefit from, and draw on, its richness in salient features (Gurwitsch 1964), which allow novel uses, including Jim's suggestion to use the 2MASS catalogue as a resource for resolving the group's flatfield problem. What matters, then, is that users of data reach agreement on salient features of objects through employing shared methods.[31]

Many other mediated objects may likewise exhibit diverse salient features that researchers can engage for the re-use and repair of data independently of "object-oriented" ontologies tied to specific disciplinary paradigms in a Kuhnian (Kuhn 1962) sense. How, and where, could such objects be identified beyond astronomy? One could look down instead of looking up, and consider work with satellite images of the Earth, such as addressing changing vegetation cover, urbanization, and features of global change. In these cases, reference would be largely cartographic.

In other instances, reference is achieved through practices that are classificatory or taxonomic. For example, spectroscopic measurements of rocks are routinely used across diverse contexts of geological work, such as in comparing those found on Earth with those seen on Mars (e.g. Vertesi 2015). Here the uses and interpretation of spectroscopic data of materials appear to be unaffected by shifts in geological paradigms.[32] Further examples are preparations of biological specimen (Rheinberger 2015) or model organisms (Ankeny and Leonelli (2011). Another possibility would be stable digitally mediated objects which are widely available, but which can be referenced without drawing on contested ontologies. One example is neuroscience research that uses magnetic resonance imaging of human brains (e.g. Alač 2011; de Rijcke and Beaulieu 2014).

[31] Researchers typically achieve reference to scientific objects through shared practices pertaining to specific scientific domains. In other domains, the identity of objects can be more fluid (cf. Roth and Jornet 2018).

[32] Note that Vertesi (2015) does not make a claim about infrastructures or ethnomethodological reflexivity in this case but illustrates the use of spectroscopic measurements of terrestrial rocks to interpret those of Martian rocks.

5.4. Is the mediated sky a boundary object?

Susan Leigh Star and her co-authors have observed (Star and Griesemer 1989; Bowker and Star 1999; Star and Bowker 2006; Star 2010) that infrastructures may grow when members of different social worlds cooperate through sharing specific "boundary objects"– "scientific objects which both inhabit several intersecting social worlds" and are the same across contexts (Star and Griesemer 1989, p. 393). Examples in the natural sciences include "specimens, field notes, museums and maps of particular territories" (Star and Griesemer 1989, p. 408). Bowker and Star (1999) argue that such boundary objects can grow into "boundary infrastructures" (Bowker and Star 1999, p. 287). Given the importance of establishing "sameness" in both of the cases I considered, isn't the sky a boundary object in the work of these astronomers?

"Boundary object" is an analyst's category that posits the "sameness" of objects in distinct contexts. This differs from ethnomethodologists' view that agreement on what is "the same" is inevitably a methodical and practical social achievement (Lynch 1985, pp. 179–201; Sacks 1992, vol 1, pp. 428–434). In the light of my two cases, Garfinkel's (1967: vii) stance, mentioned above, that "every reference to the 'real world' (…) is a reference to the organized activities of everyday life," is adopted when considering mediated environments and objects. Thus, in guiding Nancy to engage the stability of the sky for correcting artifacts in her flatfield exposures, Ken focused on the practices of assessing and achieving sameness, for example by alerting her that "differences which are only 3 per cent are not really relevant" (Transcript 5, line 14) and therefore "good enough" to go on with (cf. Koschmann and Zemel 2014). Ken did not make the (mediated) sky itself topical. Likewise, how Spergel assesses the Planck satellite and Atacama Cosmology Telescope to "see the same thing" (Transcript 2) draws significantly on the practices of noticing the similarities between the two maps. In either case these scientists agree on the specific order of the "real world," much like in the discussion of observing the galaxy cluster in Transcript 1. Such "constitutive work" (cf. Rawls 2008, p. 4–5) is lost by the notion of boundary objects.[33]

What remains pertinent of Star and Griesemer's (1989) account of the Museum of Vertebrate Zoology in Berkeley, California, for my argument is that a rich diversity of material resources, including museum collections, provides an abundance of saliences that can be used for the methodical achievement of agreeing on sameness and difference.

[33] In pointing out the "materiality [of boundary objects] derives from action, not from a sense of prefabricated stuff or "thing"-ness", Star (2010, p. 603) demonstrates that she is mindful of the importance of practices, but she does not say whether or not the achievement of sameness counts among these.

6. Conclusions

Knowledge infrastructures of science have been considered as human-made networks or ecologies of people, artifacts, and institutions that enable the production, calibration, storage and dissemination of data. Inspired by Peters's (2015) meditation on infrastructural media, this paper has examined how scientists use the digitally mediated, shared availability of objects and environments for their cooperative and computational work. My objective was to assess how "opportunistic" uses of "natural" objects and structures (Hutchins 1999, 2005) matter to scientific work in which calibrations and the stability of reference become topical. I have tried to specify how astronomers make use of the phenomenal properties of the sky and their digitally mediated availability. I find that it is useful to consider the sky not only as a topic of astronomical work, but also a resource for its conduct beyond the accounts of their work that astronomers themselves typically give – a resource that members routinely use in methodical ways that ethnographers can specify.

A question commonly posed to ethnomethodological studies is whether, and inasmuch, it is possible to generalize from the detailed study of the work of one domain of practice (Sharrock and Randall 2004; Button et al. 2015). With this paper I aim to contribute to a corpus of studies that, while not out to generalize, may very well arrive at conceptual stability eventually.[34] Scientific practice is unavoidably reflexive in an ethnomethodological sense. Like people in other walks of life, scientists use whatever they have to organize and order their affairs (Liberman 2013). Astronomers, of course, have the sky – along with observatories, technologies of recording and processing data, standards and the existing record of astronomical observations. They also have their practice in mundane reasoning. But researchers in other disciplines have such practices and materials as well, and these may be just as consequential for their work while remaining invisible in the accounts that they produce about it. Studies of knowledge infrastructures would benefit from taking these practices and materials into account, beginning with making them visible by means of ethnography.

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[34] I thank an anonymous reviewer of this paper for formulating this goal as such.

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