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The One-Dimensionality of Scientific Relativism

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Abstract

The historicist approach to science has been accompanied by a culturalist one in the last decade or two. Epistemic localism added a horizontal axis to the existing vertical (historical) one thus science came to be presented in a coordinate system as a manifold of epistemic traditions. Taking the debate about the existence of the N-ray as an instructive example, I argue that the historical development of science creates disciplinary communities that impose unified epistemic standards on the communities scrutinizing the same aspects of reality. Accordingly, with the advent of such communities relativism became one-dimensional: science has developed into a historically changing culture that puts up a successful fight against epistemic diversity in its synchronous dimension.

Keywords: epistemic systems, geographical turn, local epistemologies, N-ray, objectivity, relativism

Introduction: Two kinds of cultural relativism

The historical approach to science in the middle of the last century revealed that scientists in different eras use incompatible concepts, methodological norms and epistemological standards for constructing and justifying scientific knowledge. Sociologists and geographers of the last decades went on with this destruction of the unity of science claiming that scientific knowledge "... is not to be thought of as some transcendent entity that bears no trace of the *parochial* or *contingent*. It needs, rather, to be qualified by *temporal* and *regional* adjectives" (Livingstone, 2003: 13, my italics). Thereby a "geographical" or "spatial turn" has been added to the historical one, creating a new dimension of relativism in science. The protagonists of this spatialist approach agree that the "... *issues of space* – location, place, site, migration, region – *are at the heart of scientific endeavour*" (Livingstone, 2003: 5, my emphases) and with "the 'geographical turn' evident across science studies ... different geographies of science are emerging" (Powell, 2007: 309). The addition of this further "turn" to the already existing ones is meant to indicate that concepts and standards of science vary with regions, therefore "just as there is a rich history of science, so there is a rich geography of science" (Withers and Livingstone, 2011: 3). It is claimed that the "... processes of knowledge production" require "judgements and negotiations by groups of scientists in *specific contexts*". Accordingly, cartographers of science take on the task to reveal "*the specific sites*" at which "... particular scientists with particular skills, materials, tools, theories and techniques" (Turnbull, 2002: 6, my italics) produce locally authenticated beliefs.

The same cultural relativism can be found in contemporary sociology of scientific knowledge as well. From the fact that this knowledge is "fabricated and negotiated by particular agents at a *particular time* and *place*", some sociologists come to the conclusion that it is "*local* rather than universally valid" (Knorr-Cetina, 1981: 33, my italics). Their conclusion is based on the premise that human reason and empirical evidence in themselves are epistemically not powerful enough, so they conceive knowledge in general as an "amalgam of experiences and socially mediated beliefs" (Bloor, 1976: 12). The social mediation of beliefs is concretised as setting up justificatory systems authorised by particular communities, therefore they look for "the specific *local causes of credibility*" (Barnes and Bloor 1982: 23 – my italics). Which beliefs are given credit depends on the epistemic standards a community deploys thus belief and knowledge differ not in the latter's being justified (and true), but in its being "collectively endorsed" (Bloor, 1976: 2-3), hence epistemic *justification* is to be replaced by *acceptance* (Bloor, 1976: 2-3).

The anthropological treatment of local relativism

Naturally, "spatiality" (just like "temporality") itself cannot be more than just a *synecdoche*: spatial or temporal coordinates have nothing to do with the justification of beliefs. What does have such an impact is the *epistemic system* people use for assessing statements. It is an "assemblage of principles and procedures that a given society or culture ... relies on, explicitly or implicitly, in distinguishing justified from unjustified beliefs..." (Williams, 2007: 94). It is often said that "e]pistemic systems vary from culture to culture..." (Williams, 2007: 94), but this does not involve that "culture" is synonymous with "being separated by a physical distance". There are different cultures in the same geographical region and the other way round, people unified by the same (sub)-culture may not be living in the same place. People of the same scientific school, tradition, paradigm etc. can adhere to the same commitments independent of their temporal or geographical location. Platonism, for instance, can be taken as an epistemic system accepted by people not belonging to the same historical era or geographical area. Certainly, cultural relativism used to be coupled with spatiality while communication, hence the authorisation of particular standards, presupposed direct, unmediated communication and interactions. Thus in the course of history epistemic systems differed due to their physical separateness, but geographical distance is not a necessary condition of such differences. What is necessary is a community that shares customs, traditions and authority structure that empowers the standards and adjudicates whether in concrete cases they are observed.

Such communities may materialize in contemporary science without their members being separated by physical distance (Longino, 2002; Coliva and Pedersen, 2017). However, the cultural relativism such communities generate is not easy to reconcile with the cognitive success of science hence attempts have been made to explain the cooperation among such traditions. One of the most ingenious of them was put forward by P. Galison who argued that the prima facie fragmentariness of science is a consequence of identifying holistic unities that make "unbridgeably isolated" knowledge blocks, "island empires" (Galison, 1997a: 17-18) using "incommensurable languages ... without a common divisor" (Galison, 2010: 42). He suggests that this vision is induced by the assumption that science must be a compact culture integrating theory, experimentation and instrumentation. Scientists themselves perceive their situation differently, therefore they do not worry about relativism. They can put up with the fact that science falls into more or less discrete subcultures, because they experience that, despite the incommensurable languages they use, their communities can escape "the methodological and philosophical commitment to relativism" (Galison, 2010: 42). The relatively independent, but partially overlapping communities, no matter where their members are geographically located, share concepts and practices enough to allow rational communication. Taking neighbouring lay cultures as his model, Galison describes the interfaces between separate scientific communities as "trading zones" where collaboration and repeated attempts at communication create inter-languages by a hybridisation of the particular idioms. Admittedly, such inter-languages provide only partial competence, but Galison claims that it is enough for mutual understanding and eschewing "block relativism" (Galison, 1997a: 14).

A slightly different still similar approach was suggested by Collins and Evans who distinguished between "contributory" and "interactive expertise", claiming that while the former presupposes full participation in the research process, the latter can be acquired by joining in discussions and collaboration with the competent speakers of the community to be understood (Collins and Evans, 2015, 2016). The interactive expertise obtained this way is restricted again, but enough for partial understanding and it allows discussion that can result in bridging over the conceptual gaps.

Galison, Collins and Evans alike use the cultural anthropological model of mediation between alien cultures for soothing the severe consequences of localist relativism in science. The shared aim of the conceptual innovations of "trading zone" and "interactive expertise" is to explain how mutual (though partial) understanding can be created by *local interactions* without homogenising the global conceptual schemes of the parties. Seeing the similarity of their pursuits, M. E. Gorman (Gorman, 2002), then Collins, Evans and Gorman together (Collins et al., 2007; cf. Collins et al., 2017) came up with the idea that by the combination of Galison's and Collins and Evans' ideas we can gain a framework suitable for dealing with incommensurable traditions and making thereby the inter- and multidisciplinary research possible (in what follows I refer to this as 'G–C-E framework').

I would argue that the anthropological method the G–C-E framework suggests may be suitable for handling the *semantic* problems caused by incongruent conceptual apparatuses, but it leaves the door open for *epistemic* relativism. The framework makes use of the linguistic and interactive relations of the communities: shared observable situations and interactions can help accommodate incongruent concepts and interpretations. Epistemic relativism, however, is caused by the incongruity of the deep-rooted standards by which evidence are gained from the observable situations and the epistemic merits of assertions are determined. Since these are *culturally ingrained commitments*, understanding of them is not identical with accepting their validity, hence the G–C-E framework alone is not sufficient for overcoming cultural relativism.

In what follows, I argue that science is a special culture that includes an urge to homogenise the epistemic system used. I present my position by means of a case study revealing how contradictory statements concerning a new kind of electromagnetic radiation were treated at the beginning of the 20th century. The case itself is known (see Blondlot, 1905a; Klotz, 1980, 1986; Langmuir, 1989; Nye, 1980, 1986; Seabrook, 1941), hence I am not going to rehearse all its details. I only highlight the features that the standard history leaves obscure though they shed light on the mechanism of epistemic homogenisation.

The rise of a local tradition

Here is the bald summary of the official story: In spring 1903, R. Blondlot announced the discovery of a new radiation that he named N-ray. After several failed attempts to replicate his experiments by other physicists, R. W. Wood visited Blondlot's laboratory and conducted there an *experimentum crucis* that empirically refuted this claim. "Wood rather cruelly published what happened in the laboratory. And that was the end of Blondlot" (Langmuir, 1989: 43).

The received explanation of the issue is that, tricked by his strong expectations, an individual scientist imagined seeing signs (namely brightness-changes of sparks) that in fact were not there. Thus he fell prey to self-delusion. As a matter of fact, however, the N-ray story was much more complicated than its standard rendering makes us believe. It is worth taking a look at the details.[1]

1. The existence of the N-ray *fitted* very well *in the knowledge context* of the era:
   More than one kind of radiation (X-ray, alpha-, beta- and gamma-ray, natural radioactivity) had been discovered in the preceding years, hence a further one could be expected.

2. Blondlot's and others' results *went through the official filters of science:*
   A huge number of papers were published about the topic in *peer reviewed scientific journals* (120 researchers published about 300 papers in a short period of time).

Lectures were delivered about the N-ray at the French Academy of Sciences and in prestigious learned societies.

Indeed, the significance of the discovery became so obvious that even *priority debates* broke out among scientists (Le Bon, Audollet, Huter vs. Charpentier).

3. The hypothesis was *explanatorily* remarkably *successful:*

   An until that time *unexplainable phenomenon* (the increased visual sensitivity of patients suffering from hysteria) became explainable by the N-ray.

   The N-ray promised to *explain parapsychological phenomena in a physicalist manner* (A. d'Arsonval came up with a physicalist account for the aura alleged to surround human body; Kéraval offered an explanation of telepathy by N-rays).

4. Most importantly, the discovery was *empirically massively reinforced*:

   The experiments were successfully *replicated* (at least 40 researchers demonstrated experimentally the existence of the N-ray between 1903 and 1906).

   Several scientists managed to *specify, extend and elaborate* Blondlot's discovery and *new scientific problems* were inspired by it (why certain materials radiate and others do not etc.).

   The presence of the N-ray was experimentally demonstrated in *other fields*: first in *physiology* then in *psychology* (A. Charpentier found that human nervous system emits such radiation; di Brazza claimed to have discovered rays emitted by the active brain).

   d'Arsonval *experimentally localised* the source of emission in the Broca-centre of the brain.

   A *quantitative correlation* was found between the intensity of the physical–psychical activities and the strength of the radiation emitted by the human body.

   By increasing mental or physical activity, physiologists seemed able to *manipulate causally* the emission and the quantity of the N-ray.

   J. Becquerel's *control experiments* showed that anaesthetisation of the nerves and contraction of the muscles can suppress N-ray emission.

Looking at the list of scientists, institutions, publications, experimental evidence and successful applications, one does not see a solitary scientist's self-delusion. What can be seen is rather a hot research area, a rapidly developing interdisciplinary field that quickly produces experimentally reinforced, quantified results promising solutions to several previously irresolvable problems. As the community constituted by the endorsement of Blondlot's methods consisted of researchers working in north-France (University of Nancy; the Sorbonne, the Institut Général Psychologique; Académie des Sciences in Paris), I am going to refer to it as the N-F community. Let us see how other scientists reacted on the claims this community published.

Failed attempts at an experimental replication

Several physicists at different laboratories in Germany, England and the USA (the G–A community, for short) made several abortive attempts to replicate the published experiments. The failures were explained in two different ways.

1) <u>Incompatibility with the received knowledge</u>

Some accounts referred to the irreconcilability of Blondlot's experimental results with the then known properties of radiations. H. Rubens, for instance, warned that Blondlot's assertion that the N-ray is an electromagnetic radiation of long wavelength that travels through a 0.1 mm window was *incompatible with Maxwell's theory* (Nye, 1980: 131). According to G. Sagnac's calculations the wavelength of the N-ray must have been so long that it *could not pass through the quartz lens* Blondlot used (Nye, 1980: 132). The properties of the N-ray were incompatible with V. Schumann's discovery from which it followed that the air and the quartz lens *would completely absorb* a radiation with the given properties therefore it could not have been seen without a vacuum spectrograph (which Blondlot did not use). C. C. Schenk (1904) pointed out that if the beams traverse a 5 mm wide slit then after passing through the prism they must become so broad that their intensity will considerably decrease therefore they *cannot be separated from each other*. He concluded that "under the conditions of the experiment it would hardly be possible to detect the existence of separate beams at all" (Schenk, 1904: 487).

The gist of these objections was that Blondlot's experimental results contradicted the received physical knowledge of the day therefore it could be a priori known that such rays could not exist. This argument, however, was not conclusive for two reasons:

a) As mentioned above, other experimenters reinforced Blondlot's outcomes, hence despite the a priori arguments, there was a posteriori evidence for it.
b) Radiation-physics at that time was a new field thus the so-called "received" knowledge was not confirmed firmly enough to adjudicate Blondlot's outcomes by it. Accordingly, Blondlot was quick to reverse the argument: the incompatibility of his experimental outcomes with the theory suggests that something is wrong with the received knowledge, therefore the strange properties of the N-ray indicate that he had hit upon something important. So when Wood expressed doubts that "a ray bundle 3 mm in width could be split up into a spectrum with a maxima and minima less than 0.1 mm apart...", he responded that "this was one of the inexplicable and astounding properties of the rays" (Wood, 1904: A 82–86).[2]

2) Concerns about the method

Several members of the G–A community were tempted to think that the bizarre properties are to be taken as evidence that the experiments had gone wrong. Thus, taking up Schenk's invitation that the scientific community should "... direct attention ... to certain experimental precautions not sufficiently observed ... by Blondlot" (Schenk, 1904: 486) the second group of arguments targeted the *experimental method* Blondlot applied.

a) O. Lummer gave a lecture in the *Deutsche Physikalische Gesellschaft* (Nov. 1903) in which he argued that the brightenings the experimenters sensed were due to the different light-sensitivities of the cones and rods of their retina, thus the whole phenomenon was but some illusion (Lummer, 1904a: 280). In the discussion that took place at the *76. Naturforscherversammlung zu Breslau* in 1904, he concretised his objection claiming that until the N-ray was objectively justified, he (together with H. Rubens) reserved the right to attribute the phenomena exclusively to physiological and psychological causes (Lummer and Weiss, 1904: 676). Eventually, at the sitting of the *Deutsche Physikalische Gesellschaft* he explicitly declared that "Blondlot's experiments may be almost exactly imitated in their effects *without employing any source of illumination whatever*" (Lummer, 1904b: 378, my italics). This meant that Blondlot's experimental method was not objective because he failed to exclude the experimenter's distorting effect from his experiments.
b) A. A. Swinton was not as harsh. He admitted that the brightness of the screen changed, but experimentally demonstrated that this was caused by the heat of the instruments (Nernst lamp, Auer burner) used (Swinton, 1904: 412). Thus, contrary to Lummer, he "proved" that *there was an external cause*, it only was not the N-ray. This again meant that the experiment was technically sloppy because Blondlot took a noise to be a sign.
c) S. Hooker conducted a control experiment keeping a container filled with hot water close to the screen, but he found that "there was absolutely no brightening" (Hooker, 1904: 686). On the other hand, putting the screen among the branches of a mimosa plant he experienced increased luminosity, thus he concluded that organic beings and what he called "human ray" caused the brightening. With this he not only admitted the presence of an external cause, but came quite close to Charpentier's, di Brazza's, and d'Arsonval's "physiological radiation" that they had identified as N-ray.

In sum, these experiments brought incompatible results that, contrasted with the successful replications of the N-F community, gave the impression that the discussion about the existence of the N-ray have reached a deadlock.

Breaking into the experimenter's regress

The G–A community did not acquiesce in this thus they opened a new line of attack. They took

Blondlot's occasional replay to his critics that they fail to see the brightenings because of their "lack of sensitiveness" (Wood, 1904: A 32–33) to mean that his experiments presuppose some *extraordinary ability*. Swinton noted with a tongue in cheek that "those who have unsuccessfully tried the experiments can only imagine that ... they are *only visible to certain individuals*" (Swinton, 1904: 412, my italics). Others went sarcastic: "I am at a loss to find any other explanation of M. Blondlot's result than that he has come across a radiation *to which some men are blind and others not so*" wrote J. B. Burke (Burke, 1904a: 365 – my italics). It was he who in another paper called the N-ray a "mysterious" phenomenon (Burke, 1904b: 198) and repeated this qualification at the 74th meeting of the British Association for the Advancement of Science where he gave an account of his efforts to reveal the nature of this "mysterious radiation" (Discussion, 1905: 468). The efforts mentioned included an investigation in which he "*had tried the vision of numerous persons*, but *in no case* was there satisfactory evidence of any external action upon the sight" (Discussion, 1905: 468). This was again a clear allusion to the *extraordinary ability* that Blondlot was thought to had made a precondition of proving the existence of the N-ray. Eventually, A. Turpain declared boldly that "if the N-rays *can only be observed by privileged rarities*, then they no longer belong to the domain of experimentation" (quoted by Nye, 1980: 144, my italics). Reading Wood's report about his visit to Blondlot we see that he was not free of this "expectation bias" at all: he went to see the laboratory "in which the apparently *peculiar conditions* necessary for the manifestation of this most elusive form of radiation *appear to exist*" (Wood 1904: A 6-8, my italics).

This pushing the N-F community toward irrationalism seemed to allow a spectacular solution to the problem of contradictory experimental results. The instrument Blondlot used came to be conceived as being composed of two parts: one physical (the spectroscope) and one human (Blondlot's mysterious sensitivity). Since the G–A community used a different (exclusively physical) instrument, the data the two communities produced were in fact not *incompatible*, but simply *different*. To demonstrate this, in the darkened laboratory Wood allegedly took the prism out of the spectroscope. This trick disabled the physical part of the instrument, leaving just the human one that even according to Blondlot detected only what the physical part projected on the screen. In Wood's report, Blondlot went on reading the spectrum of the refracted N-ray as if nothing had happened. So the fact that he saw the non-existing effects proved that not his visual ability, but another of his faculties, namely his imagination worked there, thus the data he saw were irrelevant.

Mentioning Wood's taking out the prism I used the adverb "allegedly" because the only documentation we have of this action is what M. Ashmore called "the tale of the Removal of the Prism" (Ashmore, 1993: 67). As he pointed out, we have to rely exclusively on Wood's testimony concerning when the prism was in or out and how these physical states correlated with what Blondlot said he was seeing. Wood claimed he had taken the prism out of the spectroscope thus no visible spectrum could exist; Blondlot, however, continued to see the spectrum therefore he was sure that the prism must have been in place (cf. Ashmore, 1993: 86). This means that Wood simply replaced the original problem (whether the replicated experiments failed to show N-rays because there is no such radiation, or because Blondlot's experiences were caused by experimental sloppiness) with a new equally undecidable one (how Blondlot's claims were related to the prism's alleged ins and outs).

The situation was what later came to be called the "experimenter's regress" (Collins, 1992: 83-89). The disagreement between the two communities seemed irresolvable by the standard means of science: Blondlot's positive results could be valid detections and other physicists' inability to replicate could be caused by their using an inappropriate (exclusively physical) instrument – or the other way round: their instrument was adequate and they did not see the N-ray because it did not exist. The question at that time was undecidable. All the less, because by its rash publication Wood's act became *irreplicable* as well, since after what happened, Blondlot (or other N-rayists) could not be expected to report their private sensations of extremely delicate stimuli with the original unsuspecting innocence (cf. Ashmore, 1993: 90).

The only chance to break into such an experimenter's regress is to find "a criterion … which is independent of the output of the experiment itself" (Collins, 1992: 83). Wood found such a criterion, namely *the authority of the G–A community* that approved his "tale of the Removal of the Prism" as a scientific demonstration of the fact that Blondlot's complex equipment was inappropriate. Thereby the G–A community was justified in saying that the N-ray did not exist since the proper instrument could not detect it.

Notice that this breaking into the experimenter's regress was made possible by two factors:

a) The G–A community's endorsement of the "tale of the Removal of the Prism", and
b) the community's interpretation of Blondlot's words on the visual capacity needed for the success of the experiment.

It is obvious that without the G–A community's authoritative support Wood's procedure would never have qualified as a "scientific" proof, let alone an *experimentum crucis*. He unabashedly presented conditional formulations of unjustified assumptions as incontrovertible evidence: "It *appears* to me that it is quite *possible* that…" (Wood, 1904: A 52–56, my emphasis), or "I *feel very sure* that *if* a series of experiments *were made*... the source of error *would be* found" (Wood, 1904: A 62–67, my emphasis). In vain produced Blondlot photos showing an increased brightness on the unshielded half of photo-sensitive plates exposed to the N-rays, they were brushed off with the remark that the photos "were made, *it seems to me*, under conditions which admit many sources of error…" (Wood, 1904: A 44–45, my italics). He completed rhetorical manipulation and neglect of facts with wild exaggeration. According to the report, when Blondlot was asked to indicate when he saw changes in the brightness while Wood was alternately blocking the source of the N-ray, "*in no case* was a correct answer given" (Wood, 1904: A 36–37 – my italics), and Blondlot "was *almost 100 per cent* wrong" (Wood, 1904: B 14, my emphasis). In a similar test "*in no case* was a correct answer given…" (Wood, 1904: A 36–37, my italics). Even if we assume that, seeing no real signals, Blondlot gave random answers, Wood still would have owed an explanation why the usual probability distribution of random choices was so extremely distorted. He, however, did not have to care about even the most basic requirements of an empirical refutation. The G–A community backed unanimously his claim that this action was the "crucial and most exciting test" (Wood, 1904: B 25) proving that Blondlot could not see traces of the N-ray.

The same went for the short and quick concluding part of the "falsification process". Having spent hardly more than three hours in Blondlot's laboratory, Wood returned to Paris and next day he sent a report to the *Nature*. Leaving all the usual precautions aside, this was published immediately. There was no peer review process, the report came out as a "letter to the editor". Despite providing no detailed account of a systematic and repeatable test, the report was immediately given full credence. Indeed, it was republished in leading scientific journals of the field in quick succession (September: *Nature*, October: *Revue Scientifique*, December: *Physicalische Zeitschrift*) making sure that everyone concerned be authoritatively informed about the debunking. The whole action was carried out in a desperate hurry and was in startling contradiction with the advertised "organised scepticism" of science: the meeting in Cambridge at which Wood's visit was decided took place in late August 1904. He visited Blondlot's lab 21st September, and his letter in the *Nature* came out the 29th. The completing phase of the whole N-ray affair was then the consolidation of the contemporaneous interpretation. Historians of science subsumed the case under the category of "pathological science" (Langmuir, 1989).

What was at stake?

Considering the importance of the question whether there was or there was not a further kind of electromagnetic radiation one finds difficult to comprehend why such a large-scale operation was launched. Not a fundamental theory was refuted, both the existence and the non-existence of the N-ray fitted perfectly into the basic physical views of the era. Thus it is not easy to get rid of the conjecture that the serial of publications in the leading physical journals was rather a dramatic demonstration.

To see what happened, recall that Blondlot's opponents spoke about some mysterious ability he allegedly relied on. In fact, however, Blondlot never referred to any extraordinary capacity. What he said was that the stimuli in the N-ray experiment were just above the human perceptual threshold, therefore the observer should look at the screen as "an 'impressionist' painter", and that "to attain this *requires some practice*, and is not an easy task" (Blondlot, 1905b: 83, my italics). That some sensations require previous practice was not an unheard idea in the psychology of sensation at that time. Blondlot even quoted Helmholtz saying that certain sensations "demand much practice and consequently many facts of this nature cannot even be observed without long preliminary practice ... On many points, therefore, we are restricted to the observations of very few individuals" (Blondlot, 1904: 24211). He echoed this last sentence saying "a few person succeed at once, others after more or less practice ... a few never succeed" (Blondlot, 1904: 24211). It is perfectly clear that his remarks on the personal differences in seeing referred to an *acquirable skill* and not to some extraordinary *gift*.

Anyhow, skills were not admitted into the methodology of experimental physics of the time either. The idea that the success of an experiment depends on the *active intervention of the experimenter* seemed so absurd to the contemporaries that it overshadowed the difference between an inborn ability and a learnable skill. At that time, to use one's personal skill appeared a serious deviation from the norm that the cognitive process should be strictly separated from the subjectivity of the epistemic agent. This was the taken-for-granted conception of the "mechanical objectivity" (Daston and Galison, 2007: 115-190) that dominated the 19th and early 20th century science. The physicists were convinced that the experimenter must act as a "will-less machine", like a camera that was believed "... to offer images uncontaminated by interpretation" (Daston and Galison, 2007: 121, 139).

To put it bluntly, the N-F and G–A communities were committed to *different epistemic systems*. Their debate was not about a contingent *fact* (whether there is N-ray or not), but about the *legitimate epistemic standards* to be used for the justification of scientific claims in experimental physics. They had no conceptual problems, they shared their cognitive aims, values, theoretical backgrounds, and were committed to the general method of experimental justification, to which they deployed standard instruments. They ascribed identical meanings to the crucial terms like 'radiation', 'electromagnetism', 'spectroscope', 'wavelength', 'prism' etc., still they assigned contrary epistemic values to the claim "the N-ray exists". The standards of objectivity they used differed in at least three interconnected respects:

a) What does the *objectivity demand*: the mechanical exclusion of the experimenter or rather a participative attitude?
b) Consequently, what is *the appropriate attitude of the epistemic agent*: should s/he be passive or interventionist?
c) What is to be regarded a *proper experimental instrument*: is it exclusively physical or can it be supplemented by human skills?

These standards determine the conditions under which experimentally produced beliefs count as justified, consequently the one concerning the existence of N-ray may take opposite, but equally rationally certified epistemic values. The partisans of the mechanical objectivity found it blatantly obvious that "*objective*" meant "being determined exclusively by the investigated *object*" and any interference of the epistemic agent leads to prejudiced, biased, therefore *subjective* results. Lummer concretised this general conviction when he declared that the existence of the N-ray could only be admitted if it was "incontestably proved by means of *objective instruments* of precision" (Lummer, 1904b: 380, my emphasis). Blondlot realised that in order to be an objective instrument the spectroscope needs to be completed with a trained eye capable of noticing feeble signs, discern subtle differences, distinguish between sign and noise. Without such a skill, he thought, the spectroscope did not show the fine effects of reality on the screen therefore it was *not* an *"objective instrument of precision"*. Indeed, because of their taken-for-granted principle of mechanical objectivity, Blondlot's opponents were not in the position to be able to debate his claims by empirical arguments. As it expressly forbade them to acquire the necessary skill, they could not be sure that the screen *in fact did not brighten* or their eyes were simply not sensitive enough *to notice its actual brightenings*. That is why Wood had to resort to an indirect argument claiming that the "tale of the removal of the prism" proved that his imagination deluded Blondlot, therefore it did not matter what sensations he had.

Briefly, both Blondlot and his opponents could rationally think they proved their case by objective experimental data. Objectivity, however, meant different things for them so they justified their contradicting propositions by applying different standards. That is exactly what we usually call epistemic relativism that, according to Boghossian (2006), is comprised of three interconnected theses:

1) "Epistemic non-absolutism": there are no objective empirical data or logical inferences that unequivocally justify a belief.
2) "Epistemic relationism": the epistemic value is always related to some epistemic system.
3) "Epistemic pluralism": "There are many fundamentally different genuinely alternative epistemic systems, but no facts by virtue of which one of these systems is more correct than any of the others" (Boghossian, 2006: 73).

The G–A community put up a naive defence against relativism attempting to refute the first thesis by presupposing the absolute cognitive power of reason and/or experience. They hoped that, like in he case of semantic relativism, by the observation of the situations in that beliefs arise and by the application of rational assessment they gain uniform justified beliefs, thereby the second thesis becomes invalid, hence the third irrelevant. When such attempts fail, usually there seems to remain no alternative but to admit that "there are no absolute proofs to be had that one scientific theory is superior to another: there are only locally credible reasons" (Bloor, 1999: 102). As an expression of a general theoretical position this assertion may be true. Applied specifically to science, however, the expression "locally credible reasons" calls for a special interpretation.

Was experimental physics at the beginning of the 20th century in fact pursued by communities that could cherish their own "epistemic norms" lending "local credibility" (Seidel, 2014: 143)? Not at all. The contemporary physicists sprang to the defence of the consensual standards of objectivity. The quick and easy approval of Wood's fishy debunking, the replacement of argumentation with rhetoric, the hasty publication of the dubious "refutations", and the neglect of the difference between an inborn gift and an acquired skill demonstrates that epistemic relativism was not considered a viable option. The physicists of the G–A community did not look upon themselves as representatives of one of the possible epistemic systems, but as the guardians of scientificity and rationality as such. This suggests that science makes an exceptional sort of culture in which the inference from non-absolutism to localism is not automatically licensed.

Local communities and disciplinary cultures

Galison is convinced that the attitude of tolerant discussion and cooperation between incommensurable cultures of science "offers an *alternative* both to the picture of crazy-quilt *fragmentation* and to one of *homogenous unification*" (Galison, 1997a: 51, my italics). Accordingly, as the G–A and N-F communities had "distinct cultures" (Galison, 1997a: 51) with "differences in classification, significance, and *standards of demonstration*" (Galison, 1999: 146, my italics), their integration would have required some hybrid language facilitating rational "exchanges (coordination), worked out in exquisite local detail, without global agreement" (Galison, 1997a: 46). In fact, however, we find no attempt either at establishing it or at obtaining interpretive expertise. What could have been a better occasion for such a local coordination than the *téte-a-téte* between Wood and Blondlot? Still, no attempt was made at that. What happened was a rather belligerent intervention and a truculent disqualification of the deviant standards and their advocates. Instead of steering a middle course between fragmentation and unification we see a hard push for the latter.

Looking for an explanation we find the difference between *understanding* the meaning of sentences expressing *beliefs* and *justifying* the normativeness of *epistemic standards*. The two groups had no problems with the former, but did not even try to come to terms with the latter. The rational resolution would have taken arguments and reasons, however, the binding force of arguments and reasons would have been provided by the very standards they were expected to be arguments and reasons for. So the usual ingredients (genuinely different epistemic standards + "norm-circularity" (Seidel, 2014: 137-138) of epistemic relativism were given. The impossibility of a metajustification of one of the set of standards prevented a rational debate between the two groups. This, however, did not lead to epistemic relativism. As Kusch puts it, "[a]ll forms of epistemic relativism commit themselves to the view that it is impossible to show in neutral, non-question begging way that one 'epistemic system' (...) is epistemically superior to (all) others." (Kusch, 2017: 4687). These, however, are the philosophical conditions only. Their presence provides only the necessary but not the sufficient conditions of a local relativism.

The epistemic standards are "*fundamental commitments* which are (...) *immune to rational evaluation*" (Pritchard, 2016: 66, my italics) hence their endorsement presupposes a culture, tradition, customs or socialisation. This seems to match the main argument for the extension of cultural relativism onto science, namely that "... science is not above culture; it is part of culture" (Livingstone, 2003: 180). This seems to entail relativism since as there are different human cultures there must be scientific traditions as well that differ from each other in their entrenched commitments to various epistemic systems. I argue, however, that science is a special kind of culture that does not tolerate diverse commitments at the same time.

The reason is that contemporary Western science consists of disciplines that have been getting unified since the 17th century on. An integral part of the disciplinary cultures this historical process has brought about is the principle that no synchronous alternative epistemic systems are tolerated. The unity is not established or preserved by reference to neutral experience and universal reason: it is consensual, historically changing and is confined to disciplines. But it creates shared commitments.

The unification I refer to is certainly, not that complete one the neopositivists dreamt about. Their reductionism, verificationism, and methodologism are not part of the project. It is acknowledged that reality may be too complex to approach every region of it by the same methods. Further, it is not imagined that every research groups should take the same particular metaphysical assumptions for granted and it is not hoped that scientific research will eventually produce a grand unified theory of the world (Dupré, 1993; Cartwright, 1999.). The unification I have in mind concerns the epistemic systems (Seidel, 2014; Kusch, 2017) or methodological assumptions (Longino, 2002: 184-189) that select and evaluate evidence and assess scientific statements. This unification is not based on philosophical criteria, it is rather a *historical process* that brings about "arational hinge commitments" (Pritchard, 2016: 89-103) confined to *disciplines*. The N-ray case is revealing because it makes visible how, despite lacking some higher-level epistemological principle, the majority of the experimental physics community defends the epistemic unity of their discipline by imposing their uniform standards of objectivity upon a deviant minority.

Spatial – local

The unification of the disciplines is a historical process entailed by the development of communication that creates place-independent communities. It seems natural to think that local scientific communities come about from the unification of individual efforts. People of the same geographical region who are beset with similar problems compare the ideas and methods they individually hit upon, select the best ones, complete, correct, reinforce or refute each other's views. Thus individual researchers get unified into local epistemic communities kept together by personal relations.

When spatialists describe science they seem to have such communities in mind: "science is always an ancient Chinese, a medieval Islamic, an early modern English, a Renaissance French, a

Jeffersonian American, an Enlightenment Scottish thing..." (Livingstone, 2003: 13). When, however, they form their general thesis about the localist nature of science they assume, without offering any reason, that the integration process stopped at the level of the local communities. Why would it be so? Certainly, there were contingent practical reasons for the existence of isolated local communities until communication among distant regions was technically difficult and rare. When, however, the facilities of travel, correspondence and publication created dense communicational connections among physically remote communities the process that brought them about simply went on. Urged by curiosity, the spirit of competition, the quest for learning, etc. people doing philosophy of nature in Europe tended to get in touch in an increasing measure from the 17th century on. Correspondence, visits, public experiments, journals, learned societies and academies connected these people irrespective of the geographical place they happened to live in. Invisible colleges emerged that, as described by Price and Crane, are loose, informal assemblages of people, held together by *intellectual proximity*: regular communication, exchange of preprints, conferences, visits, sameness of the literature read etc.

This spontaneously emerging epistemic homogeneity became institutionalised in the 20th century when a worldwide institutional system (universities, academies, learned societies, peer review system etc.) was developed (Drori et al., 2003; Schofer, 2004). This system takes care of the selection and preservation of the epistemic standards of science by:

a) A standardised *knowledge-transmission system* that ensures uniform cognitive socialization.
b) An *artificial virtual space for constant communication:* journals, conferences, workshops etc.
c) A c*onstant migration* of persons. Scholarships, visiting scientists, workshops, conferences, summer schools etc. keep up dynamic personal contacts, blend ideas and practices.
d) The strict separation of the *local contexts of discovery* and the *global context of justification* ensures that the locally embraced results have to gain accreditation from the whole disciplinary community, therefore justification must observe the actually endorsed non-local criteria.

Thus, despite being unable to offer philosophical arguments for the unity of science as such, we can observe a historical process and an institutional system that bring about the epistemic unity of the researchers dealing with the same problems. Ironically, despite emphasizing that "a whole body of recent empirical and theoretical work" shows "the local, situated and embedded nature of science" (Shapin, 1998: 6), spatialists tend to overlook the historical and institutional homogenising tendencies. The *historians* of science are certainly justified in their focusing "on the *local institutional setting of science* ... and on the *particularities of the practice* that characterise it" (Golinski, 1998: 55). This attitude, however, became obsolete with the emergence of disciplinary communities with which cultural relativism ceased to coincide with spatialism in science. Thanks to the communicational connections, it was no longer important where scientists were geographically located. What made them an epistemic community was that their practices were entrenched in non-local institutions that made them conceptually, methodologically and epistemologically united.

Before outlining how this change effected non-geographical localism in science it will be useful to distinguish between three senses of the adjective "local" in epistemic contexts:

LOC1 The most fundamental sense of localism is *non-absolutism*: "Rational evaluation is ... an essentially local activity, one that always take place relative to arational hinge commitments" Pritchard 2016, 103). Pritchard calls this the "essential locality of rational evaluation". This locality is strictly epistemic and means that all epistemic evaluation presupposes some epistemic system.

LOC2 Cultural relativism was first motivated by the *spatial* meaning of localism. Norms are developed by geographically isolated communities hence the beliefs' credibility is provided by "specific *local causes"* (Barnes and Bloor, 1982: 23).

LOC3 Finally, localism is contrasted with "*global* relativism" that includes every sphere of

> culture (moral, cognitive, aesthetic etc.). Contrasted with this, local relativism means that one is relativist "with respect to some designated domains" (Krausz, 2011: 74) where more than one genuinely different epistemic system prevail.

With this conceptual articulation in hand we can refine the general statement that the only "characteristics all knowledge systems share is localness" (Watson-Verran and Turnbull, 1995: 116). It is especially important if "localness" is applied to science. Scientific disciplines are obviously local in the first sense: theories and statements cannot be assessed "simpliciter" (McKenna, 2017: 172) only in the context of some epistemic system. As indicated, science ceased to be local in the second, spatialist sense by the emergence of the disciplinary communities. In what follows, I intend to argue that scientific disciplines are not local in the third sense either, i.e. the concretised version of the above statement, namely that the "fundamental characteristic of *scientific knowledge* is its localness" (Turnbull, 1996: 6) does not apply to the scientific disciplines. My claim is that different epistemic standards may turn up in disciplinary communities, but their existence is transitory only, the synchronous unity of the fundamental commitments is preserved.

The no-tolerance principle in disciplines

Why I claim that the differences of standards are transitory in disciplines is shown by the N-ray case. At first glance the incompatible experimental outcomes back the localist claim that it is "useful to talk about the *difference in cultures* between the interacting groups that participate in physics" (Galison, 1997b: 669, my italics). However, the cruelty of the eradication of this discrepancy is a clear sign that the culture of science does not tolerate synchronous subcultures in the same field for long.

Why science is intolerant in this respect is understandable from the fact that its main characteristic since the emergence of disciplinary communities is *non-individuality*: knowledge is manufactured by a series of *interactions* like collaboration, critique, adjudication, making use of data produced by others, evaluation of claims to decide about publication, grants, jobs etc. The consequence of the constant interactions is the emergence of a shared stock of ideas, a conventional set of standards, concepts, authorities, common practices, and standard techniques. Certainly, science is a complicated epistemic activity, thus the consensus is never complete, groups may establish local standards, schools and local traditions may come about. Evidence may be insufficient to decide about metaphysical assumptions or about the effectiveness of a methodological innovation. Rival hypotheses and alternative methods may coexist for a while, conceptual, and methodological innovations are suggested etc. However, the necessary diversification of expertise, the effectiveness of the distribution of cognitive labour, the enormous quantities of data, huge instruments, escalating costs of research, and coordinated research programmes of different laboratories make constant communication and interaction inevitable among the scientists pursuing the same discipline.

Division of labour, *collaboration* and *competition* are the conditions of producing the best humanly possible knowledge of reality, therefore science cannot consent to incompatible views justified by local commitments in the long run. Disciplinary communities cannot acquiesce in domain relativism because the incompatibility of standards prevents cooperation and competition that are fundamental constituents of this culture and are the preconditions of its cognitive success. The researchers who want to rely on or criticise the theories and data produced by other research teams, or want their own results be used by others, have to adjust themselves to shared non-local standards. That is why one of the principal norms of this culture is to seek homogeneity of fundamental cognitive principles of rationality, justification, and objectivity. Variations are kept under constant pressure and in the long run they are fitted in the general patterns of interaction at any cost: "… one can understand investigative, or scientific, communities as constituted around selections of substantive and methodological assumptions. *These selections are a function of both the aims of research and inherited tradition*"

(Longino, 2002: 186, my emphasis). We have seen that when the N-F community did not fit into the inherited tradition of mechanical objectivity it was forced to return to the discipline's "inherited tradition".

As I read it, the N-ray case opens a window on the historical period when the experimental physicists' unified disciplinary community emerged. Referring to the 19th century Britain scene, Livingstone could rightly say that "Bristol science, Manchester science, and Newcastle science are not the same as science in Bristol, science in Manchester, or science in Newcastle. The place-name adjectives in these designations attest to scientific practices that were constituted in different ways by different urban cultures" (Livingstone, 2003: 108). By the beginning of the 20th century, however, these cultural differences have disappeared from experimental physics. Physicists working in Baltimore, Breslau, or London discussed the results produced in Nancy as if they had been in the same town. The epistemic homogeneity of the discipline was considered so important that it was defended without seeking a fair mediation between the G–E and the local N-F sub-cultures. No trace of a Galisonian "trading zone", of an "intermediate domain in which procedures could be coordinated locally even where broader meanings clashed" (Galison, 1997a: 46) can be seen. No attempt was made at developing an "interactive expertise" by getting into concrete practical and communicative interactions with the competent local speakers (Collins, 2004). Instead, it was made abundantly clear that the decision about what qualified as "acceptable method," "reliable instrument," "confirming evidence", "proper experimentation", and "criteria of objectivity" was *kept under control by the majority of the disciplinary community,* and if it intended to be part of this culture, the a local community had to go by them. And it did.

The N-F community undoubtedly regarded itself as part of the same culture of experimental physics as the G–A community. Blondlot and his colleagues never claimed they pursued a different cognitive venture: they regarded their discovery as an addition to the then known kinds of radiation; they published their results in the common forums (journals, conferences) of this culture and their ambition was to have it accepted by the international community. Their results were produced and justified by the standard instruments and in the standard laboratories of the experimental physics, and when Wood asked for Blondlot's collaboration in checking his results, he was ready to receive him right away and agreed to conduct experiments together. After Wood's accusation that they broke the norms of experimentation, the N-F community did not put up a resistance. Not everybody went as far as J. Becquerel who, forgetting about his earlier experiments with the N-ray, suddenly realised that "the purely subjective method employed for testing the effects of the N-ray is antiscientific" (Nye, 1986: 74). Still, the majority tacitly withdrew and prudently changed their field of research. No one replied "we have our own tradition", "we use different standards" or "our claims are just as true as those of the G–A community". They readily admitted that they belonged to the overall culture of science and could not apply local standards.

Naturally, discovery in science is often induced by local factors like authorities, patterns, traditions, co-presence of like-minded people and instruments existing in a local community (Henke and Gieryn, 2008). Thus we can say that "knowledge is *constructed* in specifically designed and enclosed space" (Golinski, 1998: 98). Cases like the N-ray or cold fusion, however, clearly show that after the emergence of the disciplinary community it is no longer true that "place matters in the way scientific claims come to be *regarded as true,* in how theories are *established* and *justified*" (Livingstone, 2003: 13, my italics). In vain established the N-F community a firm local consensus, in vain were Blondlot's experiments replicated there, this was far from scientific justification. The accreditation process took place in the abstract space created by *Nature*, *Lancet*, *Scientific American, Revue Scientifique*, *Physicalische Zeitschrift,* and other journals and conferences, and was executed by the broad community of experimental physicists.

There was a clear sign of the disciplinary unity of the experimental physics. Lummer remarked in the *Berliner Ophtamologische Gesellschaft*'s assembly in February 1904 that "up to now only the French researchers have seen those rays" (Lummer, 1904a: 280). A bit later an anonym author in the *Scientific American* put it more sharply: "... why English and German scientists have been uniformly unsuccessful in detecting the strange emanations ... and why French physicists ... furnish more convincing proof of their existence every day"? (*Scientific American*, 1904: 434). The realisation of the *locality of justification* could have suggested the presence of "paradigms that pass each other like ships in the night" (Galison, 1997a: 13), however, the thought that a group of physicists had created an autonomous local tradition with particular methods of *justification* did not even cross anybody's mind. On the contrary, the very fact that somebody was sent to Blondlot's laboratory to check the experiment on the spot, and that Wood expected "*peculiar conditions*" in the laboratory (Wood, 1904: A 6–8, my italics), made manifest the suspicion that it was not a "placeless place" (Kohler, 2002) as proper science would have required. Surfacing the non-replicability by standard methods caused a crisis and was considered as "one of those scientific *anomalies* for which *no adequate explanation can ever be offered*" (*Scientific American*, 1904: 434, my emphasis). The understanding of the situation as an unexplainable "anomaly" instead of a difference between epistemically equal, domain specific "local traditions", clearly shows that at the beginning of the 20th century experimental physics was already conceived as a unified discipline with shared fundamental commitments. And the N-ray hypothesis was treated accordingly.

We can theoretically (practically often not) accept that there are multiple ways of living, customs, morals, religious and political views, schools of art, i.e. that broadly dissimilar lay cultures can exist next to each other, without even attempting to reach agreement concerning vital questions. Not so in particular scientific disciplines whose ideal aim is the true or at least the instrumentally most effective description, the deepest possible understanding and the most comprehensive explanation of nature. The only chance to achieve it is interaction that is made impossible by the plurality of epistemic standards. Epistemic relativism is irreconcilable with the collaborative and competitive practice of disciplines therefore the diversity of standards is tolerated only as long as it is inevitable. The members of this culture are socialized to make every effort to unify their epistemic norms to make possible cooperation, division of intellectual labour and critique. The sub-communities that want to have their results accredited have to take part in the selection process that in the long run lets stand only one epistemic system at a time for a disciplinary community. That happened in the N-ray case. Despite their mutual understanding the two communities continued to maintain their contradictory views about the existence of N-ray. As the epistemic standards they used were not rationally discussable, the unification did not happen by argumentation: the majority imposed its standards on the local community by power to restore the unity of the discipline.

Conclusion: the one-dimensionality of scientific relativism

According to the general definition of epistemic relativism, "knowledge is relative ... because different cultures, societies, epochs, etc. accept different sets of background principles, criteria, and/or standards of evaluation for knowledge-claims, and there is no neutral way of choosing between these alternative sets of standards" (Siegel, 2011: 201). Like the definitions of relativism in general, this one as well focuses on the impossibility of choosing rationally among the possible alternative epistemic systems. Epistemic relativists regularly assume that since norms and criteria solidify in communal processes that bring about customs and traditions *historical* and *cultural* relativism are on a par. The process is the same no matter that the commitments come about by the change of time, physical distance or by the formation of a particular school. Thus historical and cultural relativism is usually regarded different only in their "*emphasis on the diachronic* rather than the *synchronic* dimensions of the determinants of thought and action" (Baghramian, 2004: 6, my italics).

My argument was that in science this does not mean that one can pick any of the theoretically possible systems. Historians claiming that the history of science "is on the cusp of a transformation that is about to leave us with a growing number of local historiographies of science"

(Nappi, 2013: 103) seem right. Indeed, pessimistic induction suggests that the present methodological norms of physics may radically change in the future, hence *historical relativism* seems a well-founded phenomenon. However, the acknowledgment of the systematic "... relationships between thought and its social setting" (Ophir and Shapin, 1991: 9) does not entail *synchronous relativism* in the disciplines of science.

Disciplines does not establish a synchronous unity by the assumed universality of experience (protocol language) or by the inborn general norms of rationality, not even by the G–C-E framework. Here the unity is created by the necessary interactivity of the cognitive process and by the matching "social setting", namely by the institutions of science. Science is a practically and communicationally unified cognitive machinery that brings about synchronously *universal* epistemic systems in its disciplines. Thus it is not to be denied that social causes play a pivotal role in the assessment of scientific beliefs, and that "true" is replaced by "warranted" or "credible in a community" (Bloor, 1999: 84). But the "causes of belief" that elicit credibility (Bloor, 1999: 84) in science are local only in the sense of *epistemic non-absolutism* (LOC1), but not in the *spatialist* (LOC2) or *domain-relativist* (LOC3) sense.

In other words, relativism remains a legitimate historiographical norm, inevitable if one wants to escape presentism. We should, however realise that the examples spatialists come up with are examples of "*historical* geography" that do not prove at all that a "geography of *contemporary science*" should exist. No matter if they work in CERN, in Stanford, or in Tsukuba high energy physicists cooperate, discuss theoretical questions, share experimental data, and exchange experts without difficulty. This suggests that speaking specifically of scientific knowledge we should distinguish *two types of relativism*.

1) The cognitive culture we call "scientific" is a historical development, the validity of its standards is based on consensus and as such it can change substantially in time. Therefore *historical relativism* does apply to scientific cognition. The standard examples of the Aristotelian, Newtonian and Einsteinan physics, phlogiston chemistry etc. can be completed by the case of the N-ray: Wood refuted Blondlot's claim by showing that he failed the standards of mechanical objectivity. These standards themselves, however, had to be abandoned soon. In the first decades of the 20th century new instruments were introduced for observing beams, waves, sub-atomic particles, electric and magnetic fields: screens displaying fluorescent lights, cloud chambers showing the visible tracks of electrically charged particles, EEG for recording the electrical activity of the brain etc. These instruments showed flickering lights, shimmerings on screens, photos and charts with very complex and entangled patterns, whose discern and interpretation demanded trained eyes. The epistemically naive principle of "use objective instruments only" or "inborn natural sensational capacities ensure objectivity" did not work any longer. By the middle of the century it has become accepted that experimental practice, observation, language, and calculation alike involve a tacit dimension (Polányi, 1966). Thus a "trained-eye objectivity" came to replace the old mechanical one (Daston and Galison, 2007: 329).[3]
2) Despite being a kind of culture, contemporary science cannot be subsumed under *cultural relativism* (LOC2 or LOC3). It is exceptional among the cultures, not because of its exceptional methodology or epistemic excellence, simply because it succeeded in establishing universal epistemic systems in several of its disciplines. To put it bluntly, Hopi conception of time, Maori epistemology, African traditional cosmologies, Zande witchcraft and the like cannot be regarded as alternatives to the scientific conceptions. Such examples demonstrate epistemic relativism only if the validity of their separate justificatory frameworks are acknowledged as "scientific". But if they would, then they should not be exempted from the selective pressure prevailing in science and this would result in ceasing the alternative conceptions.

Thanks to the special social setting of the culture of science, all the reasons for relativism revealed by SSK (social legitimation and historical change of the norms of rationality; the symmetry thesis; the empirical flexibility of interpretations; social constructivism) can be endorsed without accepting synchronous relativism. This certainly does not mean that such communities obtain an absolute perspective: science remains a human culture, but at least not fragmented into synchronous local points of view, its relativism has only one dimension.

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Notes

1 The following summary is based on Nye 1980 and 1986, and Klotz 1980 and 1986.

2 I cite the two versions of Wood's paper from the appendix attached to Ashmore 1993 because there the lines are numbered. The text denoted by 'A' was originally published in *Nature* 1904; the one marked by 'B' in Seabrook 1941. The numbers indicate the lines of the respective text-versions published by Ashmore.

3 As an illustration of the change, have a glimpse at the description how the neurologists learn to see an EEG-chart:
"stage I: nothing makes any sense, stage II: you think you understand but you see abnormalities everywhere, stage III: you gain more hindsight. You recognize a spike but wonder if it is actually significant, stage IV: you are finally able to form your opinion, even if it is different from your teacher's. This last stage is the sign you have matured. You have acquired enough experience to have your own opinion and to discuss an EEG (Crespel and Gélisse, 2005: 13)".

Programming Visuals, Visualising Programs

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Abstract

This article examines the role of visualisations in astrophysics programming work, showing that visualisations are not only outputs for those producing them, but can help those developing them understand how to do their work. Studies of visualization in programming have mainly been of social and cultural factors influencing scientific research. We concentrate on the material aspects of scientific work, as of interest in their own right and on methodological grounds (since capturing the material practices of computer screen-work is an underexplored area). Using a 'video-aided ethnographic' method we analyse an episode of computational astrophysics involving the use of the Python programming language. We identify a selection of activities comprising the screen work of an astrophysics researcher to unpack how those activities contribute to the production of scientific knowledge.

Keywords: Astrophysics, programming, visualisations, video-aided ethnography

Programming visuals, visualising programs

The spread of computing throughout social life has impacted the natural sciences such that the use of computers to simulate phenomena or automate the gathering and analysis of data has become an alternative to physical data collection and experiments (for studies of computational programming work see Button and Sharrock, 1994, 1995, 1996; Knuuttila, 2006; Knuuttila et al., 2006; Knuuttila and Boon, 2011; Martin and Rooksby, 2006; Merz, 2006; Rooksby et al., 2006). Using video-recordings of a researcher testing out a program to convert electronic input relayed from an orbital telescope into a set of images enabling the identification of gravitational lenses, we explore an assortment of problems that the researcher meets in trying to ensure that his program is dependably categorising these galactic images.

Our attention to the visual features of computation reflects a growing interest in how scientists engage with visualisations (Amann and Knorr Cetina, 1990; Burri and Dumit, 2008; Carusi et al., 2010; Lynch, 2011; Messeri, 2017). Our work is aligned with studies exploring the material work of dealing with digital images and visual data in scientific research (Alač, 2011; Carusi, 2008, 2011; Coopmans, 2006, 2011; Daipha, 2010; Hoeppe, 2012, 2014; Sormani, 2014; Spencer, 2012; Vertesi, 2012).[1] We focus on the practices involved in visualisation-based and visually-oriented research work, and how those practices intertwine with the wider scientific knowledge and context of that work. Alač notes of this:

> The materiality of the scientific data – their digital character – allows the practitioners to understand what they are working with as something that is mathematical, while it, at the same time, moves and needs to be rotated, squished and squashed. (Alač, 2011: 145)

Images and visualisations are *used* by the practitioners that generate them as part of their routine work, in such a way that "scientific visuals do not *represent* knowledge and problem solving, but are a part of such processes" (Alač, 2011: 162). Our approach to visual-work is grounded in Coulter and Parsons' (1990: 255) claim that "'seeing' is akin to an *achievement* and is not any sort of activity, process, or undertaking". Therefore we attend to the various activities that generate and construe an adequate 'seeing' of an astronomical phenomenon – the 'searching for', the 'inspecting', the 'observing', etc – on the part of one astrophysics researcher, to show more clearly what constitutes an achievement of this kind.

We begin by exploring two relevant literary bodies around the roles of computing in scientific research work and the underdevelopment of social research attending to its material practices, outlining a series of methodological concerns around capturing and analysing 'independently-executed' computer screen work. After depicting the context of the activities that form our topic, we analyse our data along six themes capturing a variety of material practices involved in the visual-work of scientific research. These themes are: making code visual; highlighting for visibility; finding through searching; finding visual utility in images; arranging for comparison, and, finally, visual diagnostics.

Background: Science and programming

As computer tools have become increasingly prominent in routine scientific work, so they have become increasingly pertinent to social studies of science, which focus on the constructing and constraining functions of interaction in an era of computational and digital science. There are two related issues in this body of work: firstly, distinctions between 'science' and 'computing' work, and secondly, the neglect of the material work of using computers to do science (relative to a focus on communal and collaborative elements).

Several studies (e.g. Agar, 2006; Bijker et al, 2016; Bruun and Sierla, 2008; Götschel, 2011; Hine, 2006; Larivière et al, 2016; Louvel, 2012; Pettersson, 2011; Mulinari et al., 2015; Rall, 2006; Sundberg, 2010; Voskuhl, 2004) present computer-aided scientific projects as comprising distinct expertises: the practical hands-on skills of programmers and the conceptual/theoretical knowledge of the scientist, combined through collaboration. Taking a selection of such studies as exemplars, this theme is apparent in Agar's claim that historically, "one difference that [the introduction of] computers made to science was deepening the division of labour – and expanding one side of the division, professional computing services" (Agar, 2006: 900). Similarly, Hine argues that:

> This division of labour [between science/knowledge and computing/programming] is conventional in [the] development of information systems. The database developer is responsible for identifying 'user requirements', and is expected to get to know users and find out what their needs are. (Hine, 2006: 281).

On the 'shop floor', scientific projects and the problem-solving work they involve are depicted as presenting the cultural challenge of combining skills and expertise by managing group work to integrate members' different capabilities. This is exemplified by the following quotations:

> This particular problem had nothing to do with acoustics or digital-signal processing. Rather, it was a problem that required those mystical skills which enable 'computer wizards' to rescue and manipulate their machines from the most hopeless situations...My informants would refer to those who were capable of successfully manipulating computers as being 'wizards' who always knew a 'trick', an obscure command, or another solution to a problem. (Voskuhl, 2004: 405)

> Feynman[2] is everywhere in this story...Against the odds, as the problems increased in size and complexity, his team continued to improve [in their ability to provide the calculative power necessary for the project]. (Rall, 2006: 955)

What these two accounts (and those of Agar, 2006; Bruun and Sierla, 2008; and Hine, 2006) convey is a sense of scientific knowledge as achieved through integrating disparate skills and understandings into a socially-constructed unified (though distributed) solution. However, where Voskuhl (2004) refers to the mystical skills of 'computer wizards' as tricks of programming, our interest falls upon what such 'tricks' practically consist of, and how they might constitute the practical work of *doing* acoustics and/or digital-signal processing with computers. Similarly, if Feynman is everywhere in Rall's (2006) story it is because Rall is narrating Feynman's endeavours as a team manager, whereas we would be interested in the story Rall *doesn't* tell of Feynman's role as a physicist.[3]

Some researchers seeking to investigate the organisation of scientific knowledge as a topic completely separable from the content of scientific knowledge; e.g. in Sundberg's (2010: 39) analysis of 'simulation code collectives' – groups whose collective and cultural properties implicate "the definition and control of simulation code use and development", whilst others extend STS' remit to include a singular concern with the cultural aspects of research. Pettersson (2011: 47) for instance aims explicitly "to analyse experimental practices among plasma physicists as gender creating processes with perspectives from masculinity studies", Götschel (2011) studies how physics has been used to reinforce misogyny, Louvel (2012) investigates the industrialisation of doctoral scientific work as representative of a grand shift in what constitutes scientific work, and Mulinari et al. (2015: 55) critique the "uneven, partial and sometimes even contradictory" neoliberal social and political factors surrounding stem cell research.

We do not dispute the findings of these studies – rather, we suggest that their accounts of 'knowledge production' in scientific research are partial, inasmuch as they do not capture the practical activities through which scientists produce knowledge in their labs (or at their computers). Thus the aforementioned researchers preclude a demonstration of the ways in which the social and cultural factors that form their topic enter into the day-to-day doings of scientific research as knowledge production. Their focus comes at the expense of acknowledging the material practices of *doing* scientific work, and how those practices execute scientific tasks – for example, the hands-on nature of experimentation in neurobiology (Lynch, 1985), or the aspects of embodiment involved in understanding how a Mars Rover moves and sees (Vertesi, 2012, 2015), or in the case of the present paper, leveraging computer programming skills to explore gravitational lensing as an astrophysical phenomenon. We aim to reinforce a focus on the 'content' of scientific knowledge (and the scientific business of making analysable records of it), by shifting focus from surrounding social and cultural factors and towards the practical activities comprising the execution of the work. Though we acknowledge the wider social context in which one astrophysics researcher's work is embedded (and account for this in detail below), the purpose of this paper is to concentrate more intently on the 'independently executed' aspects of scientific work as the underexplored counterpart to the great wealth of studies which focus more on the directly collaborative and/or interactional activities of scientists.

Methods

Our choice to focus on the material aspects of scientific programming is partly methodological – as a hitherto underdeveloped site of research, it is worth exploring what sorts of activity scientific programming might comprise even if only to elucidate on how such things might be captured for future social research. The neglect of the material practices of computational scientific work has been attributed by Bruun and Sierla (2008) to the difficulties in locating and capturing such activities. As they note:

> Recordings of real-time actions and interactions of the project members would have contributed to an in-depth understanding of the circumstances through which knowledge networking solutions were produced. This could have been accomplished through video-recording, but many of the interactions, decisions and deliberations in research projects were difficult to capture in real time, even with a video camera, because they were not fixed in time and space…What is more, in software development much of the crucial interaction occurs when engineers browse, study, modify and integrate artefacts that have been developed by colleagues. These activities dominate the experience of most software engineers and constrain many of their decisions, but there is little overt, bodily behaviour to be observed: only mouse and keyboard use. (Bruun and Sierla, 2008: 140)

Bruun and Sierla (2008) have two complaints: firstly, that people won't stand still long enough for their interactions to be videoed, and secondly, that what does take place in a static setting – mouse and keyboard use – is not of any interest. However, they thereby overlook the sense in which the operational work of mouse and keyboard usage is embedded within scientific knowledge. It is precisely this arena involving little overt bodily behaviour in which much of the work of programming-for-a-scientific-project takes place, and it is the goings on within this arena that forms the focus of the research presented here.

It is not our claim that screen-work – work performed and achieved using the visual resources available within a computer screen – is an asocial endeavor. Indeed, screen-work is sometimes a thoroughly collaborative affair, as in the case of traders in the foreign exchange market dependent on information appearing on-screen in Knorr Cetina's (2003) examination of the role of 'scopic media' in their work, or in Vertesi's (2012, 2015) work on the role of images and image construction across the different disciplinary teams collaborating on the Mars Exploration Rover project. However, in the cases analysed here, screen-work is done without much (if any) face-to-face or even remote (i.e. online) collaboration. That much scientific activity is collaborative does not exclude the fact that it can also be performed via solitary effort. We agree with Carusi's (2011: 332) claim that there is more to visualisation work than face-to-face interaction, and that "the sociality of visual practices – the fact of their being shared by communities – is not sufficient to account for what is seen through those practices".[4] This is evidenced in Vertesi's (2015) work which attends to the ways in which images pertinent to the Mars Exploration Rover project move between two types of setting: the collaborative team-based planning meetings and conference calls, and the desks and screens of individuals scientists. Vertesi's ethnography demonstrates that though the work of image construction is inevitably achieved through individual effort – mouse and keyboard usage (cf. Bruun and Sierla, 2008) – their efforts are designed and conducted *precisely so that* they feed into, and even *display*, the broader social and cultural context work of the Mars Exploration Rover project. Failing to acknowledge the movement of images between the two settings would entirely misrepresent what it is those individuals are doing, and their reasons for doing those things in the way they do. Just so with the astrophysics researcher whose work forms our subject – we explore the specific ways in which this occurs for our case-at-hand below.

For present purposes however, it is worth noting in a general sense that the social elements of the tasks of screen-work, at least for the astrophysics researcher whose work forms our subject, are visible in the work only in an asynchronous fashion. This is something captured by Button and Sharrock (1996) who characterise the annotating work of programmers, as holding a utility not for their current task but for future users and developers of their program. In an even more fundamental sense, the work of programming rests on the performance of other forms of interactivity which consist of irrevocably social elements – no more can there be a private programming language than there can be a private linguistic one (cf. Wittgenstein, 1974)! Yet there remains a degree to which certain episodes of project work are isolated from the communal scientific action that typically forms the subjects of sociological interest, as Vertesi (2012) notices of the embodied scientific work of remotely controlling the Mars Rover:

> During my fieldwork, I certainly witnessed situations in which such semiotic acts [embodied movements representing the physical hardware of the Mars Rover] were communicative in nature, in which a wheelie chair maneuver or a skilled twist of the elbow was a central articulation in the work of coordinating action at a distance. However, the vast majority of times I witnessed these gestures, *there was no one else in the room*. Most frequently, scientists, engineers, and technicians alike gestured in what were clearly formal, codified, standardized ways of enacting the Rover, but they did so entirely alone, speaking to mutually invisible interlocutors on a telecon line. (Vertesi, 2012: 402)

Similarly, the astrophysics researcher's work depicted in this paper may be understood as *independently executed* – work achieved in large part without guidance or consultation, though nonetheless embedded in a collaborative structure reliant on remote and asynchronous connection through infrastructure rather than face-to-face interactions.

In saying that the work is 'independently executed' we have the following in mind: (1) in relation to the gradual acquisition of professional competence, postgraduate researchers (such as HR, whose work we report) can be making the transition toward being able to work independently of close supervision and evaluation in carrying out a research task on their own behalf, (2) in relation to the task, whose execution does not depend on interaction with and contributions from others but can be carried out in (relative) solitude and (3) in relation to the division of labour within the project where the task at hand is self-contained and does not require connections to the several other comparable graduate projects that are contributing to the wider goals of the research group.

Video-aided ethnography

Methodologically, this presents a problem for a social study of science – what is to be found in a setting where nothing explicitly social seems to have happened? And what might constitute an appropriate method of capturing whatever work might be involved? We have used an analytic orientation that captures key features of the settings as they appear to those involved (i.e. astrophysics programmers). Our understandings of the setting rely on knowledge gained through fieldwork[5] as well as repeated viewings of the video. Drawing on ethnomethodology (Garfinkel, 1967) and associated video analysis techniques (Goodwin, 1994, 2001), our approach attempts to understand how the organization of the work at hand is displayed – made accountable – within the resources available on the computer screen where the work is sited. This is patent to the practitioner doing the work, it being his routine activity, but needs to be accommodated in sociological descriptions of that work. The adoption of a video-aided ethnographic method is designed to elicit access to the resources with which scientific researchers using computer technologies can achieve their work independently, and to examine what sociologists can draw from this seemingly arid environment.

This paper examines work from a larger project investigating the use of computerised technologies (typically, programming languages) in different sciences which combine research with training. The focus is on early-stage researchers doing project work toward the attainment of a postgraduate qualification. The focus on this stage in a research career facilitates the observation and understanding of the settings in question as exploratory endeavours in both scientific knowledge and method, both of which are actively topicalised by participants as part of their work. Furthermore, through that topicalising, both of those things are made available to social research, i.e. made 'accountable' for both participants and observers (Garfinkel, 1967).

Our approach to video collection and analysis draws from existing ethnomethodologically-informed studies (e.g. Alač, 2011;Sormani et al., 2017; Bezemer et al., 2011; Goodwin, 1994, 2001; Lindwall, 2008), and equips our video analytic work with a strongly contextualised understanding of the setting. Preparation was undertaken by the principal author to furnish the video analysis with the level of scientific competence necessary to understand the finer details of the activities at hand (see footnote five). It is difficult to quantify the time spent on preparation – preparatory work has continued throughout the analysis and presentation of the research, each iteration prompting more 'preparation' to understand previously unnoticed features of the video. Recording the video took much less time – approximately twenty hours over several days.

Results

Context of the Study

We examine one astrophysics researcher's activities over one working day, as he attends to a problem in developing a program for automatically identifying instances of the astronomical phenomenon of gravitational lensing. The researcher in question (HR) was a postgraduate student, with an undergraduate degree in physics incorporating the learning of programming languages in addition to classes in more conceptual topics (i.e. fluid dynamics, quantum mechanics, stellar evolution, etc). HR was working on his dissertation within a research group consisting of 15 other postgraduates, all under supervision by a professor of astronomy. The projects undertaken by each 'team' member were topically diverse and coordinated by their shared supervisor (who had developed each of their projects to feed into his ongoing research interests and projects). The projects underway at this time were typically designed to address technical and/or procedural research questions – the relative 'mundanities' of astronomical research which may not promise discoveries in the sense of finding and explaining new phenomena or objects, but which address the requirements for *doing* discovery work.

Returning to the ways in which HR's 'independently executed' work is conducted within a broader scientific context, we note that this is evidenced most clearly in two ways. First, that HR's position as a postgraduate researcher, working under a supervisor who manages a thematically-organised team of postgraduate researchers, places him as a cog in a grander machine. In this sense, HR's work is inherently integrated with other researchers working under his supervisor, as well as with the supervisor and their colleagues (who are vested in the success of postgraduate projects to feed into their own research). Second, and related, the code and images HR works with are *designed* to be used and viewed by others. His work (described below) is to produce a *technique* which can be replicated and applied in other scientific contexts and by other scientists. Hence, the value of HR's code and visualisations is, and can only possibly be, evaluated on the basis of their contribution to other scientific efforts. Taken in this way, the problems that HR encounters in his work (some of which we outline below) not only obstruct the successful completion of a postgraduate dissertation, but present difficulties in terms of the capacity for the work to be used by others in the scientific community. However, it is worth reiterating that for both these reasons, constant face-to-face coordination is not essential to the undertaking of HR's project, even despite its inherent connectedness with other scientists' work. HR's scientific activities are social, without co-present collaborators at the time of their undertaking.

Turning now to the specifics of HR's project, we note that his project was to investigate the potential for an automated computational method of gravitational lens detection to displace the non-automated/time-consuming practice of identifying lenses solely 'by eye'. HR's work was designed to be achievable through his independent research activities – having been given the project brief and some initial suggestions as 'jumping-off points', HR was expected (by his supervisor and by the design of his project as a postgraduate dissertation) to develop and deploy the necessary skills to complete the work individually and without need for supervisory guidance. It was HR's sole responsibility to learn how to see and read features of his visualisations, grounded in his existing programming and astrophysics learning. Despite HR's project involving writing a bespoke program, this work was conducted using widely available and 'off-the-shelf' tools which are simultaneously task-specific and all-purpose, consisting of a freely-available dataset (see below), a standard laptop computer, a programming language (Python), several freely-available Python libraries providing functionality relevant to handling numbers and images within Python, and a text editor interface within which the programming language could be developed.

HR's method to identify gravitational lenses was to find, by looking at the images on the screen, two peaks of radiation emission relating to each stellar object in each of the images of his 2148-strong dataset – his data consisted of 537 possible lensing events, each of which had 4 images describing a different EM (electromagnetic) radiation profile. This information was used to ascertain if there was a visible (to HR) distortion of the radiation emitted by each object and from that decide if the image represents a gravitational lens.[6] HR's data was drawn from the Sloan Digital Sky Survey (2013), a large database of images available to scientists and the general public. The SDSS is a long-running data collection enterprise using a dedicated telescope at Apache Point Observatory (New Mexico, USA) to collect astronomical images for a variety of purposes. HR had acquired, via his supervisor, a curated subset of an SDSS dataset, containing candidate images of gravitational lenses, and it is these which HR is attempting to classify so as to develop an automated classification procedure.

The video shows HR working on a basic program he had already written which, so far, classified with a maximum 80% accuracy (this being determined by the computer's inability to produce any kind of result for around 20% of the images). To improve (and more systematically measure) the program's accuracy, HR worked on developing a manual input system which would provide his algorithmic technique with information about the coordinates of the two radiation peaks on an image, to develop his program's capacity to locate radiation peaks on the images it processed. The reasoning it thus: while some images may contain anomalies which confuse the computer's ability to decide, if the program is told which of the two peaks are relevant (and ignore all others) then it should yield better decisions about whether an image represents a lenses.

Having decided how to improve his image-classification program, HR wrote a script to allow a viewing of each of the 2148 images in turn and entry of the coordinates locating the peaks in the image. The usage of this script – effectively a front-end for contributing new metadata to each image by cycling through the corpus and appending the location by two mouse clicks – is captured on video. The process can be boiled down to the following (ideal) steps: he inspects the image to see if the position of the peaks is obvious (as is the case in Figure 1, in which there are two clear peaks with a clear lensing interaction between them). For more ambiguous cases, HR can use other images of the same galactic system in other wavelengths to cross-check against the image being worked on (see Figure 2 – also note the sub-display which magnifies the section

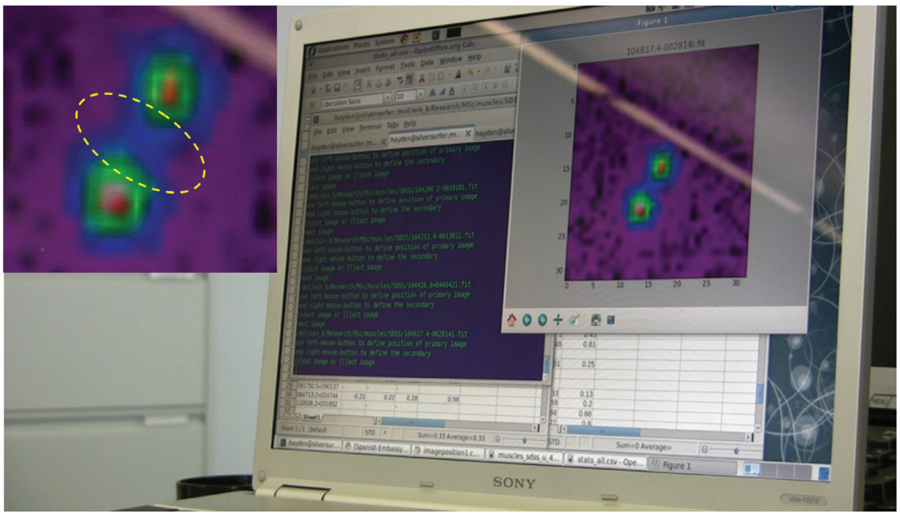


Figure 1. A 'good' lens with a clear lensing interaction (highlighted).

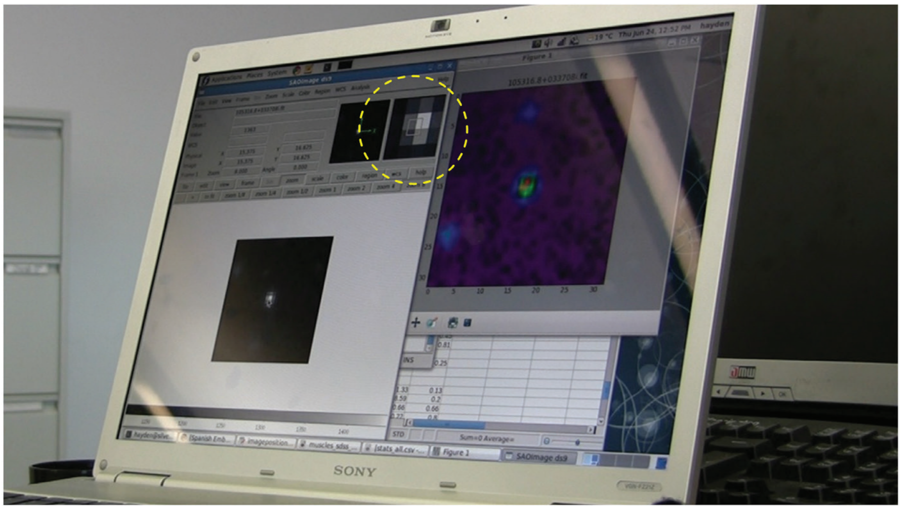


Figure 2. Cross-checking in another wavelength.

around the cursor). Having identified the peaks 'by eye', HR can record the location of the first peak by clicking on it with the left mouse button, the second peak with the right mouse button, then keystroke [n] to move on to the next image and repeat the process. Various elements of 'looking for' and 'finding' activities bear on HR's work.

Making code visual

Code is scripted text providing a list of operations (and the instruction to run them) collated under the larger structure of a program, and is written in a dedicated programming language (i.e. a software package for mathematical and computational processing) which a computer can implement. However, it is vital that not only computers but *programmers* can read and understand code, and as Davis and Hersh (1981) note of the work of mathematics (which has a direct relationship to the work of programming in a multitude of ways):

> The layman might get the idea that a skilful mathematician can sight-read a page of mathematics in the way that Liszt sight-read a page of difficult piano music. This is rarely the case. The absorption of a page of mathematics on the part of the professional is often a slow, tedious, and painstaking process. (Davis and Hersh, 1981: 281)

Familiarity and skill with a programming language is often essential to absorbing the vast amounts of code making up complex programs, but as Button and Sharrock (1995) note, the visual organisation of the code is crucial to making explicit the specific reasons as to why it might be structured in one way and not another. One method by which programmers enable understandings of their code is through comments. Comments never form a functioning part of the program; their presence does not affect the program. However, their use is common, and not only for documentation to guide future users.

According to Button and Sharrock (1996), some programmers see documentation as an annoyance that is irrelevant to the 'real' task of getting a program to work. In contrast, HR's comments are for his own use in navigating his program, highlighting their dual functions of organizing and pathfinding. Though the program is inherently structured for the purposes of machine readability – code always executes from line 1 down the page (though this may also incorporate functions and loops instructing the program to return to a previous line) – the programmer has to organise and notate this structure for human readability. As Spencer notes:

> Scientific software is an intricate labyrinth, one whose construction and navigation are accomplished by one and the same movement. (Spencer, 2012: 99)

To elucidate this aspect of programming, we examine HR's division of his code into separate sections (to mark points where one coding task becomes another) by making a border of blue[7] commented hash marks at the start and end of each section. Practically, this means that HR can easily search for specific sections of the program, relying on visual cues. HR also uses comments to label distinct coding tasks – visual tags that make the subsequent code more understandable. For instance, HR has a line appearing as follows:

```
#BEGINNING OF PARSELTONGUE[8] SCRIPT
```

This comment marks out the following code as other than typical Python language – since ParselTongue is different to Python it is useful for HR to remind himself to read the following code as pertaining to ParselTongue specifically (as opposed to Python generally); this provides clarity when it comes to reading, debugging and other tasks. However, comments are not just labels for code. Comments can also situate code as part of a process. For instance, HR has the following comment in his code:

```
#now mask out a few pixels around this peak position, to detect the second peak
```

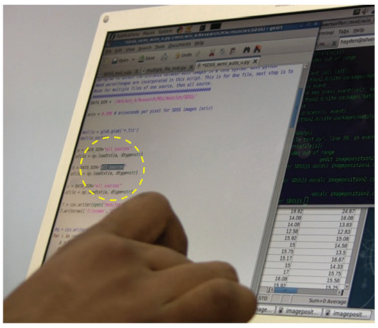
As Button and Sharrock (1995) note, the visual organisation of a program accounts for (i.e. makes visibly apparent) its own computational organisation, and comments such as this help HR to navigate through the master code screen by giving some indication of where in the code HR is if this is the section he's looking at. The comment above, by implication, relates to a section of code that must be after the section that deals with finding the *first* peak on an image. As such, if HR was to search for the specific code dealing with finding the first peak, the comment is a resource for ascertaining whether to look before or after (and also, how far before or after) the section of code currently on screen. HR enforces what Brown and Laurier (2005: 252) refer to in mobile-based cartography as a 'structure of places', which simultaneously locates the boundaries of entities within the structure (be they geographic areas or coding tasks) and renders the structure navigable. HR's practical work with comments also displays the utility of comments as navigational devices; signposts that point programmers in the right direction, helping them find the code they're searching for against otherwise visually undifferentiated lines of code.

Highlighting for visibility

HR's work also involves the integration of information from different sources (i.e. his database of manually inputted peak coordinates, image files, the master code screen, etc). HR practically transitions between windows by creating a temporary visibility arrangement through highlighting his current location in one window. In editing, HR added to a variable in the master code to integrate his new peak coordinates database into it. Effectively, he tells the computer not to look at the raw images, but to use his new coordinates databases to direct where it focuses with regard to the two peaks. This editing involves making two copies of the (linked) variables below:

```
a = DATA_DIR+'all_sources'
afile = np.loadtxt(a, dtype=str)
```

This copying of variables reflects a known feature of programming – there is 'a propensity towards re-use and economy in finding solutions rather than working out a solution from scratch' (Martin and Rooksby, 2006: 8). HR edits the copied versions of this variable by changing variable names and associated data (from 'a' to 'b' and 'a' to 'c', from 'afile' to 'bfile' and 'afile' to 'cfile' etc). Most crucially, the 'all_sources' script needs changing to reflect the filenames that HR wants the new variables to pull his manual input data from. To do this, HR must check the filenames of these databases, navigating temporarily away from the master code window to the database itself (which features the filename in its title bar). Prior to moving windows, HR highlights the 'all_sources' script in the new variable 'b', to make it stand out against the background of other code on-screen. HR then goes to the database to retrieve the filename and upon his return to the master code window, is able to use the highlight to reorient himself quickly and easily to the section of code that this filename should replace – the 'all_sources' script in variable 'b' is changed to 'imageposition1', and variable 'c' is changed to 'imageposition2' accord-



```
a = DATA_DIR+'all_sources'
afile = np.loadtxt(a, dtype=str)

b = DATA_DIR+'imageposition1'
bfile = np.loadtxt(b, dtype=str)

c = DATA_DIR+'imageposition2'
cfile = np.loadtxt(c, dtype=str)
```

Figure 3. Highlighting 'all sources', plus the finished edit of the section of code under development.

ingly (see Figure 3 below for HR doing the highlighting work, and a representation of the section of code after editing).

Here, highlighting is a quick, easy and temporary marker, which can serve as a placeholder as the code is developed (Button and Sharrock, 1995). HR's highlighting work is thus an example of a 'micro-practice' of screen- or scopic-work (cf. Alač, 2011; Knorr Cetina, 2003; Lynch and Edgerton, 1988) which is non-intrusive to the development of the program (in that it does not change the machine instructions) but can provide a visual emphasis on the script-to-be-changed to make it more 'findable' and thereby easily editable.

How to find through searching

Clearly, recoverability is a key issue for HR – he has to be able to find specific images, various databases (and particular information within them), filenames, sections of code, etc. Often, the location of the thing HR is searching for is not defined exactly and the best possible direction can only be phrased as 'somewhere within this database' or 'somewhere in this set of images'. Various practices of 'looking for' items such as these come up in HR's work, and these practices use resources available through HR's design of his working practices. As Martin and Rooksby (2006: 8) note of coding, "knowledge of the code base is knowledge of your way round it, how things might be connected and what the implications of changing a piece of code may be". This applies to HR's visualisation work in a variety of ways. For some sought after items, finding them can be simply entering a filename into a form, e.g., HR is searching for an image file in his database of peak coordinates, and, being able to refer to original image filename as it is on screen, he can copy this information into the 'find' form, keystroke [Enter], and the computer skips through the database directly to the desired filename (see Figure 4 below).

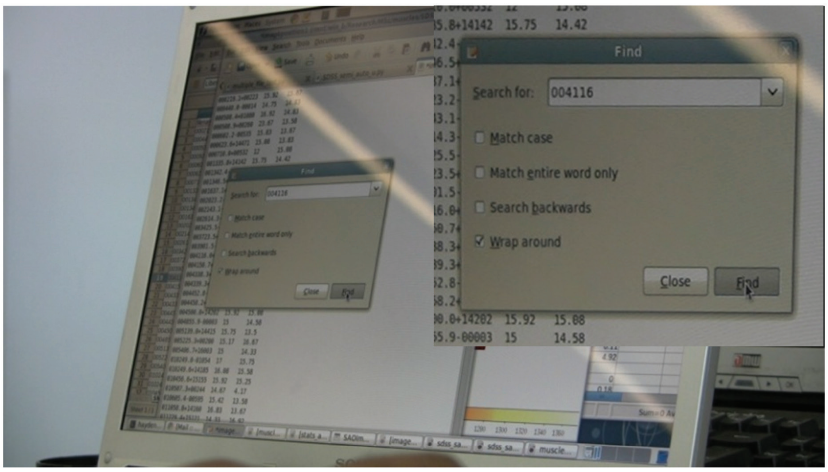
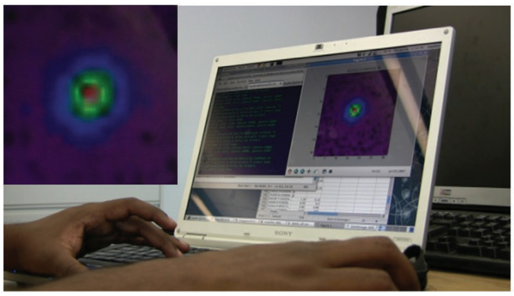


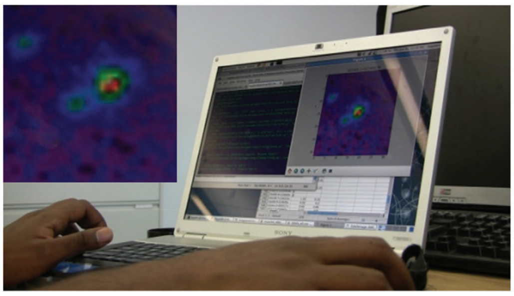
Figure 4. A 'find' menu.

In other cases a simple 'call-and-response' solution is unavailable – as Suchman (1994: 185) notes, "The problem is not simply that communicative troubles arise that do not arise in human communication, but rather that when the inevitable troubles do arise, there are not the same resources available for their detection and repair". In these cases, HR relies on other (visual) resources, e.g. HR makes a mistake in clicking on an image (image 1) and only realises this after moving to the next image (image 2) (see Figure 5 below). HR then needs to go back, re-examine image 1, delete the information mistakenly entered, then re-process the image). He does this by temporarily stepping out of the confines of the manual input/image processing work to recall it.

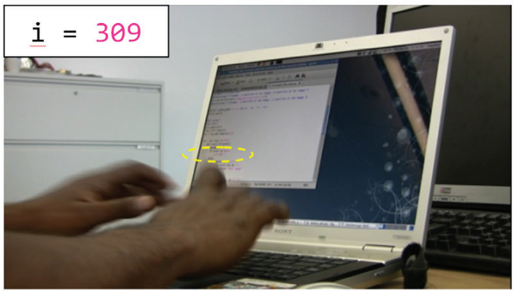
Working outside the program, HR has to call up images using the master code window. He has to start the manual input program again, but can choose at which point in the sequence of images to start: if the value of the variable 'i' is changed to 309 (as in the video), then the program calls the three hundred and ninth image in that set. HR chooses a value of 'i' that he thinks relates image 1 (i = 309), only to find that the image this value brings up is not the one he wants. He has to use other resources to ascertain the value of 'i' for the image he does want; having seen the unwanted image now on screen he can use its visual features to work out its likely position relative to image 1 (i = 309). The image on-screen at this point was the one after the image he needs to redo – he can



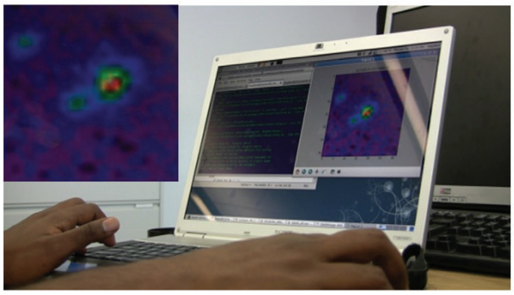
(a) HR (mistakenly) processes image 1.



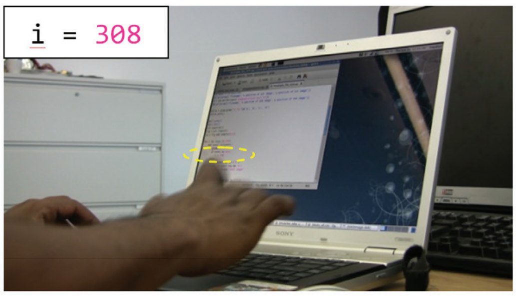
(b) Before he realises the mistake, he has moved on to image 2.



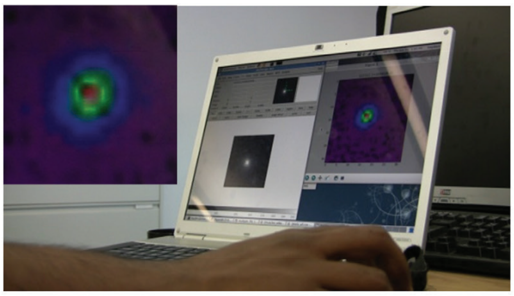
(c) He changes the value of 'i'.



(d) 'i' = 309 cues up image 2; the image *after* the one he wants.



(e) HR edits 'i' to cue up the image *before* `i = 309`...



(e) ...and this brings him back to image 1, which HR can then analyse and process properly.

Figure 5. Storyboard of events.

see image 2, but he wants to be able to see image 1 – and as such, HR can infer that the value of 'i' he actually requires to continue with his work is *one fewer* than 309 (i = 308). Here, HR has to draw on visual properties of the images on-screen (i.e. does it look like the one he wants? If not, can he recognise it? If so, can he pinpoint where in the sequence this unwanted image is and infer the relative position of the wanted image?) to tie specific images to their specific points in the process. As Goodwin (2001: 179) notes, "visual phenomena become meaningful through the way in which they help elaborate, and are elaborated by, a range of other semiotic fields" such as sequential organization, and by relying on various identifiable visual properties of the things he is searching for, HR is able to draw on a set of resources that makes his working with visualisations achievable.

Finding visual utility in images

HR's program is meant to distinguish between gravitational lensing systems and other non-lens objects, given an input of images of those objects in one or more wavelengths. At this point in HR's work the program is in the process of being developed; its capacity to do this consistently is therefore in question. As Lynch notes of his own work on biology lab science, 'artifacts' – "moments in the work, where the ordinary transitivity of practices was a confounding issue" (Lynch, 1985: 84) – "were not collected and analyzed in lab research, but 'fell out' as occasioned troubles in 'visibility' or 'interpretation'" (Lynch, 1985: 89). However, for HR, the possibility of artifacts is more expected given the uncertainty around the program's ability to perform classifications. HR is mindful of such artifacts appearing in his results as questions-that-have-yet-to-be-addressed – are the images the program identifies as lenses *actually* lenses? Are the other objects it identifies as non-lenses *actually* non-lenses? Are the images for which the computer produces a 'je' error[9] *actually* ambiguous? All of these questions are answerable only upon the production of results, and to determine whether or not the results the program produces are (likely to be) accurate, HR has first to classify the images himself.

The work HR puts in to classifying the set of images manually allows him to match results to images and make an informed decision about how well the program is performing, which is something the program cannot yet do. In one instance, HR comes across a 'nice' image (see Figure 6) during manual input which he picks out because of an interesting feature that is clearly visible on it – a galactic arm.[10] This feature is interesting to HR for a number of reasons, chief amongst which are that it is rare to see something so well defined among these images, which makes it of general interest astronomically. Hence, HR sets this image aside – he selects (Lynch, 1988) and values (Vertesi, 2012) it at least in part for its aesthetic qualities as a clear representation of a galactic object. However, the presence of this feature is also relevant to the current programming, in that it stands as a strong indicator that the image is a gravitational lens (because at least one of the primary objects is very likely to be a galaxy, which is the case for a good deal of positively identified lensing systems), and would therefore be useful as a test case for checking against the result the program produces. It is the finding of a distinguishing feature in a specific image that provides its utility. As HR explains:

> This looks kinda cool, I think this is a gravitational lens and is a-, this one looks very close to the...to the...so you- you tend only to have one bright lens: another one and this [the secondary object] one looks close to the galaxy cos you can see some sort of galactic arm. So, that might be nice to see what's gonna happen.

For HR, images like this, where there are criteria for judging this a 'strong' lens or non-lens, are useful in getting the program to work. Goodwin (2001: 163) notes that it is particularly important to attend to "the contextually based practices of the participants who are assembling and using […] images to accomplish the work that defines their profession". With this in mind, being able to spot these 'strong' images as they come up becomes a key element of HR's programming work. He can capitalise on his ability to make scientifically-informed visual classifications of single images, which when combined with the program's capacity to process lots of images quickly (and with quantified statistical information that indicates how accurate it judges its results to be) provide adequate resources for refining the program. Lynch and Edgerton (1988) mark a quantitative/qualitative distinction in the scientific use of images in astronomy, citing examples of astronomers noting that images do not enable quantitative tasks, but allow for broader and more intuitive viewings of the data by eye. Lynch and Edgerton's (1988) approach, with which we would agree, is not to argue that these qualitative viewings are 'unscientific' in any way, but to recast the work of producing quantitative (scientific) results as something that can legitimately be achieved by a work process featuring qualitative (subjective, creative) elements.[11] As HR looks at the image of a galaxy with a visible galactic arm, he is able to spot at a glance what his program has (as yet) no ability to 'see'. This asymmetry between HR's and the programs' capabilities provides a tool for progressing towards a positive outcome.

Arranging for comparison

For HR, this day's work is to improve the program's ability to discriminate lenses from non-lenses (i.e. to reduce the number of 'je' errors in the results, currently in around 20% of cases). HR therefore needs to ask if this day's work is contributing to this objective, and finding a way of checking this becomes an issue. In one instance, HR compares the results produced by two different versions of the program: version 1 (the original program, which takes basic data from all images) and version 2 (the 'new' program, which integrates information about the peak coordinates defined by HR through manual input). This is intended to reveal what is happening in the new version, and both versions of results are fundamentally comparable – there are entries for each individual image in both versions. This is similarity to the reading work mammographers apply to their images, as characterised by Slack et al.:

> Mammograms are arranged to be viewed in a manner that renders the biography of a particular breast visible. Mammograms from previous screenings are juxtaposed with those from the current round. Practically, this enables the radiologist to assess if any changes have taken place and to examine features in a retrospective-prospective manner (Slack et al., 2007: 178)

Furthermore, Amann and Knorr Cetina note that, "Analyzability is not just imposed upon the visual record by labelling and other techniques. Rather, it is *built into* the record from the beginning through the way the experiment is designed"' (Amann and Knorr Cetina, 1990: 107), and in ways that rely on the visual arrangement of on-screen information in the name of facilitating the work to be done with them (Knorr Cetina, 2003). Comparably, HR has pre-designed the day's task such that he can produce, arrange and correlate two tables of results (from version 1 and version 2) for single images and use any differences in results to judge whether the new program is better or worse in terms of its ability to discriminate lenses from non-lenses.

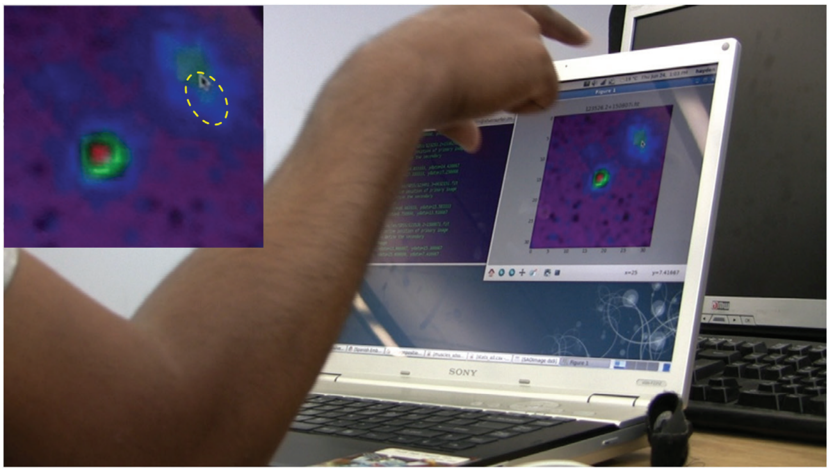


Figure 6. A 'nice' image featuring a galaxy with visible arm (highlighted).

To amplify this comparability and make it more visually manifest, HR arranges the two results screens side-by-side, such that the results for individual images are broadly on a level (see Figure 7). With this configuration of the two versions' results, HR makes an at-a-glance comparison of the first few cases – so far, the results seem improved in that there appear to be fewer 'je' errors in version 2 than in version 1. However, looking more closely, HR begins to compare individual cases from both versions' results, accenting these cases by clicking on cells within the row (thereby drawing attention to individual lines on each display to enable an easy shifting of gaze between them). Thus, HR highlights the cells in case three in version 1, then the cells in case three in version 2, allowing him to see that for this case, version 2 produces a 'je' error whereas version 1 produces a valid result. It is this fact that prompts HR to pick out case three specifically – for case three, the supposedly 'improved' program (version 2) can no longer classify an image that was classifiable in version 1. The program's capability to make a decision should have been improved across the board; that it has worsened in some cases is a possible cause for concern. HR goes through more case-by-case comparisons for cases in version 2 resulting in a 'je' error, and finds that this is not a one-off, but recurs. HR eventually attends to case nineteen (see the magnified section of Figure 7) and explains:

> [The program] gives me one [a 'je' error] here- oof! Thissa bad one. This is bad... I'll just have to go through the data to...it seems that it's not as ideal as I thought.

Because case nineteen has a particularly strong numerical result in version 1, the presence of a 'je' error in version 2 has a stronger resonance for HR's work, instigating a diagnostic approach to ascertain why this is so (see *Visual Diagnostics* below). As Lynch notes of his biology lab researchers, when their experiments failed to work, a question remained: "'Did we do it correctly? Is there anything we could have done that would have made it work?' Such questions arise in the absence of a possible authoritative resolution by means of comparisons to a standard" (Lynch, 1985: 114). HR however *does* have a standard (of sorts) since he understands how the two versions differ and so is able to use an earlier version of results as a 'sub-standard' (the comparative criterion being that the old results should be worse than the new). From looking at how this comparison is made, it is clear that there is a marked difference between what HR can see at first glance (i.e. that version 2 is in fact an improvement) and what can be seen on closer inspection (i.e. that that improvement has some concerning caveats which must be further investigated). Through visually arranging the two sets of results for comparison HR allows himself

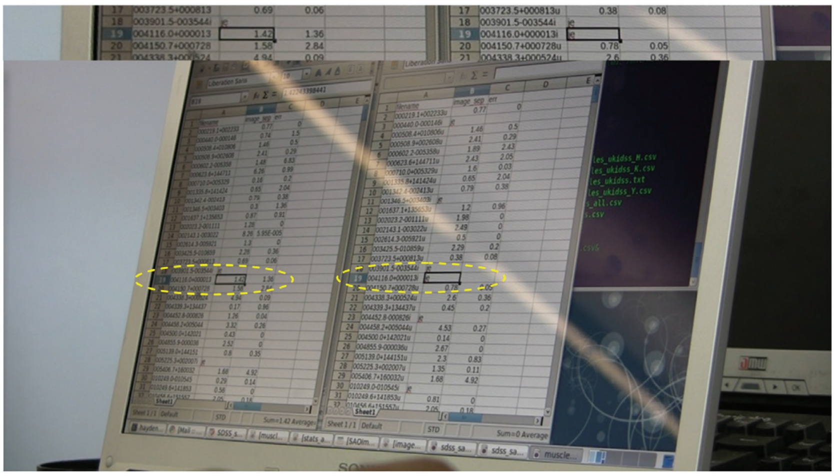


Figure 7. Comparing results side-by-side, with case nineteen highlighted in each set.

both a broad at-a-glance comparison between them, and sets the stage for a more detailed case-by-case comparison which counts towards a positive development of the project.[12]

Visual diagnostics

As with any other endeavour, working with visualisations often heralds problems, and diagnostic work must be performed to search for, locate and solve them. Complex problems might even 'hide' errors from view, and programmers might have to rely on a variety of diagnostic techniques to come to a solution. These are, in the ethnomethodological parlance, the 'normal troubles' of programming work. Given his reliance on visualisations, HR makes use of visual resources for diagnoses. To ascertain why his new version of the program is producing 'je' errors where there were no errors in the original untreated results. HR checks results case-by-case, and notices that case nineteen is giving a 'je' error in version 2 of the program but a valid result in version 1. However, the question why this should be remains – which version of the program has made the correct call – perhaps the program is *right* to call image nineteen a 'je' error if the object is genuinely ambiguous (i.e. that it is difficult to tell whether it is or is not a gravitational lens)? Or perhaps, as the weight of evidence of unexpected 'je' errors in version 2's results suggests, the program is somehow not using HR's manual input as he would like it to? To resolve his problem HR calls up the original image for case nineteen (see Figure 8 below) to classify it with his own visual judgment. As Knuuttila notes of particular types of programs used in syntactic analysis called 'parsers':

> above all, the parser must function well, which means that a parser must be able to carry out some of the tasks (i.e. syntactic analysis) that humans can. To do this, parsers do not necessarily have to be 'psychologically realistic,' and it is highly probably that they will not be so. (Knuuttila, 2006: 47)

Here, HR is attempting to ensure that his own program functions well by pitting his own abilities against the 'psychologically unrealistic' program's. From a quick visual analysis of the image, HR can see that the image for case nineteen looks to be a clear example of a gravitational lens. HR concludes that version 2 must be mistaken in its classifying of image nineteen as 'unclassifiable', and therefore it is something in the program that is at fault and not the image or the lens itself. As HR notes at this point:

> This is weird; this is a really good lens! It gave me an error on something that supposed to be, well, perfectly fine. Oh boy. This is not going to be good.

This is a significant problem for HR's project, requiring work to understand why the program is not able to classify certain lenses that he can easily classify himself. As Lynch notes of his biology lab researchers, for them, "the most interesting (and problematic) artifacts were not definite 'things,' but were 'possibilities' […] As possibilities they were not, as yet, specific features of any microscopic scene, but were tied to readings of the scene" (Lynch, 1985: 86). This is exactly how HR uses visual clues to diagnose problems – he infers, from various visual properties of what can be seen on screen, the possibilities of what might be happening. As it stands, the next obvious possibility as to what might be happening is that maybe HR's manual input – his clicking on the two peaks in each image – was to blame.

HR opens the two databases of his peak coordinates (*x* and *y* coordinates of where he clicked on the primary peak, and *x* and *y* coordinates of where he clicked on the secondary peak) to ascertain exactly where on the image he clicked. This can then be compared against the image itself – this particular screen features a cursor magnification function allowing HR to zoom in on the area around his cursor and thus locate both peaks more precisely (see Figure 2 above and Figure 8 below). Comparing his previous clicks on the image against where he would now click, having taken more care in identifying the peaks, HR finds his original clicking was not accurate enough: the coordinates in the database are some distance from the coordinates of the peaks as they appear under the magnified cursor. Therefore, HR concludes that his original manual input will need to be re-done if it is to be of any use in terms of improving the program. HR's inaccuracy is comparable with Suchman's (1994) concept of a 'garden path result', whereby during the course of his manual input work, HR:

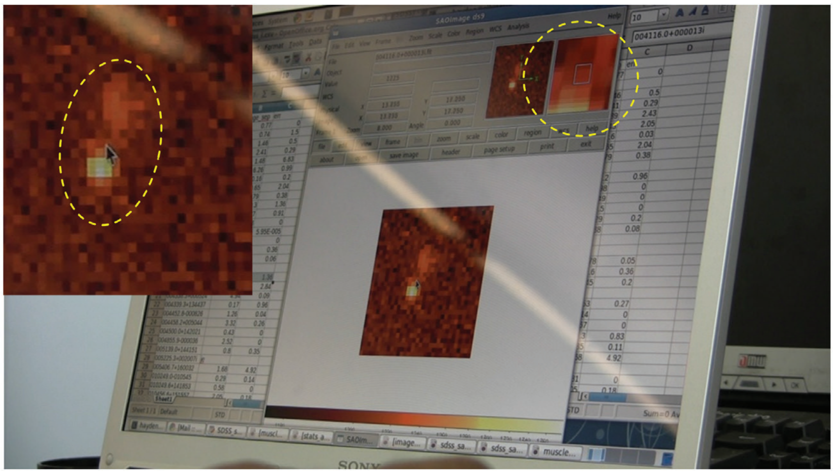


Figure 8. The image for case nineteen – the clear distortion of the radiation emitted by the two objects indicates a good lens. Also note HR's use of the magnification display to closely analyse this distortion.

> takes an action that is in some way faulted, which nonetheless satisfies the requirements of the design under a different but compatible interpretation [i.e. that two clicks have been made, regardless of their accuracy]. As a result, the faulty action goes by unnoticed at the point where it occurs. At the point where the trouble is discovered by the user [or programmer], its source is difficult or impossible to reconstruct. (Suchman, 1994: 170)

Here, however, HR *is* ultimately able to diagnose and reconstruct the trouble's source and find the problem and its solution, through looking more closely at that which (as he understands it now) he had rushed through. As Spencer (2012: 92) notes, "visualisation can also draw the scientist beyond the fact of error, towards its underlying cause and towards the future of its eventual resolution", and it is this feature of visualisations that HR draws upon in returning to the pictorial view of the data. HR is checking if the program can produce something he can identify visually, and finds the issue is his own precision placement in a visual field; his accuracy with the manual input, which limits the program's ability to consistently distinguish gravitational lens.

Discussion

The argument presented here is deeply-rooted in major themes within the field of STS dealing with the interactivity and collaboration involved in producing scientific knowledge, particularly pertaining to the usage of digital data and programming languages. This work has been characterised by some as purely a matter of the social and cultural organisation of scientific research, where success in science is achieved through the effective bringing together different knowledges and skills through collaborative interaction (Agar, 2006; Bruun and Sierla, 2008; Götschel, 2011; Hine, 2006; Louvel, 2012; Mulinari et al., 2015; Pettersson, 2011; Rall, 2006; Sundberg, 2010; Voskuhl, 2004). The present paper extends its scope to settings where there is "little overt, bodily behavior" (Bruun and Sierla, 2008: 140) other than independently-conducted mouse and keyboard use. Though we do not deny the sociability inherent to all scientific work, we focus our attention on precisely such 'independently executed' activities, in order to round out the discussion beyond the more overt social and cultural focus that has historically been given primacy in the field of science and technology studies.

With this in mind, our attention falls upon the ways in which work is achieved through the material and practical usage of screen-based resources – the visuals and visualisations that are generated and used in routine tasks that inform reasoning and inference based on what can be seen on-screen. The material aspects of scientific research work raise a perennial question for STS around a (supposed) contradiction: the experimenter's regress. Ruivenkamp and Rip (2010) describe Collins' (1992) original conception of the problem: "The unknown is to be captured in an experiment, using instruments adequate to the task. However, we do not know whether the instrument is adequate until we are sure it gives us correct readings. But since the phenomenon itself is unknown yet, there is no way to decide what correct readings are" (Ruivenkamp and Rip, 2010: 4). This paper has aimed indirectly to puncture this standard conception by demonstrating, in the fine detail of their scopic work, that scientists can find ways to measure without opening up a regress. Hence, the routine character of these practices is (or, rather, should be) a critical topic for STS researchers. As Garfinkel et al. (1981:139) note, "Situated inquiries are practical actions and so they must get done as vulgarly competent practices". It is practices such as these that we have gone some way towards unpacking here.

Invariably, for researchers working with visualisations these practices are bound up in the visual resources available, not just within code but throughout the visualisations themselves. As Burri and Dumit (2008: 302) note, "Visual expertise also creates its own form of literacy and specialization". Such literacy involves the skill to use visualisations as resources and as *sources* of resources. Throughout the day's work HR could draw on the clues left as part of comments in his code, temporary visibility arrangements, the 'sequentiality' of images and visible features of the images themselves, the ability to distinguish by eye between 'good' lenses and non-lenses, arrangements to facilitate both general (i.e. between tables) and direct (i.e. between individual cases) comparisons of results, and comparisons of different versions of the program. This particular constellation of visual resources is useful to HR because achieving a working program is the object of his work. HR's visualisations are not simply outputs; they are new resources for doing new things. Visuality is both the topic and the means to address that topic, meaning HR does not have to rely entirely on the results produced by the program to inform his work – the results themselves can be legitimately questioned. This makes the program an interplay between the original observed data (the images) and the results, facilitating an iterative process that requires a 'building up' of understanding of what effects manual input might have on results, and accordingly, what information can be drawn from the results and associated diagnostic work about the quality of the manual input. There is no decisive criterion of which iteration might be the last, yet this nevertheless allows for the development of a program that will eventually be able to discriminate lenses from non-lenses with so few 'je' errors as to make the whole collection of results statistically useful.

We have tried to show how the practical tasks involved in visualisation-based research and programming iteratively inform each other, and more widely, how work of this kind is conducted in such a way as to contribute to the successful progress of a scientific project which is reflective of scientific research in a computational age. It is worthy of note that HR is not any kind of special combination of programmer and scientist, in that many recent science graduates now have some introduction to and hands-on experience with one or more programming languages as an essential part of their training. For HR, learning how to construe visualisations is a joint product of his disciplinary knowledge of astrophysics and his programming skill. The instances considered simultaneously reflect programming activities for scientific purposes, the two inextricably bound together in the work. What our analysis of the collected video data has shown is that despite the work at hand being visible through a computer screen and associated keyboard and mouse usage, it is possible also to attend to the sense in which it makes available a set of material practices for achieving scientific knowledge.

We have developed six 'themes' in HR's work activities, revealing a selection of work activities that are mundane and routine in astrophysics programming, but which have been, at times, overlooked from sociology's accounts owing to their material character. Without denying that scientific work is extensively collaborative and interactive and affected by social and cultural factors, we do take issue with how such a focus might be singularly applied to the effect of neglecting other aspects of what is going on. In this regard, we have explicated HR's work as a 'twinned'[13] problem-space of scientific phenomena and software. The software constructs and constrains HR's perception of the data – literally, his ability to perceive gravitational lenses in the images– whilst the phenomenon constructs and constrains the use of the software (in that his programming work relies upon an accurate scientific understanding of gravitational lenses).

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Notes

1. Certainly the astrophysics research presented here is computational through and through, yet there are elements to other types of astrophysics work which are decidedly 'manual' and which may only use software rather than develop it – see for instance Hoeppe (2012) on the work of collaboratively operating a satellite telescope to collect. In this sense we say only that the specific type of astrophysics work depicted here is inherently computational, and explore how this specific type of work is achieved.
2. The Feynman under discussion is noted physicist Richard Feynman. Rall (2006) investigates his work as the manager of a computing team building the atomic bomb, which first consisted of a) untrained scientists' wives, then b) computer-trained WACs (Women's Army Corps) and finally c) soldiers with computer training and full knowledge of the project objectives.
3. The two versions of Feynman discussed here – Rall's (2006) Feynman-as-manager and our Feynman-as-scientist – are not the same in that they do not do the same things, they do not use the same technical languages, they do not talk to the same people, they do not draw on the same fields of knowledge to achieve their work, and so on.
4. It may be important to note that although our goal is the same – to see what else there might be to visualisation- and visual-work beyond interactive and collaborative face-to-face sociality – our project differs from Carusi's (2011). Where Carusi (2011) aims to explore the problem philosophically, our work treats the issue as empirical (cf. a similar debate between Bloor and Lynch in Pickering, 1992).
5. This preparatory work has involved (on the part of the principal author): talking to participants and their peers and supervisors about their project work and their role in wider research projects and groups; learning elements of undergraduate-level textbook science and mathematical techniques; acquiring a working knowledge of the Python programming language, and; attending undergraduate lectures across all four years of the University of Manchester's MPhys degree (including lectures on theoretical physics, mathematical requirements for physicists, and various aspects of astrophysics including stellar evolution, galaxies and early universe cosmology).
6. A gravitational lens is a phenomenon whereby electromagnetic radiation (ultraviolet rays, radio waves, visible light in the optical range, x-rays, etc) is 'bent' by the gravity of another high-mass object nearer to us in our line of sight. Therefore, a lensing system can be identified by the presence of an interconnected distortion between the radiation that each object emits, and a non-lens can be identified by the absence of this feature.
7. Python comments in the editor HR is using are (primarily) signified by the use of a hash symbol and appear in blue, further visually distinguishing them against other code.
8. ParselTongue is an interface to simplify complicated data reduction in Python (i.e. turning long strings of numerical information into images) with techniques from an add-on Python module (Astronomical Image Processing System, or AIPS) (Kettenis et al., 2005).
9. A 'je' error in HR's program was a result that signified that the program was unable to classify the image in question as a lens or otherwise – most likely the program has identified significant evidence for both instances (i.e. the image is a lens, the image is a non-lens) and can't thereby reject either.
10. The 'objects' in lensing systems are often galaxies. Though there are different types of galaxy, spiral galaxies (such as our own Milky Way) are comprised of a central concentrated 'bulge' of stars and a flat rotating disc of stars, dust and gas. This disc features long thin 'arms' of stars, which appear like a spiral due to their rotation.
11. This may in fact be a key reason for the continuing human involvement in science despite the sweeping advances offered by computing power – where computers are far more capable as number crunchers, they are somewhat lacking in the qualitative and creative department, which seems to be just as much a requirement for the production of scientific knowledge (Lynch and Edgerton, 1988).

12. Although these results look bad after close comparison, this is not an unrecoverable disaster for HR – it certainly is an upset that means his programmed technique for finding lenses and non-lenses is not working *yet*. However, it also points to a need (and direction) for further development and improvement, without which the project would be incomplete.
13. This is not to limit the problem-space to two factors only. This statement should be considered as part of the argument against limiting sociology's remit to only the interactional features of scientific work.

Citizenship in Collision:
Notions of Agency in Road Safety Work

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Abstract

In 2004, the Norwegian Accident Investigation Board (AIBN), previously restricted to civil aviation, was expanded to include a new section for road traffic, which was to investigate individual road accidents. The overall ambition behind the new organisation was to reduce the number of fatalities in road traffic. This article explores the idea that the main task of the Accident Investigation Board's section for road traffic was to construct a new kind of narrative about road accidents, which would in turn open up new possibilities for intervention. The article examines what characterizes the narratives they have constructed and how these narratives interact with conceptions of risk and causality. It also discusses how they fit into the existing structure of road safety work in Norway. It concludes that the Accident Investigation Board's narratives are implicitly political, as they partly deconstruct the notion of liberal citizenship underlying the legal system, and that this deconstruction can potentially have far-reaching practical consequences.

Keywords: Road traffic, road safety, narratives, risk objects, citizenship accidents.

"The death of one man is a tragedy; the death of millions is a statistic".
Attributed to Josef Stalin

Introduction

Road accidents are the eighth leading cause of death globally, and the leading cause of death for people aged 15–29 years (WHO, 2013). Norway, however, is one the countries in the world with the lowest number of road fatalities relative to kilometres driven[1] (European Transport Safety Council, 2013), and Norwegian authorities have long made targeted efforts to reduce the number of fatalities and injuries. In 2004, the Norwegian Accident Investigation Board (AIBN) was thus expanded to include a new section for road traffic. Their task, as defined by the Government, was to investigate individual road accidents, and to construct road safety advice on the basis of the investigations. The overall ambition behind the new organisation was to reduce the number of fatalities in road traffic (Norwegian Road Traffic Act, §44).

This article explores the idea that the main task of the Accident Investigation Board's section for road traffic was to construct a new kind of narrative about road accidents. It discusses what kinds of narratives they have constructed, how these interact with conceptions of risk and causality, and how they fit into the existing structure of road safety work in Norway. I argue that the Accident Investigation Board's reports have constructed new kinds of risk objects (Hilgartner, 1992) and that the novel narratives and the risk objects they call into being are implicitly political, as they partly deconstruct the notion of liberal citizenship underlying the legal system.

The article is based on government reports and whitepapers, published reports from the AIBN's section for road traffic, and interviews with employees in the road safety section of the AIBN, in the Norwegian Directorate of Public Roads, and in the Ministry of Transport and Communications.

The liberal citizen

The automobile and its infrastructure are important defining features of modern societies. Roads, bridges and tunnels for cars are among our most costly and invasive infrastructures, and the car-system shapes our cities (Hommels, 2005), neighbourhoods (Bendiktsson, 2015) and even our natural landscapes (Hvattum et al., 2011). Road crashes are one of the main "unnatural" causes of death in most societies, developed and developing alike. On this background, cars, roads and road safety are strangely marginal topics in STS literature, where studies of cars have often been historical, typically centred on the development of alternative automotive technologies such as electricity (Gjøen and Hård, 2002; Brown, 2001), gas (Braun, 1992), and ethanol (Carolan, 2009).

With increasing focus on issues such as pollution, public health, urbanisation, densification, and land use, road traffic and car-dependency are increasingly seen as problematic aspects of our societies. In addition, efforts to prevent traffic fatalities have intensified, and radical new approaches to road safety have been developed in several countries (MacAndrews, 2013; Elvebakk, 2009), which involve a reconceptualisation of the relationships and responsibilities between actors in the road system.

Jain (2004) and Wetmore (2004) have demonstrated that the current distributions of agency and responsibility in road traffic and road safety are not given, but the outcomes of complex and reversible processes of negotiation and renegotiation. Recent concepts in mobility studies such as 'the car-driver-hybrid' (Sheller and Urry, 2000), 'the driver-car' (Dant, 2004), and 'the autoself' (Randell, 2016) likewise highlight how technical assemblages blur or challenge notions of subjects and objects in road transport. Although most people spend considerable parts of their lives in road traffic, little attention has been afforded to how these hybrid assemblages impinge on and interact with wider societal notions of subjectivity and citizenship[2].

The liberal notion of citizenship is fundamentally linked up with individual freedom (Schuck, 2002). Traditionally, liberal theories accept restrictions on the actions of individuals in so far and only when they interfere with the rights and liberties of others: your liberty to swing your fist ends where my nose begins. Implicit in this principle is the idea that the individual is the fundamental building block of society, whose actions, plans and strategies, in so far as they are not harmful to others, require no further justification. Arguably, this conception also implies that the liberal citizen is fully formed, and must be accepted as such, without reference to the formative process. John Stuart Mill, for instance, states that "there is a part of the life of every person who has come to years of discretion, within which the individuality of that person ought to reign uncontrolled either by any other person or by the public collectively" (Mill, 1999: 371). Thus being an autonomous agent involves being an independent entity (Dworkin, 1972). John Rawls' similarly presents the "political conception of a person" (Rawls, 1993), which has been described as "an antecedently individuated subject, the bounds of whose self are fixed prior to experience" (Sandel, 1998: 55).

Sandel's criticism is usually categorizes as communitarian, but also feminist theorists such as Robin L. West (1999), Judith Butler (2011) and Wendy Brown (1995) have presented alternative visions. They argue that these tenets of liberalism overstress the masculine values of autonomy and independence, while ignoring that individuals belong in tightly knit networks, most importantly families. According to McClain (1991: 673), liberalism presents a "model of separate, atomistic, competing individuals establishing a legal system to pursue their own interests and to protect them from others' interference with their rights to do so". A central aspect of the feminist criticism is that it frequently problematizes the liberal distinction between the public and the private, arguing that the (public) voluntariness advocated by liberals is illusory, as people's choices are formed by their (private) socialization into, among other things, gender roles (Higgins, 2003. In other words, liberal theories ignore the histories behind the autonomous subject.

The corresponding tendency in ethical theory to treat individuals as fully formed and independent has been challenged by those espousing alternative approaches to ethics, perhaps most notably theorists associated with 'the ethics of care' (Gilligan, 1982). These criticisms tend to emphasise that borders between individuals are secondary, and that relationships of entanglement and responsibility are prior to universal human "rights", especially the right of non-interference. Autonomous subjects are constructed through a process of rearing, where women typically play a significant role. We could sum up these criticisms as maintaining that the liberal subject does not have a history, is not to be found in a specific context, and has no concrete, specific relations to others. Kymlicka (2001) concludes that while liberalism seems a valid description of ethical relations between independent individuals, an ethic of care better describes relations to dependents. Since all individuals start out as dependents, having dependents is a necessary condition for having independents. The question then, becomes where to draw the line between the two states; when an individual can reasonably be considered autonomous, as is a premise for much liberal theory.

In liberal societies the tension between dependence and autonomy has frequently been solved through excluding certain individuals from the sphere of full citizenship. Various gatekeeper functions define when, and in what circumstances, one should be accepted as a fully formed citizen. Children are usually excluded, and so, in many contexts, are persons with severe mental deficiencies.

The particular citizen of Norwegian road traffic is often defined with reference to paragraph 3 in the Norwegian Road Traffic Act, which states that "A driver shall show consideration and be alert and cautious so that he does not cause damage or risk, and so that other traffic is not unnecessarily obstructed or inconvenienced". In road traffic, licencing requirements and regulations exclude children, sufferers of various deceases (such as Alzheimer's, etc.) and individuals in states that can interfere with their ability to make choices (e.g. drink drivers) from driving a car. This citizen works as a standard, and like all standards, it will exclude as well as qualify: some people are not allowed to drive cars, because they are too young, do not possess the relevant physical or mental abilities, have not passed a driving test, or have had their licence revoked. For shorter periods, one is excluded from the standard when under the influence of alcohol, drugs, or certain kinds of medication.

Making Norwegian road safety work

In Norway, road safety work is mainly organized on three levels; national level (Ministries, the Norwegian Public Roads Administration (NPRA) and directorates), regional level (counties and regions) and municipal levels. Various public bodies and NGOs contribute considerable efforts on all three levels.

At the level of government, the Ministry of Transport and Communications has the primary responsibility for road safety, while the Ministry of Justice is responsible for enforcement, and the Ministry of Education for traffic education in schools, and driver training. Technical road safety work is the remit of the Norwegian Public Roads Administration.

In practice, the work has been divided into three separate spheres; on the one hand, there is the judicial sphere, encompassing law-making and enforcement by the traffic police. Secondly, there is the Norwegian Council for Road Safety (*Trygg Trafikk*), which is an umbrella organisation for voluntary road safety work and serves as a link between voluntary associations and the road safety authorities. The Council is to promote the best possible road safety for all groups of road-users, and holds a special responsibility for promoting traffic education in schools and kindergartens (Norwegian Council for Road Safety, undated).

Thirdly, there is the Norwegian Public Roads Administration (NPRA), led by The Directorate of Public Roads. The NPRA has sectorial responsibility for roads and road traffic. The agency is, among other things, responsible for planning, building and maintaining state and county roads, and developing regulations and guidelines for road design, road traffic, driver education and vehicles. The NPRA also performs controls of workshops, vehicles, driving and resting times and seat belt use, and conducts driving tests and supervises driving schools. The organisation has an overarching responsibility to actively promote road safety, for instance through measures such as road safety campaigns. This means that historically, the NPRA has had a very broad influence over many aspects of Norwegian road safety work, and has not been subject to independent scrutiny, with the exception of the Government.

The narratives about road accidents produced by the NPRA have traditionally been stories of aggregated numbers. The keeping of statistics on road accidents with injuries to persons or major material damages dates back to 1939 in Norway, while from 1964, only accidents with injuries to persons have been reportable to the police. From 1977, a joint form for reporting accidents has been shared between the Police, Statistics Norway, and the road authorities. About 9000 accidents are reported annually (Statistics Norway, undated). These statistics contain information such as the date and location of accidents, the age and sex of those involved in accidents, the category of road user group (driver, passenger, pedestrian, cyclist, etc.), and the severity of injuries. Provisional[3] accident statistics are published monthly, and routinely compared to the number of fatalities the corresponding month the previous year and to the aggregated mean for the last five years.

The NPRA and other actors such as research institutions develop further statistics on the basis of this data, for instance pertaining to the average age of drivers involved in accidents, the average age of the car, the day of the week and time of day when accidents take place, the risk of specific groups of road users, etc. Accident statistics are also linked to other records, such as The Road Directorate's registries of motor vehicles and driving licenses, and drug use data from the Norwegian Institute of Public Health. These narratives about road accidents have thus relied heavily on a statistical style of reasoning (Hacking, 1990), where aggregated numbers are used to construct law-like connections between (an increasing number of) phenomena and outcomes.

In addition, the NPRA's regional accidents analysis groups (AAG) publish annual reports on fatal accidents in their regions and occasional thematic reports on topics such as fatal accidents involving young drivers or cyclists. These groups began their work in 2005, and display an influence from system oriented safety thinking (Shalom Hakkert and Gitelmann, 2014) in their multi-causal approach to accidents and explicit avoidance of apportioning blame. Their reports, however, follow the traditional logic of the accident statistics; they provide more detailed information on vehicles, road users and environments involved in accidents, but they still present their findings in terms of aggregated numbers and well-defined categories, and their results are combined to form a searchable database.

These stories told by the road authorities have served a specific purpose in the Norwegian system of road accident prevention; they establish causal links. For instance, the disproportionally high number of young drivers involved in accidents has contributed to constructing the young driver as the kind of thing that may cause accidents, as a 'risk object' (Hilgartner, 1992). In this system, the risk object is never so as an individual, but as a representative of a group, and its existence is necessarily established over long time periods and through high numbers of instances, to avoid arbitrariness. Thus the calculated 'normal' functions not only as a descriptive, but also as a normative standard (Hacking, 1990). Specific measures have been developed to bring down the risk of 'high risk groups': older drivers are required to go through medical certification; driver education has been modified to improve the performance of the young[4]; targeted safety campaigns have been run, etc. A risk object is not necessarily a road user, however; there is an ongoing effort to remove unsafe cars from the roads, and the entire Norwegian road system has been divided into stretches and given a safety rating based on accident numbers (compared to the calculated mean). When there is a disproportional number of accidents on a stretch of road, the NPRA will consider various measures to make it safer, such as improving the road, reducing the speed limit, or installing speed cameras (Ragnøy and Elvik, 2003).

These aggregate numbers thus work as basis for policy, and policy is justified with reference to accident statistics. Statistics is accumulated over several years, however, and it can take a long time from a potential problem is identified to measures are taken. Measures have typically also been justified with reference to cost-benefit analysis: measures – and certainly big and costly measures – should ideally be profitable societal investments. Currently, a statistical life in traffic is valued at around 35 million NOK (Statens vegvesen, 2010), which means that life-saving measures will be deemed profitable if they cost less than the number of statistical lives saved multiplied by this sum. The rationality of the system therefore rests on this logic of statistics and macro-level predictability, and the quantitative stories guarantee the rationality of the system of accident prevention. In this system, a single accident necessarily has limited informational value.

Investigating road accidents

Around 2000, Norwegian roads were among the safest in the world, yet the Ministry of Transport and Communications was strongly committed to working for further reductions in the numbers of fatal and serious accidents. Their ambition was to see accident statistics improve from one year to the next, in spite of the continuing growth in traffic. At the time, the government was also working towards adopting the Swedish concept *Vision Zero,* a long-term vision of a road system that does not lead to fatalities or permanent injury (Elvebakk, 2007; MacAndrews, 2013.) However, many traditional road safety measures were perceived to be exhausted, at least within realistic budgetary constraints. The question that arose, therefore, was, as one of the employees in the Ministry of Transport and Communications put it, "what next?"

The Norwegian National Transport Plan 2002-2011 announced the Government's intention to consider the establishment of a joint accident investigation board "for all major accidents and incidents in sea, air, rail and road transport." (Ministry of Transport and Communications, 1999). In 2001, the Government appointed a working group to review an expansion of the existing Accident Investigation Board for Civil Aviation into an organisation similar to the American National Transportation Safety Board, which holds a broad mandate and investigates accidents in civil aviation as well as major accidents in the other transport modes.

The governmental working group submitted its report in 2002, recommending that the AIBN be expanded to encompass the road and maritime sector. The report predicted that such a multi-sector organisation would benefit from economy of scale, and enable the introduction of a cross-disciplinary approach that would complement technical investigations with insights from the social sciences and competence on human factors (Ministry of Transport and Communications, 2002a). This recommendation led to the appointment of a second working group, tasked with considering consequences of the expansion of AIBN to the road sector. The report from the second working group was published in April 2003 (Ministry of Transport and Communications, 2003).

The expansion of the Board met with no political opposition; the central-right Government which replaced the social democratic Government in 2002, included the establishment of a cross-sectorial accident investigation board in their government platform (Ministry of Finance, 2002), and the bill passed through Parliament on a unanimous vote and without debate (Norwegian Parliament, undated) in 2005.

Among professional actors in the road sector, however, a more cautious attitude prevailed. According to the informants from the Norwegian Directorate of Public Roads (the lead agency of the NPRA), the Directorate was overall in favour of the expansion of the Accident Investigation Board to the road sector, but expected the Board to possess a competence that complemented rather than competed with their own (at this time only planned) accident analysis groups.

The official documents provided a general framework for the activities of the new organisation. The Proposition to Parliament (Ministry of Transport and Communications, 2005) presented a regulatory framework for the road section of the AIBN (hereafter AIBN-Road) that differed from the other sectors, as it needed to be adapted to the Road Traffic Act, but the organisation's mandate was not described in detail. There was an explicit ambition for the organisation to benefit from its autonomous position (Ministry of Transport and Communications, 2003), and thus there was considerable room for manoeuvring when the practical day-to-day operations of the Board were to be given shape. The intention was for the new section to benefit from its co-location with the rest of the AIBN, and for it to adopt a methodology similar to the one used in aviation. There are significant differences between these sectors, however, which constitute potential obstacles to successfully copying methods between sectors. Most importantly, in contrast to what is the case in aviation, road traffic is characterised by a very high number of accidents, most of which do not lead to serious injury, and even in fatal accidents, the number of fatalities is usually very limited.

The high number of potential accidents meant that an attempt to investigate *all* accidents and "serious incidents", as in air traffic, would be forbiddingly expensive, especially since the new section was intended to be staffed with 4-5 persons (Ministry of Transport and Communications 2005). It was therefore necessary to find some way of delimiting the task. The first working group report suggested that the Board should focus on accidents with "high risk potential" (not necessarily catastrophic consequences), and, most importantly, accidents that held a promise of safety improvement, through the acquisition of new knowledge (Ministry of Transport and Communications, 2002b). The report further assumed that these guidelines would probably lead to a focus on accidents involving professional drivers, such as public transport and road haulage, which could more profitably be investigated with the methods used in aviation, due to greater similarities between the actors involved. As in aviation, one could address an organisational environment, rather than individual drivers and their diverse backgrounds and networks.

Unlike police investigations, the AIBN-Road explicitly—and in compliance with international regulations for airline investigations— should avoid stating only one cause of the accident; the aim is to find out how several causes interact, and how the processes leading to the accident could have been intercepted at different points. Its investigations should not allocate blame, and the information uncovered in their interviews cannot be used as basis for criminal procedures (Norwegian Road Traffic Act, § 49).

Narrating the accident

The AIBN-Road published its first report in 2006, and has since published 3-8 reports per year. The AIBN-Road freely chooses which accidents to investigate, and publishes its findings in reports, which conclude with a list of "safety recommendations". The recommendations are based on the findings in the individual investigations, and point to weaknesses in the system of road traffic. The AIBN-Road submits its recommendations to the Ministry of Transport and Communications, which, in turn, forwards them to the Norwegian Directorate of Public Roads. The Directorate is the agency responsible for 'closing' recommendations, i.e. following them up with practical measures or policies. The Directorate reports to the Ministry, which informs the AIBN of the process. The AIBN's responsibility ends with the completion of the report, however, as any further involvement might jeopardize its autonomy.

The reports usually focus on single accidents (typically involving at least one professional driver), and sometimes include lengthy technical appendixes. These reports introduced an entirely new genre of storytelling into Norwegian road safety work, as the focus was no longer on the big picture, but on one single accident at the time. The AIBN investigations have a duration of several months, and the reports relate the story of the individual accidents in painstaking detail, as illustrated in the quote below (all quotes translated by the author).

> Around 8 o' clock in the morning on Thursday September 29th 2005, an 18-year-old girl drove from [...] in the direction of [...] High School, where she was a student in her final year. On her way she went by a house in [...], to pick up her 17-year old-friend. (AIBN, 2008: 5).

As we can see, this AIBN-Road report sets the scene quite differently from the standard accident statistics. It relates the story of the accident; how it unfolds inexorably towards the point where the car is hit by a truck when turning onto the state road, and the crash leaves the young driver dead and her passenger severely injured. The report is illustrated with maps and photographs from the scene of the accident. It briefly describes the two drivers; her experience with driving and her performance in driving school education; his daily job and routines as a lorry driver, and working conditions on this particular day. From this point, the investigation turns to the causes of the accident, and how the accident, or its consequences, could have been prevented.

The report cited above concludes with the following three safety recommendations:

- The Norwegian Public Roads Administration should detail requirements for visibility from driveways on the basis of existing regulations, and develop a system for following up the requirements.
- The Norwegian Public Roads Administration should establish guidelines to ensure that the right of way on crossroads leading onto heavily trafficked roads is made clearer to road users.
- The Norwegian Public Roads Administration's should analyse accidents involving drivers with recent licences in relation to their achievements in driver education and driving tests.

The recommendations function as the conclusion of the report, although not in the sense that the report is a deductive argument, as the logic of a causal analysis does not lead directly to recommendations for prevention (Hopkins, 2014). The set of potential causes is infinite, and the analysis must always be based on a counterfactual story and expert judgment. The implicit counterfactual narrative is one in which the accident does not happen, or does not have severe consequences, and this does not follow from an accident analysis, however detailed.

Thus the conclusions to the reports do not follow from the facts with logical necessity, and they can be disputed. In the early years of AIBN-Road, the NPRA indeed frequently disagreed ardently with its conclusions and recommendations. This specific accident and the subsequent report had become a source of conflict between the AIBN and the NPRA at the time of my study, and was brought up in several interviews. The investigator in charge (ICC) of this analysis at AIBN was therefore on the defensive when describing the reactions to his report:

> [This report] has become a laughing stock [with the NPRA], because they think we have expected more than they should really be held accountable for. But I disagree with them, and – of course lots of other things are more important, but it's such a central finding, that I believe it is important. This is to do with visibility; that you make sure that visibility is sufficient for you to actually drive safely. It's not according to the books; that's not it, but about what can be safely performed. And I believe that the road authorities should take on that responsibility and make sure that any driveway into the road network is sufficiently safe. (AIBN-employee, interview)

In the narrative constructed in the report, there is clearly something that could be done in order to prevent this accident: improved visibility and a clearer right of way might have made a difference. There is a point at which the relevant authorities might have intercepted, erected a safety barrier, and prevented the tragedy. The system had a flaw, and was less safe than it might conceivably have been. The narrative also introduces a novel risk object; the unsafe driveway, against which measures should be taken. But on the other hand "lots of other things are more important" in the sense that they would be based on accumulated evidence, show up in the statistics, and probably prevent a higher number of accidents.

This kind of narrative did not sit well with the Directorate for Public Roads, and one of the informants there presented the same case in a very different light:

> Two years ago there was this eighteen-year-old girl, recent driver's licence, had driven from home and onto the public road, from her own driveway, and was killed because she didn't look around. And then they made a recommendation that the NPRA

> should control every driveway every year or at least at regular intervals. And, you know, it was her own driveway, and inattention. If, on the other hand, we were to inspect every driveway in Norway, that would probably amount to a hundred man years or so a year. (DPR employee, interview)

This creates an entirely different narrative: the story is first and foremost one about individual blame; a recent licence, she does not pay attention, and it is her own driveway, with which she should be familiar. Thus, the endpoint to this story is an established risk object, 'young driver' and a well-known statistical category, 'human error'. These terms serve to tidy up the narrative through placing the failure in a category which relegates it from the traffic system to the moral and legal system.

The story of individual blame ties in with the quantitative approach to road safety. The facts listed are *known* risk factors that are already familiar from road safety statistics, and as such, the story is brought to a satisfactory end: there is, after all, one cause, and that cause can be located in the single, young, inexperienced and inattentive person. The last sentence in the quote also refers to the rationality of the system; it can be read as an elliptical reference to cost-benefit calculations. Given that we have to accept that humans are fallible, and still allow them to drive, there really is not much to do about it. This is a narrative that does, in its own way, have a neat closure. In the manner of a crime novel, and in the manner of the criminal investigation frequently following a road accident, the story is brought to a close when the guilty person has been identified.

This approach to road accidents was also referred to by another employee of AIBN-Road, who contrasted their own methods to the tidy ending to investigations in the legal system:

> The police want to allocate blame and responsibility. If you have a single accident and the driver was killed, then it is not interesting, and the case is closed, because the guilty party cannot be found. (AIBN-employee, interview)

The closure of this kind of story is convenient, because it seems to suggest that there is actually not very much to be done. The isolated individual is to blame, and therefore the system is blameless. The story told by the road authorities contributes to upholding a stable set of relations between actors in the system, where the individual driver is allocated certain characteristics and responsibilities, and the demarcation line between the individual and the system is drawn with reference to such characterisations. Failing to perform relative to the standards places you outside of the system of orderly traffic, and renders you a subject of the separate system of legal accountability, and categories such as 'inattention', 'inexperience' and 'young driver' serve as keys changing between these registers. The legal system thus upholds the system in cases of failure – it is the guarantee that the system is working, even in the cases when, apparently, it is not.

Another employee at the Directorate for Public Roads was explicit that even if you could always "blame the system", this was not always a fruitful approach to take to accidents:

> Causal chains can be traced too far, not every consideration is equally interesting. But this probably stems from the methodology, which to some degree locks in the AIBN's work, and sometimes leads the recommendations in too many directions. It gets too complicated, too specific. One has to ask oneself what will contribute to the reduction of the number of casualties and injuries. (DPR employee, interview)

This quote illustrates how establishing the causes of road accidents is not a neutral and descriptive activity (Fahlquist, 2006), as causality is not just a factual aspect of the accident; it is related to the practical day-to-day work of accident reduction. Finding a cause involves proscribing a cure, and extending causal links might mean extending the responsibility of the relevant authorities in unforeseen and unwanted directions. An important element of the construction of risk objects consists in constructing linkages between objects and harm (Hilgartner, 1992). Since there are many branches in the processes leading to harm, and because the branches in principle have no end-points, such a construction is always problematic. However, some such end-points have been established as 'final causes', among them 'bad luck'[5], 'acts of God', and importantly, in this context, 'human error'. The deconstruction of these established end-points that is a corollary of severing the link to the statistical categories leads to a proliferation of risk objects. This proliferation naturally poses a challenge for agencies tasked with interrupting causal chains that point in "too many directions"

As noted, the NPRA has traditionally delineated their charge through a form of cost-benefit calculation: any big investment should pay off in the form of improved accident statistics; ideally a sizeable reduction in the number of fatalities. The ICC in charge of the report was also quite aware of this of this problem, and did indeed see the Directorate's perspective:

> You have 10 000 road accidents in Norway every year, and some – I don't know how many – are related to lack of visibility, but I don't think that's a lot. And then this is a kind of recommendation where you don't go "Naturally, we'll have to do this". In light of having a lot of accidents, and then you are told to prioritize visibility in driveways, it's no wonder you laugh at it. But then you miss out on a perspective – you are more concerned with the forest, as such, but not the individual trees, if you see what I mean. (AIBN-employee, interview)

The AIBN, of course, was explicitly established to consider individual trees. Their task is to construct the story of the individual accident and its possible prevention. In contrast, the NPRA's focus was not to prevent every single accident; it was to reduce the overall number of accidents as much as possible within the limits set by available resources, and within the framework of established routines, regulations and practices. As one of the managers in the department of safety in the Directorate saw it:

> The problem is that when [recommendations] become too specific you could have a problem with finances. For problems can be solved in many different ways, and not necessarily in the most expensive way. And you do not always need a 100 % effect; you can do well with a 50% effect, to put it a bit simplistically. (DPR-employee, interview)

The NPRA narratives were not stories about rendering the individual accident impossible. These were narratives of a reasonably safe system, where accidents might occur as the result of individuals failing to meet reasonable standards. The road authorities were committed to improvement, but perfection did not really seem to be on the cards, as long as individuals were fallible. Thus, their narratives frequently established end-points that excluded accident causes from the system of traffic. In the AIBN-Road's narratives, however, these causes were firmly placed within the system, and consequently, the NPRA was attributed a greater responsibility. These narratives, then, were revolutionary narratives, redistributing roles, agencies and responsibilities (cf. Wetmore, 2004), and suggesting a novel techno-scientific assemblage, which did not allow for the relegation of malfunction to the system of blame and law.

Narrative strategies

What made the AIBN's narratives revolutionary? For one thing, the individualised reports may in themselves be read as calling for more drastic measures and they create a greater sense of urgency than the aggregated numbers presented by the NPRA, where individual accidents are statistical aberrations until otherwise proven. An employee in the Department of Transport and Communications remarked that reading the reports from the AIBN served as a cruel reminder of what she was actually working with. Unlike the statistics, the narratives contain characters who, although elliptical and anonymous, are made present to the reader through brief descriptions of their age, gender, occupation and everyday routines. Narratives work through absences and lacunas as well as through what they choose to display (Lothe, 2000), so when presented with the 18-year-old girl on her way to school, in her own driveway, with her friend, it is easy for the reader to fill in the neighbourhood, her family, her friends. The report's brief account of the lorry driver's working day before the accident seems to build up to the disaster through its undramatic style and content:

> His trip was the first of the day. He was to ferry concrete to […] a few kilometres north of the scene of the accident. Work this day was as usual, according to the driver, not stressful. He started driving at about 8 o'clock, and chose the same route as a colleague who had delivered a load to

> the same address half an hour earlier. The driver reported that he was acquainted with the route, and had clear ideas about the right of way for the crossroads. (AIBN, 2008: 10)

The description of the routine and ease of the day renders vivid not just the man's reconstruction of the events leading up to the accident, but also the sudden reversal of his day from routine to tragedy, and his painful justifications for his actions after the fact. The narratives from the AIBN have more in common with classic literary genres than with statistics; they have characters, a beginning, a middle and a tragic end. Thus the AIBN's narrative turns the accident from a "normal accident" (Perrow, 2011) and a number in the statistics, to something profoundly tragic, and, it would seem, something that should be prevented at almost any cost.

Secondly, the narratives of the AIBN-Road were obviously differently framed. In the traditional narratives from the NPRA, only a few factors – although their numbers have been steadily increasing – were allowed inside. In the NPRA's annual statistics for 2011, the following categories were used: factors related to road users (speed, lack of skills, driving under the influence of alcohol or drugs, fatigue, disease, other factors), factors related to vehicles, factors related to roads and road environment, and factors related to external conditions (Norwegian Public Roads Administration, undated). The category of 'human error' – a collective term referring to a number of the factors related to roads users – was one of the largest. The categorization enables comparison over time and across locations, and exemplifies "the strategy of moving toward universality: rendering things comparable so that each actor may fit their allotted position in a standardized system and comparisons may be communicated across sites" (Bowker et al., 1996: 353). Classification, however, has not only a practical, but also a political function; rendering something explicit means rendering it visible, while other factors are excluded. While the category of human error was thus made very visible, its concrete instantiation, and any possible problematization, disappeared from view (Star, 2001).

The individuality of the stories recounted also brought with it a distinctively new kind of geographical framing; in these narratives, accidents take place in specific, modifiable geographical localities. The tragedy takes place in this specific driveway, where visibility could easily be improved by cutting down specific trees. This is in stark contrast to how accidents, from the perspective of the Road Directorate, could be seen as taking place in an abstract sphere of identified risk factors interacting in semi-predictable ways (Beckmann, 2004). However, some of the employees in the AIBN suggested that their position was better understood by people working closer to the operative part of the Roads Administration who "felt the problems more acutely". This statement is illustrative of a perceived dichotomy between the local, material practice of preventing accidents, and the dislocated and atemporal scientific approach of the central organisation. There are two seemingly incompatible speeds at work; the urgency of the specific, local situation is at odds with the timeless, universal truths of science. Statistics seek the static; to determine whether the seeming cause is a real cause, or a spurious association, and whether the risk object is real or only apparently so.

Thirdly, as noted above, the AIBN's narratives did not find their natural end-point in the responsible and fallible human actor, but extended agency spatially and temporally. In the Road Directorate's publications, the individual history of the deceased driver is left out of the frame along with the disastrous aspect, the tragic. The AIBN's approach was originally deemed best suited to professional traffic, since in organisational safety work, the choices and behaviours of the employees are seen as being at least partly within the remit of the employer. The employer can be expected or required to train or supervise employers, and in many cases, the organisation will be accountable, rather than the individual. In other words, the original instructions to the AIBN suggested that private citizens were better suited to remain end-points, and be evaluated in terms of individual liability and blame, whereas the actions of professional drivers could more fruitfully be seen as consequences of external factors. The AIBN challenged this idea, however. In the detailed narratives they constructed, every actor was part of a network that could be modified, and that was already subject to official regulation and modification:

> […] you actually have organisations behind every accident. If private citizens are on the road, then there are, we might say, no organisation behind them, but you still have an organisation behind the road system, which we look into. And we also look into how the systems work, among other things where health information is concerned. How that is taken care of; there are health requirements for driving. The health system, how it works, how it operates relative to licencing regulations. (AIBN-employee, interview)

These narratives created an image of an encompassing network, where individual actors are not isolated first movers, but enmeshed in systems that shape actions and consequences. Contrast this with this opinion offered by an employee in the Directorate:

> Your average car driver is not a professional, and using a "systems approach" is more fruitful when you are part of a system, such as employed by a company. In many accidents, the driver is the main cause of the accident, we are talking about explicit mistakes, and if that is the case, recommendations directed at other fields appear odd. (DPR-employee, interview)

There is a practical reason why the Directorate resists such attempt to challenge traditional notions of agency in traffic: if individuals are not responsible for their explicit mistakes – who is? If agency is spatially and temporally extended, who needs to act to make the roads safer, and who should control and monitor this onslaught of novel risk objects? So, again, it was suggested that a story should end when the culprit had been identified. The employees of the AIBN, however, objected to the use of 'human error' as a natural kind, and worked to pry open the category.

> It's fine to have guidelines and road standards, and everything, but you also need to know that those standards work. If you built a road in accordance with the standard, and 30-40% of the people using the road use it incorrectly: is there something wrong with the system or with the people using the system? (AIBN-employee, interview)

The AIBN-Road's narratives thus challenge the clear demarcation line between the human subject on the one hand, and the road system and the wider society on the other. Instead they present agency and human errors as network effects. These two types of narratives will have radically different practical implications for road safety work, and simultaneously perform fundamentally different ideas about the nature of citizenship.

The citizen in accident investigations

As we have noted, liberalism's essentialising of the political citizen, and disregard for contexts, histories and relationships arguably contribute to upholding the political status quo. The statistical accounts of road accidents similarly close off the citizen, through allowing their narratives to end where the citizen has been found guilty of 'human error'. Thus, road safety policy also constructs a specific kind of liberal citizens, responsible for the consequences of their actions, but not themselves the outcome of earlier processes.

In the case of the accident report discussed above, the recommendations given by the AIBN open up the citizen in different directions. One way of opening it up is through recommending that the story of deceased driver should include her performance during driver education. This recommendation suggests that having passed the test and becoming a licenced driver is not sufficient, that the history of the subject remains relevant after she has been accepted as a car-driving citizen, and that the interactions of individuals, regulations and practices are (still) part of the story behind the individual accident. This suggests expanding the narrative of the individual driver, and to allow this narrative to remain relevant after legal accountability has been established.

Second, it opens up the citizen through suggesting that the regulations governing the road users' actions may not be sufficiently clear; thus the blame shifts from the blameworthy individual to regulating authorities. Although the regulations were not legally ambiguous, the report suggested that they might still be ambiguous to road users. Again, this suggests that the story needs to be expanded: it now includes a larger number of actors –actors invisible in the official story as long as no formal mistakes have been made. The new actors are not those interacting in the traffic system, but those shaping it, for instance through developing regulations. The system is capable of unambiguously allocating blame, but is now accused of co-producing this blame.

Third, the recommendation that the NPRA should develop requirements for visibility from driveways and a system to follow them up indicates that the actions of an individual cannot be understood in separation from their material context, and that the material environment must be adapted to humans, rather than the other way around. The responsibility of the individual is presented as a quality that comes in degrees; it is possible to modify the surroundings in such a way that the individual is *more likely* to act correctly although it should already have done so; the fatal action is not so much a choice made by an autonomous subject, as the outcome of a material network of interconnected relations. This recommendation also illustrates that the move from aggregate numbers leads to a proliferation of risk objects. When the risk object no longer emerges from long series of events and disproportional risk as compared to a 'norm', what you try to do, in fact, is to prevent *this* accident in the future. Since every accident is unique, the number of elements is in principle infinite.

The recommendations thus present the accident as the outcome of a temporal and spatial history involving a number of agents, where the agency of the legally accountable citizen is a constructed entity and the result of a history, not a final and unitary cause. When opened up, the road using citizen turns out to have a Medusa head, with contents uncontrollably snaking their way in every direction. This distributed agency implicitly presents the system as liable to be held accountable as the road user. This is clearly at odds with the Norwegian Traffic Act, according to which the drivers have strict liability for their actions.

Concluding remarks: Narratives and politics

As an extension of and supplement to concepts such as 'the car-driver-hybrid' or 'the driver-car', this case demonstrates how hybridity is not limited to the single vehicle. The borders between the individual, the vehicle, the surrounding environment, and social and legal institutions are all open to renegotiation. Just as intelligent transport systems installed in cars will imply that "only as the car-driver hybrid can both subject and object get 'smarter'" (Beckmann, 2004), an improved driveway might transform 'inattention' into 'alertness' and improved driver education could eliminate the 'young driver' as a risk object.

The narratives of the AIBN remove some of the agency from individuals to their social, material and institutional contexts. This technical move is also political. There is a reason why "liberal theory has had to take individuals much as it finds them on the surface." (Schuck, 2002: 132.) The AIBN's reports present a view of causality and agency that conflicts with the one prominent in the NPRA. The AIBN's approach to accident investigations problematizes the notion of free choice through seeking the causes of individual actions and behaviours in the subjects' past, and thus casts doubt on the citizens' agency. The smaller the scope for relegating actions to the moral/legal category, the more circumscribed the liberal subject. The laying bare of the processes behind an action is a double-edged sword; if a process is implicit, opaque and crudely articulated, this may be a sign of the powerlessness of the actors involved, but it may also an indication of their self-determination (Star, 1990).

Risk objects are causal, and thus the proliferation of risk objects will infringe on the presumably non-causal (moral, legal and reason-governed) sphere of society. Law and Mol (2002: 10,) thus warn that "absolutist" safety work comes at a price: "Too much of one good undermines some other good". Arguably, the reports of the AIBN do not only go against the grain of liberal theories in their presentation of the subject; their focus on how public actors might prevent the individual tragedy can also be seen as an attempt at placing responsibility above autonomy, as advocated by an ethics of care. There might be a tendency, however, that the consolidation of the individual as an *object* of value, leads to a weakening of the individual as *upholder* of values, as individuals' actions, reasons or beliefs, once explained by reference to contexts, seem to lose some of their independent value.

There is a danger of overstating the break with the past that the AIBN represents. Obviously, the legal system has always taken mitigating factors into account in road accidents (see for instance Fedtke, 2003). Also, assessing the safety performance of different kinds of infrastructure is an established practice (explaining, for instance, the proliferation of roundabouts), and the existence of road safety programmes for children shows that the state does not take the formation of traffic-savvy citizens as a given, but as something to be constructed. The narratives of the AIBN are not completely novel, and they will not in themselves deconstruct the liberal subject – they are only stories. But, as Law and Singleton (2000: 769) argue, the difference between telling stories and acting realities "isn't so large". So far as AIBN's narratives and recommendations are included in road safety practices, at least in the sphere of road traffic, a slightly different kind of citizen is being enacted.

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NOTES

1 The risk of road accidents can be measured and reported in different ways, and the choice of measure is not innocent. The simplest measures, such as the absolute number of fatalities relative to the population, do not control for the number of driving licences or the number of cars in a country, for instance. The more frequently employed measure, used in this article, controls for the number of kilometres driven, which means that less affluent countries may perform worse, but also that efforts to reduce accidents through reducing car-dependency and traffic in society will be invisible in the statistics. This effect is reinforced by the fact that measuring risk relative to distance travelled will favour faster modes of travelling, implying that more sustainable modes, such as walking or cycling, appear relatively riskier than they would if, for instance, risk was measured through exposure time or the number of trips made. It is therefore a commonplace among road safety professionals that a shift to more sustainable forms of transport will typically lead to a higher number of road fatalities and injuries.

2 Brown (2001) is an interesting discussion of the notions of citizenship implicit in California's Electric Vehicle Program. In his account, however, the role as citizen is opposed to the role as consumer, as the Program is seen to promote a consumerist conception of citizenship. While the distinction between citizens and consumers can impinge on the role of road users in various contexts, it is not directly relevant to the focus of this article, which is the regulative and retributive aspects of citizenship as it related to individuals in a government-controlled legal and technical environment.

3 The reported numbers of fatalities and injuries are provisional until the publication of final annual numbers, usually by the end of May in the following year. Although the numbers do not usually change much, the process illustrates how the category of "traffic fatalities" is a complex construct: Only fatalities taking place within 30 days of the accidents are included in the statistic, and confirmed suicides as well as accidents assumed to be the result of sudden illness are excluded. Thus, the number of fatalities in the official statistics are usually, counter-intuitively, lower than the provisional figures published.

4 Interestingly, while young drivers did for a long time appear to be a very enduring risk object, this has now started to change: figures from 2013/14 revealed that the risk (per kilometre driven) of drivers ages 18 to 19 had been reduced by 40 % in four years (Bjørnskau, 2016).

5 In this light, we might also interpret the fact that the World Health Organization launched its World Health Day in 2004 under the heading 'Road safety is no accident!', as an illustration of how the traditional end-points of road safety have been challenged for some time.

We Are Standing Together in Front:
How Scientists and Research Groups Form Identities in the Life Sciences

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Abstract

Reputation building and visibility represent pressing requirements for living and working in academia today. These demands have been key to the corporate world and are acted upon through 'branding' practices. 'Branding' has further been shown to impact on employees and workplace identities. In academia, researching identity work is especially important because of a competitive funding climate that requires research groups to resemble an outstanding image and reputation. At the same time, stable jobs are scarce, bringing forth insecure and volatile environments characterized for example by temporary limited contracts and required internationalisation in scientific careers. Based on ethnographic work in globally recognized life science departments, I explore how individual and departmental identities relate. Thereby, I propose the concept of 'enrolling', that conveys how a research unit acts as a 'brand', and show how 'enrolling practices' produces stability through coherence and distinctiveness in individual and collective identities. My analysis thus allows a critical reflection on academia and the re-orderings in today´s universities that create pervasive demands for living and working.

Keywords: academia, branding, identity work, life sciences, research groups, scientist

Introduction

Today´s laboratories need to possess reputation, visibility and productiveness, in order to succeed in attaining funds, attracting international scientists and publishing successfully (Ylijoki, 2014; Fochler et al., 2016)[1]. These demands are further entwined with the necessity of evaluation and scientific performance that permeate today´s science landscape and invoke selective processes as a central force in academia (Hammarfelt and de Rijcke, 2015; Dahler-Larsen, 2012).

The need for reputation and visibility was tangible in my first visit in one of the research departments in which I did my ethnographic work - what I refer to here as the Random Austrian Science Department (RASD). I discussed my project with its director and one of his PhD students. We talked about the research project of the PhD student that I would be joining and agreed that I would be part of all lab-related activities, talks and meetings over the upcoming weeks. During this meeting, the director and his student also debated the work plan of the research project. When we started to talk about my stay, the director proposed that we could extend its length.

> Director: Would you like to get a degree from RASD[2]?
> Me: Well, I already have a master's degree in biology.
> Director: But not from *here*. You will get it in six months and a promise for endless interviews.

The meeting continued with laughter and jokes for a while before we went on to discuss how I could get involved into the PhD student's work as a 'helping hand'. So, what does this anecdote illustrate? By suggesting an additional degree from the research department to be gained during my stay, the director delineated RASD as being special. A degree there is supposedly worth more and clearly distinct from programs at other universities, such as my former research degree from a medical biochemistry laboratory. The distinctiveness of this department in contrast to others was brought forth in a joking mood. This continued throughout the period of the ethnography, when for example the director stated the need for *getting the genes* of RASD or when members continuously referred to themselves as "RASDies". These joking remarks made clear to me that references towards the 'specialness' of the place represent an integral part of departmental life and are worth investigating further, as they are relevant for the way in which both scientists and groups form an identity in academia today. So, as the director emphasizes the 'specialness' of RASD, he hints towards the importance of the research department´s name and quality by proposing a specific reputation and visibility.

Several studies have already attended to the increasing need for the creation of a 'good image' and a distinguishable or 'special' place for research (e.g. Wæraas and Solbakk, 2009; Steiner et al., 2013). Building reputation and visibility - known within organisation and management studies as 'branding' - is however mainly directed to a research unit´s exterior, for example to reach out to international scientists and funding agencies (e.g. Rindova et al., 2005). Steiner and colleagues too conceive of reputation as external dimension for building up the identity of a university, yet extend this conception and propose a model of "interconnectedness between organizational identity, symbolic identity, image and reputation" (Steiner et al., 2013: 411). Thereby, they convincingly show how organizational identity relates to the external perception of a university, and claim that researching identity formation is core for understanding underlying intentions and strategies in universities.

Moreover, building a collective and an individual identity as scientists in and of a particular place is challenging as research groups in the life sciences are described as increasingly volatile, "more-or-less stable and continually changing" in their composition of researchers (Hackett, 2005: 793). Scientists repeatedly join and leave a group due to project-related work and its temporary limited contracts, but also due to the need to progress their careers by moving from one lab to another, also internationally. As such, a constant struggle for research groups is to create intellectual and social coherence in scientific knowledge production processes. Moreover, as frequent re-assemblage of research groups poses challenges for a group´s internal relationships as well as its external perception, the formation of individual and collective identities is the main interest of this paper.

Since the academic world is intricately related to broader societal challenges, a lack of coherence has been prominently debated as a part of general societal transformations in Western cultures. This transformation is characterized by increased individualization due to the deconstruction of traditional formats of work and intensified demands of mobility and internationality (e.g. Beck, 1986; Giddens, 1991; Keupp, 1994). Furthermore, the lack of group coherence is in line with Bauman's argument (2004) that in today's society, with its changes and challenges, identities are characterized by a lack of stability in their local embedding. Thus, it is crucial to understand the identity work of researchers and their groups and departments, and reflect on how this identity work is acted upon. Thereby, I focus on the micro-processes by which scientists create their identities and establish group belonging in times of prevalent need for reputation and visibility in academia.

At the core of this paper is the argument that creating coherence and distinctiveness is central for research groups and departments and the ways in which scientists belong to them. In this piece, I specifically focus on the dynamics in one research department that was part of my study. It consisted of 6 research groups in which the boundaries between the department and the groups were constantly blurred. Hence, while RASD - which I conceive of as a conglomerate of groups - is at the centre of my analysis, the blurring boundaries between group and department allow a multifaceted gaze: *How do identities of scientists and their research groups/department relate to one another?*

This question is subdivided into a set of questions: *How do scientists inscribe to the collective of a research group/department? How, in turn, does this collective incorporate these scientists? And how does the relationship between scientists and their research group/department have an impact on the building of individual and collective identities especially in today´s competitive academic landscapes (for instance through branding)?*

In order to analyse this process, I propose a new perspective on the concept of 'enrolling' - that shows how the scientists are incorporated in their groups and departments. This process also shows how the scientists inscribe to the collective of their groups and departments. Studies on research groups analyse, for example, enculturation practices of novices into their groups (Delamont and Atkinson, 2001; Traweek, 1988) or how social processes are enmeshed in building and maintaining a group (Davies and Horst, 2016; Hackett, 2005). I, however, argue that 'enrolling' serves the group's outside image and is simultaneously related to internal practices, showing how scientists are part of an academic culture that reveals characteristics of a 'brand'. Thus, I claim that understanding 'enrolling practices' sheds light on how scientists perceive of themselves and their groups and provides further understanding of how 'branding' takes on essential roles in academia. In that sense, 'enrolling' allows a critical reflection of the implicit assumptions and values present in academia today regarding how scientists should *be* and act as part of a research community, and at the same time offers insight into how scientists engage with today´s demands of being visible and having a reputation.

In what follows, I first discuss in more depth the broader context of transformations in academia that have taken place. I then explain how I conceive of scientists and identity work, how research on 'branding' and identification relates to my work and how I use the concept of 'enrolling'. In the empirical part I demonstrate how and through which practices the researchers are being and becoming part of RASD. On the basis of this analysis, I argue that the concept of 'enrolling' helps us to understand how individual and collective identity work is entangled and enables a critical reflection on this relationship and its tensions.

Transforming universities

The need for a recognisable and visible identity that I have made central in this paper emerges out of broader trends in academia. In this section, I discuss a number of relevant aspects of living and working in science in the context of institutional transitions, such as the implementation of management practices in academia, the emergence of evaluation processes, the dependence on third-party funds and increasingly insecure working environments.

In light of larger cultural and economic shifts in which managerial approaches become ever more important, Maasen and Weingart (2008) show how practices from business science, management and corporate advisories are increasingly implemented in academic landscapes. Similarly, Chandler and colleagues describe how corporate management practices have found their way into academic institutions, bringing managerial restructurings in the wake of New Public Management (NPM) (Chandler et al., 2002). This transition is described as fostering an integration of a managerial logic inclined to incorporate innovation, market concerns and commercialisation into a more traditional logic of universities and their higher education values (Shore, 2008). The implementation of such principles then accounts for a reorganization of research according to requirements for performance and competition (Fochler, 2016). This accordingly impacts research practices and "our lives, our relationships, our professional identities and the manner in which we conduct ourselves" (Shore, 2008: 281). Consequently, there is a need to understand both the narrative as well as day-to-day practices that guide researchers' identity work - for which these transitions are of the utmost importance.

Further studies on transitions in academia point out how scientists are involved in evaluation processes and how competition and visibility are intricately related to these (e.g. Fochler and de Rijcke, 2017). Hammarfelt and colleagues (2016) for example analysed researchers' online profiles in social networks and their relation to how they quantify themselves as part of such structures. Connected to the demands of being reputable and visible - the scientists are immersed in a game of representation that at the same time evaluates their market value. Other studies show that there is an increasing stress on evaluative devices to measure scientific performance standards as part of quality assessment and evaluation criteria (Chandler et al., 2002; Felt et al., 2013; Fochler et al., 2016). This is for example mirrored in what Power calls the "audit society" (Power, 1997) in which evaluation procedures as part of a so called "evaluation machine" (Dahler-Larsen, 2012) are described to affect scientific research practices. Power (1997) claims that audit changes how people are perceived and how they position themselves for instance towards evaluation and performance indicators. Yet policy rules are not simply being implemented by force but draw on academic institutions as "*actors* of these policies" as they implement and translate them into institutional processes (Stöckelová, 2014: 437). While these studies show that distinctiveness is a common benchmark in universities evaluation regimes, there is a need for explicitly drawing attention to how today´s demands are acted upon within everyday lab contexts and within the relationship of researchers to their groups and departments.

Another point of contention is how the decrease in direct state-funded research has fuelled competition for third-party funding and resources (Hornbostel, 2001) and how this relates to temporary limitations in funding possibilities and employment contracts for scientists. In this context, the notion of 'academic capitalism' - furthering market as well as market-like behaviour due to competitive funding from external resource providers - has become a prominent neoliberal practice (Hackett, 2014; Slaughter and Leslie, 1997; Kleinman and Vallas, 2001; Linková and Stöckelová, 2012). These changes can be seen in in the implementation of a new university law in Austria in 2002 and government-induced austerity measures. Due to a subsequent re-shaping of universities, this law led to an intensification of universities' autonomy (Felt and Glanz, 2003). This also had an impact on e.g. the implementation of quality criteria (Fochler, 2016), the availability of funding in relation to short time contracts and an increase of externally funded scientists up to eighty per cent (Sigl, 2016). Moreover, temporary restrictions on contracts manifest themselves in uncertainties for researchers within a 'regime of uncertainty' - linking social as well as epistemic insecurities - leaving scientists in need of deploying coping strategies (Sigl, 2016).

Hence, individual career prospects are intricately related to institutional changes and substantial reformulations of what it means to pursue a career in science. In this context, third-party funding, visibility and "attaining a good image" form key currencies for university departments in order to attract funding, highly skilled personnel or to secure high-impact publications (Ylijoki, 2014: 70). These key currencies contribute to the requirements for gaining or keeping a job in science. Yet aside of institutional re-orderings, we need to understand the practices that not only affect the production of knowledge but how scientists relate to and identify with their groups and departments. Thereby, we can understand further how orderings (Law, 1994) have a profound impact on the norms and values against which identities are being built. In this vein, I argue that re-orderings of universities foster re-orderings in scientists' accommodation to their place of work, and their identification and sense of belonging within research groups.

Scientists and identity work

Taking into account these vast changes in academia, some studies focus on the different roles scientists inhabit, such as shifts from traditional 'ivory tower' researchers to 'entrepreneurial' scientists (e.g. Shapin, 2008; Henkel, 2005; Lam, 2010). For example, Owen-Smith and Powell (2002) conclude that identities are influenced by the "economic, institutional, and scientific transformations" as these "are changing the meanings that academics attach to scientific careers" (p.24). In a similar vein, Hakala (2009: 186) focuses on the moral framework of young scientists under permanent change and concludes that a more stable environment would "create possibilities for more coherent identities". It is also argued that coherence has been further disrupted by a "filter feeder" phenomenon with research groups (Hackett, 2005: 793), wherein researchers arrive and drop out constantly, impacting for example on publication practices and authorship distribution. But while Hackett makes a profound analysis of the tensions that underlie the researcher-group relationship, I instead focus on how the researcher-group relation counteracts the "filter feeding" process by bringing forth temporary stability for the researchers.

I further take stability as a concept deeply entangled with coherence and distinctiveness. Coherence refers to "a sense of continuity and recognizability over time and situation" relating experiences and minimising fragmentation, and distinctiveness refers to the unique definition of somebody, sometimes "shared with others" but still distinguished as 'one' (Alvesson and Willmott, 2002: 625). Coherence and distinctiveness thus constitute central elements of identity work. In line with this, research groups have been shown to rely on coherence for recruiting and motivating scientists to work for a common goal (Griffith and Mullins, 1972) but also on distinctiveness in order to create a distinguishable independent image (Hackett, 2005). Hence, creating a stable reference frame and perception of the group is key for the individual and collective identity work of scientists and their research groups.

Identity work and 'branding'

The need for coherence and distinctiveness is further mirrored in management and organization studies that have made identification and identity work of employees a core interest of their work (e.g. Brannan et al., 2015; Alvesson and Willmott, 2002; Vallas and Cummins, 2015; Rodrigues and Child, 2008). They show that 'branding' informs the identity work of employees' and thereby the authors reinforce questions regarding identity work. Balmer (2001) for example indicates, that in order for a company to be successful, it must create a brand that is distinctive and emotionally meaningful for users. It must also create a corporate image, culture and a reputation employees can relate and commit to. This formulation is an extension of 'brand' definition solely based on a product to a framing that includes all people who are important for a company, such as managers or stakeholders (Hatch and Schultz, 2008; Balmer, 2001) and employees - who become shareholders of the brand (Schultz et al., 2002). Another prominent example is Kunda´s study (2006) of a technology company in the United States, which emphasises the coherent framework that stands for the ideology of a company. This framework serves to internally control and reproduce a specific culture with "rules for thoughts and feelings", "mindsets" and "gut reactions" as well as a strong commitment to the company and its goals (Kunda, 2006: 7). These studies have however explicitly focussed on companies, missing out on other realms of work and how the formation of a brand might look like outside of the world of business, such as in academia.

Keeping both 'branding' and identity work in mind, I understand identities in the context of today´s science regime as essential "objects, goals and resources" that are subject to "strategies, tactics and regulating procedures" (Rose, 1998: 9). In this vein, identity work means to work for one's 'self' as a project, a corporate identity, with the aim to excel and create self-fulfilment (Rose, 1998; Bröckling, 2007) while being at the same time highly regulated and controlled (e.g. Alvesson and Willmott, 2002). Against this, I analyse how identity becomes established when scientists 'enrol' to RASD. I also ask what happens when they purposefully connect and manage their social and self-identity as part of the department (Watson, 2008) but also when they have to fulfil standards of an ideal type scientist. Accordingly, I regard identity work as the construction of coherence and distinctiveness (Sveningsson and Alvesson,

2003) that are being done in practices that are characterized by 'branding'.

Identity formation between individuals and research groups: 'Enrolling'

To open up *how* scientists and their research department interact, this paper draws on a long-term engagement with the life science field. It is mainly based on - but not limited to - a three-year research project, which included fieldwork dispersed over two research sites in Europe and the US, respectively. Aside of the daily lab work and numerous informal conversations during and after lab work, I have conducted 17 scheduled interviews.

For this paper I mainly draw on participant observations and interviews at RASD - a "research department" consisting of 6 research groups. RASD comprised about 50-60 scientists of all levels during the time of this study. The total number depended on a variable number of rotation/intern students, as well as doctoral candidates in different stages of writing up their thesis or experimental work. All research groups featured distinct projects while collaborating with each other on a daily basis and working in the same sub-discipline. As part of the observation, I was following two doctoral students and took part in their daily lab routines, such as working side by side, helping with cleaning and attending weekly lab and social meetings. In addition, I was invited to join a PhD retreat in the countryside, observed a visit of the minister of science, a scientific workshop with international guests and the opening ceremony for a newly founded research platform.

By participating in the group's daily research endeavours, I aimed to observe commonly shared understandings and interactions within the research groups and relate individual experiences to stories of the group. Capturing impressions and experiences in the role of a participant observer (Bryman, 2004) and grasping an ensemble of "local practices whose ways and workings are only accessible through a competent practitioner's in-depth experience and familiarity" (Pollner and Emerson, 2001: 123) were at the forefront of my concern. Due to my former background as a microbiologist, critically engaging with my role as participant observer was crucial and helped to reflect on what kinds of stories I was being told and searched for (Schönbauer, 2017). In the formal and informal interviews I engaged with senior scientists (postdocs, group leaders and directors), as well as junior scientists (PhD students, master students), and technicians[3]. As a result, the analytical material consists of extensive fieldnotes and interview transcripts that provided a key source for creating codes and memos (Charmaz, 2006).

Following the creation of initial codes and intense memo-writing, a key matter of concern turned out to be the relationship between the researchers and their workplace as well as the proposed 'specialness' of RASD. Considering this, I was mainly interested in how the scientists would make sense about their stay at RASD, such as reasons for applying, the specificities of the department, daily life experiences and how they would contrast former experiences to their current life at RASD. In subsequent steps, I focused on the respective everyday practices and narrations that inform this relationship, guiding how identity work at RASD is accomplished and how scientists accordingly 'enrol to' and 'are enrolled' to their workplace.

In this paper, I take 'enrolling' as a process through which scientists and their research groups relate and produce stable configurations following the building of alliances and the definition of common interests and concerns. 'Enrolment' has commonly been used in Actor Network Theory (ANT) to understand how scientists produce knowledge and "enlist people and objects behind their banner", thereby assigning particular roles in this process (Epstein, 2008: 803; Latour, 1987; Callon, 1986). Hence, 'enrolling' speaks about anchorage and durability and about the manifold negotiations - including "trials of strength and tricks" - that determine identity (Callon, 1986: 206). More recently, 'enrolling' has further been represented as a mission of the scientists themselves, for example when they recruit or accrue research subjects for their studies (Epstein, 2008: 803). The concept has for instance been used in the case of genetic DNA testing to understand how different articulations of indigenous people and scientists are part of identity-making processes and how these articulations are used for the 'enrollment' of the tribal members (TallBear, 2013).

In line with Latour (1987), my understanding of 'enrolling' unfolds in two modes: first, 'enrolling' depicts how actors are enrolled to make them believers and responsible for dissemination "across time and space" (Latour, 1987: 121); second, 'enrolling' controls behaviour in order to create coherence. Accordingly, new allies are recruited and form a resource that is "made to act as one unbreakable whole" (Latour, 1987: 132). Consequently, I attend in this paper to the on-going enculturation practices at RASD with a focus on identity work at all stages of a scientific career that are continuously part of 'enrolling'. I conceptualize 'enrolling' as having an impact on the outside perception and reputation of a group and its name, on how scientists relate to its mission statement and how they belong to the group within everyday collective work.

Hence, 'enrolling' shows that the interconnectedness between scientists and the research department characterizes a profound dependency and orderliness for how identity work is done and might not be done otherwise, meaning that it is controlled in order to ensure coherence (see also Latour, 1987). I show how the process of 'enrolling' creates mutual dependencies between researchers and the department as it fosters a climate of commitment, persuasion and control. This allows me to trace how scientists "participate in the way they are governed" (Lorey, 2011: 4) as they 'enrol' to RASD and become 'enrolled'. 'Enrolling' in this sense opens up the relationship between scientists and their local environment, but also exposes the pressure on scientists to conform to the demands of today´s cultures of scientific work.

In the following sections, I will show how 'enrolling' provides ground for the identity work of scientists at RASD. In doing so, I will focus on how the scientists relate to the department and its famous name, analysing how the researchers merge with its collective representation, how a mission statement is framed and how everyday tasks are operated to create an engaged collective. Thereby, I will open up how 'enrolling' guides the relationship of the scientists with the department in ways that benefits researchers and the department, but also how 'enrolling' is used as a "tool for management, (and) control" (Stöckelová, 2014: 445) and thus is characterized by tensions.

Relating to a famous name

In order to be successful, a research unit or laboratory in the life sciences has to define its territory and be renowned within a certain community of scientists, as an important place and name in the landscape of research (e.g. Knorr-Cetina, 1999; Vermeulen, 2017). In the following, I will show how the scientists relate to RASD and its perception as special and famous department. In doing so, I will refer to how the name was referenced in different ways, for example when scientists talked about publishing, recruiting students, advancing in their career, when mobilizing impressions of outside visitors for their own relationship with the place, or even when voicing critique about the department.

When asked about why they decided to join RASD, the scientists gave similar answers concerning the reputation of the department. They told me about the international standing of the group leaders, about the extraordinary technological equipment that "resembles the equipment of a Max Planck institute" (Marie, group leader), how RASD is at the "cutting edge of the field" (Noah, PhD student) and that it represents a challenging environment (Matthew, PhD student). These characteristics are perceived as being important for a career in science, while also creating a particular reputation and a place with a recognisable name.

One scientist told me about her experiences of publishing a paper and how RASD impacted on the publication process:

> Well personally to be honest I also think I will profit from being here, since people want to join my group because of the name, RASD. This is also quite funny. Of course you do have, so you do have a name for yourself, I have been reading this right now in the review comments, that, so that the name, that they know who I am.
> (Marie, group leader, translated from German original)

Marie is recounting her experiences as a newly employed group leader at RASD. Since starting, she has tried to publish papers and attract students for her evolving research group. As part of the publication process, she experiences acknowledgment for her affiliation within the reviewers' comments on her submission. The reviewers Marie talks about wrote statements such as "we expect something from her", while reacting positively to the development of her career, her CV and her future career "in this environment" (quotes from interview). In that sense, the reviewers are acknowledging her individual name as a scientist by relating it back to the department and by attributing credit to her future developments as a scientist at RASD.

Marie was amazed when realising how these reviewers related her name to the research department and subsequently expected an increase in attractiveness of her profile in upcoming recruiting efforts of new employees. New members of her group would not only see her name but also the label of the department she is working at. Since her name is already part of the wider RASD-cosmos, students want to join her group because of it. In doing so, they reinforce something similar to the reviewer comments: an appreciation of the place and its reputation. Her self-identity as scientist is thus intricately connected to the department for progressing in a professional career. This relationship formulates a "brand narrative as promise" (Brannan et al., 2015) connecting her career-related anticipation to disciplinary visibility and a trustworthy reputation. This is especially visible when Marie explains: "you are the product that you wanna sell. And this is not going to change. Because in every grant proposal you will be the product again". Accordingly, RASD has intriguing effects for her career, as she continuously needs to sell herself as a 'product' for third-party funding and for advancing in her career.

Another example of RASD´s well-known name and how it helps scientists to establish a career and identity, is tangible in a lunch break conversation I had with PhD students about its 'image':

> Ben was interested in what people from the outside think about the department. He told me that there were some visitors who mentioned their impression of RASD. The visitors imagined RASD as very competitive department where everyone seemed to be stressed and under pressure because of numerous meetings and lectures within the department that members have to attend. The guests declared RASD as unique but probably exhausting to work at. Ben said that it was really interesting for him to hear and kept on referring to the department as "different" in terms of everything. (…) Later in the same lunch break, Ben stated that the group looks homogenous from the "outside" and Julian added that it would make a "good" image. Matthew followed up on that: "well, that is actually the reason why I came here". (fieldnote day 23)

In this lunch discussion, the PhD students mobilize impressions of visitors from outside as resources for their own evaluation of the place. The visitors comment on the specificity of RASD as well as its competitive character by stressing that it would be an exhausting place to work at. This assessment of its competitiveness is not only based on a list of publications or third-party funded projects, but on perceptions of exhausting work schedules and a busy departmental life. When recounting tales of visitors, the PhD students also draw on their own image of RASD and attribute an essential role to its appearance. The department accordingly becomes a symbol to which the members belong to as "the best and the brightest" resembling a competitive elite of a successful entity (Kärreman and Rylander, 2008: 117). This promotion is simultaneously done by insiders and outsiders that confirm the ambitious perception and accredit the members a status as part of a competitive elite.

The relation between insiders and outsiders to RASD and how the scientists are relating to a famous name also manifested itself, for example, when a guest scientist presented his work as part of a job application at the department. In an invited talk, he introduced himself referring to the reputation of the department: "I am happy to be here not only because of the famous RASD but also because of the lovely weather" (fieldnote day 16). In other instances, the self-identity of insiders and the collective 'brand' was related in informal encounters, such as within meetings, presentations, or when joking in daily lab life when the scientists call themselves "RASDies". In a progress report meeting, in which students and postdocs of the research department presented the state of the art of their work, an undergraduate student working on his master's thesis made explicit "thank you" notes to the scientists of the department: the "RASDies" had served as helping hands during his stay, spent breaks together with him, and shared his passion for playing the video game "Starcraft" (fieldnote day 5). Through these acknowledgements, the insiders - who show their dedication to colleagues - simultaneously appreciate the research department as a whole, just like outsiders do.

In contrast to this appreciation of RASD, department members also referred to their colleagues as "RASDies" through jokes that allowed the voicing of critique. For instance, one PhD student joked about being sick with "RASD-itis", which was "the illness of being at RASD" (fieldnote day 20). Through making fun of the name in a joking mood of being sick with it, the researcher interrelates illness and the department signifying a tension in the relationship between him and the place (Mulkay and Gilbert, 1982), pointing out that RASD is not necessarily an idyllic place. Yet this joke is not only a sign of the relationship between individuals in the lab (Knorr-Cetina, 1999); this ridicule also indicates subversion (Michael, 1986) and the building of a 'resistant self' (Collinson, 2003) as the scientists express their discontent with the department. This critique however happens without alienation from the collective identity as they relate their self-identity as "RASDies" to the place and in so doing reinforce the imagination of a competitive or exhausting workplace. Consequently, such jokes offer insights into how the scientists criticize RASD and its rules but also how its image is performed.

Accordingly, the scientists perceive RASD as a 'special' and exclusive department resembling a distinct 'brand' that has a promissory function for their career. The name of RASD establishes a reputation and visibility that helps to create a 'brand' for the department but also fulfils the need to be visible for individual scientists, such as when the name serves to establish a career and an identity as researcher. Furthermore, the collective 'brand' and the self-identity of members are connected, for instance when members explicitly draw on the department´s reputation for their own valuation of the place. 'Enrolling' then means that scientists 'enrol' to RASD because of its competitive and strong image. At the same time, 'enrolling' also opens up a contradiction: the researchers form small resistances through jokes, signifying a tension that points towards RASD as a demanding environment.

Merging into a collective representation and mission statement

In line with the making of an internationally recognisable name, RASD is actively merging its researchers behind a common mission statement and outside representation. The department is presented as a "motivating and internationally competitive scientific environment", and it is explicitly mentioned that it has "it all", a mixture of young and experienced scientists as well as a scientific network for collaborations that offers ample opportunities for future scientists (quotes from homepage). RASD´s collective representation and - connected to that - its overarching mission statement is built for example by the use of metaphors and through its online representation. The common representation is also criticized, such as when individual members oppose the collective framing.

To further understand RASD and its underlying mission statement and outside representation, the director and his authority is key. This is exemplified by an instance in which the director told me that he had originally metaphorically conceived of the department as a kind of "pirate ship", even before it relocated to Austria. As metaphors conceptualize our everyday social realities and structure how and what we argue (Lakoff and Johnson, 1980), the pirate ship metaphor of the director guides his imagination of an autonomous and untamed workplace that distinguishes itself from everybody else in an exceptional way, namely in how it represents itself in the first place. The ship metaphor is however still mobilized for present conceptualizations of the department:

> This is actually a well-functioning ship that is finding its way through. Unperturbed. Breaking the ice (laughs).
> (Jonas, director of RASD, translated from German original)

In this quote, using a similar nautical metaphor, RASD is imagined as an icebreaker that holds its course regardless of disturbances and turbulence. An icebreaker is a stable and powerful ship that continuously breaks the ice as it moves. When the director understands RASD as a pirate ship at the beginning and an icebreaker later on, steadily manoeuvring uncertain terrain, he makes an argument about continuity and stability counteracting the fast pacing ephemeral science regime. Using this metaphor for a scientific research department not only makes it a consistently floating entity, an enterprise that follows a course and transmits a particular vision, but also represents the need for being different to others.

The distinctiveness of the ship is brought forth further when the director said about a new member: "it took her a while to get that RASD gene". This was in reference to a new female group leader who needed to develop an understanding for jokes, which was an integral component of how the scientists interacted. The new member came "from outside" and "knew different cultures" since she was working abroad before (quotes from interview with director). By using the gene metaphor, the director imagines a specific collective identity, namely that of a 'family' that lets him feel "more secure, less alone" (quote from interview with director). What could be more essential than becoming part of the laboratory's genome and metaphorical 'family' by *getting the gene*? The family-collective has been further referenced as providing a backup when its members e.g. collaboratively think about research projects or if someone is "in state of a crisis" and family-members help each other (quote from interview with director). While this creates stability, it is also associated with an exclusive membership, as the 'family' mostly refers to an epistemic and social community on the group leader level.

Aside of the metaphorical references, the strong collective representation of the department can also be found on its website. It shows a range of scientific topics and notifications, such as the "news feed" mentioning successfully granted research funds, celebrations of honorary titles, or published papers. It also includes announcements of new members, PhD exam celebrations with self-crafted costumes or even when scientists became parents. More informal gatherings are highlighted too, such as a poker tournament, the RASD football team, barbecue evenings or leisure time excursions of the department to nearby places. As Lorenz-Meyer (2012) argues in her study on the performance of excellence, I find that the displayed get-togethers on the homepage enforce a specific collectivity that creates an imagination of an excellent international location and a pleasurable place to stay.

This collective representation is also performed on the member section of the homepage, which does not split up the researchers according to their group memberships but rather by their hierarchical position, such as "scientists and postdocs", "PhD students" or "faculty and staff". The director remembers the decision of group leaders and professors to represent RASD together on the homepage:

> We always said: "we are standing together above". And we show that (note: individual research groups and respective affiliations) only far down at the homepage. (…) Of course we have "news" (note: on the homepage) where special achievements can be celebrated. So this is what we are emphasizing anyway. And we are also allowed to sell ourselves to the press individually. You don't have to say RASD there. But somehow it should be clear. It is an enterprise. (…) Although not in research. (…) But in principle there is this idea that we are standing together in front. (…) We have a group of 56. (…) So, as RASD we are clearly distinguishing ourselves (note: from other research groups/research departments).
> (Jonas, director of RASD, translated from German original)

Standing together above or *standing together in front* accordingly means to work side by side as strong collective that has a common external recognition and as entity that would help each other in creating a safe and sound space. This musketeer-like attitude mostly refers to group leaders and creates an imagined community that is "communist-like" (quote from interview with director) with all leaders helping each other regarding concerns with funding or employees. In this sense, being ahead of something generates a collective vision of the department as "we". While the research groups at RASD follow their interests in slightly different directions, their collective framing serves as an overarching scaffold for being together through social gatherings and news on the homepage. Thereby, outsiders should first and foremost recognize RASD as an "enterprise". This conceptualization produces a common imagination, for outsiders, of an entity that is capable of persuasion. Following this imagination, the research department resembles an environment in which the individual scientists have to merge with its collective framing.

The coherent collective representation on the homepage is also disrupted when, for example, one of the professors featured his group as distinct part of the department by uploading a group picture and announcing the groups' members on his personal university homepage. In the decision to feature an individual profile aside of exclusively following the departmental frame, he configures his own vision of a group. The group leader acted upon the contradictory demands of becoming an independent individual while working in the means of an overarching frame. When asked about this group leaders' decision, the RASD director mentioned that this would be a sign of "small egoism" and showing one's possessions, but also that everybody would have the possibility of "selling oneself individually" while remaining part of RASD. As establishing an individual profile is, however, essential for maintaining a career in science (Müller, 2014b; Felt et al., 2017), the group leader challenged the collective representation and constructed a generative choice for his career, while using RASD as a resource for his professional career.

Hence, RASD exerts a collective representation as an enterprise to the outside and invokes the metaphor of a 'family' for some of its members inside. The depiction of RASD as 'enterprise' relates to it being an entity capable of persuasion and competition that conforms to current needs in academia. The 'family' instead can be understood as tied to an imaginative repertoire of care, safety and responsibility (Davies and Horst, 2015; Fochler et al., 2016) or a way of escaping loneliness in academia (Felt et al., 2010). Thereby, the members are expected to 'enrol' their self-identity to its collective, and adapt to its culture and mission statement. This opens up how scientists conform to and perform a collective framing. However, this is also problematic, especially if there is little room for an outside representation of its members, who are in need of creating an individual portfolio when progressing their career.

Operating everyday tasks through collective engagement

Similar to the RASD collective that is being built in a common representation and as part of the director's mission statement, the scientists at RASD also experience a collective everyday life. Life science research groups are characterized by the need to commonly organize the laboratory, such as when collectively caring for daily chores in housekeeping work (Garforth and Kerr, 2010). In the following, I will show how scientists relate to the RASD collective in the spatial distribution of lab spaces, through being involved in housekeeping work, when voicing critique on the daily and weekly schedules or on the implementation of standard operating procedures.

A collective everyday experience at RASD was intentionally created through the common internal spatial structure of the department: at RASD, the distribution of bench spaces was an explicit matter of concern[4]. For instance, all scientists (postdocs, PhD students) were assigned to lab spaces not according to their research group affiliations in order to enhance "interaction" among the "RASDies" (Felix, professor). Aside from distinguishing between undergraduates and more experienced researchers, such as PhD students and postdocs, there was no other criterion determining one's bench location. Although the distinct research groups materialized in time and space - when, for example, team meetings or social events were organized - the visibility of each group was non-existent in office spaces. This commingling was destined to dissolve boundaries between research groups by providing opportunities for communication regardless of one's belonging to a lab. Similarly to Kunda´s (2006) study on the collective experience and behaviour in a Tech company, I find that dissolving clear group affiliations provided a common ground for a collective and coherent experience among RASD members.

Another example of how the scientists collectively experience everyday work relates to the expectation of their commitment in doing chores. Felix, a professor at RASD, reflects on what it would take to be working in science and states, that aside of scientific efforts, working together and learning how to interact with a variety of people would be a main matter of concern. He explains that engagement in shared tasks, such as housekeeping work (e.g. cleaning, defrosting, storing), is key for the organization of the department and is a compulsory part of working as a researcher. So students have to learn to fulfil chore responsibilities that are not explicitly related to their thesis but that instead help to operate organizational or even social efforts at RASD.

> (…) the engagement for the bigger picture. So, the realization that one is an individual in this department, but that this (is) what the department stands for as a whole to the outside. That this is important for one´s own career sort of too. And that this is why you maybe also do stuff, and take part in stuff that doesn't provide you with a lot of benefits for the PhD, diploma thesis, not as a clear-cut advantage. That means you have to get involved in the lab, you also have to involve yourself in topics concerning the department. (Felix, professor, translated from German original)

Reflecting back on his former side jobs during his own studies, Felix remembers that everyone has duties to fulfil, regardless of the hierarchical position. "Feeling responsible for everything" appears as main criterion for working in his group and at RASD. He further relates the dedication of individual scientists to collectively shared tasks with the representation of the department "as a whole to the outside". In this sense, participation in mundane tasks of the department becomes entwined with both a passionate and trustworthy "everyone is on the same boat" metaphor (Law, 1994: 179), as well as a dedication for a research department that in return directly impacts on one´s success in science. Hence, all scientists (except for group leaders) had to engage in common collective care work. This is especially important due to the prevalent individualized work mode that widely affects postdocs and their career-related pressures (Müller, 2014a). I find that the collective responsibility at RASD counteracts these individualization procedures and creates room for collective care and engagement.

Another example of collective engagement is the scientists' commitment in doing chores: cleaning commonly shared instruments and lab spaces - called "doing the labslave" - followed a weekly schedule. All employees (postdocs, PhD students) had to take part and care for waste management (when included in this procedure I was referred to as the "labslave assistant"), such as autoclaving waste, and collecting dirty glasses and washing them. The "labslave" role was outlined according to a rotating schedule. As "this system is keeping one person responsible for an entire week every few months" (fieldnote day 2), these duties are not done by choice but by obligation. The "labslave" builds a setting in which scientists become metaphorical part-time slaves, doing waste management for the common good. This joke draws on the unexpected congruence of being a scientist and being a slave (Mulkay and Gilbert, 1982). It also provides a reference towards the formal discrepancy between scientists as competitive and visible (as demonstrated in the former sections) and scientists as resources. Yet no matter if done voluntarily or not, the scientists at RASD are contributing to the common good by dedicating time to housekeeping work.

At the same time, the scientists are critical about weekly schedules that structure their days through meetings, presentations, lab cleaning dates and other obligatory participatory actions. This can be best exemplified within an encounter I had while receiving an explanation of a statistical program, "wordle", a tool for demonstrating the most commonly used words in a text, by two PhD students talking about mandatory tasks and time schedules:

> PhD student 1: I would really like to do that (note: using wordle) with the RASD e-mails and see which words pop up the most.
>
> PhD student 2: I already have a prediction. Maybe: "attendance is mandatory"; "Cleaning is mandatory". (fieldnote day 19)

This encounter refers to the daily and weekly temporal schedules the scientists had to follow. The

PhD students were not only bemoaning the regulation of their days but the lack of control over their own temporal schedules. While I do not think this conversation contradicts the necessity of meetings for scientists, it exemplifies the department members' engagement as well as their physical attendance in meetings to be a formal requirement for their work. This talk also shows that few organizational issues are left to chance as "there is a lot of eyes and ears always making sure you are doing the right thing" (interview quote, PhD student). In line with Collinson's (2003) study on how workers conform to their authorities, my argument is similar: the scientists have to conform as dedicated members of a department while subordinating their selves to the organizational authority.

A final example of how the RASD-collective is structured for collective engagement and how the scientists were criticizing rules, is the implementation of "standard operating procedures" (SOPs). These procedures were intended to regulate scientific protocols and standardize *how* these should be written, from bench to kitchen rules. Implementing SOPs aimed to normalize all lab-relevant protocols in format and length in order to provide coherence in subsequent steps of writing, collecting and storage even when scientists leave RASD. In an organizational meeting, the researchers discussed which SOPs should be written, how and by whom, and delegated responsibilities for their making. While all standards would be stored on the department's server, some of them featured explicit instructions and were taped onto the respective technical devices. Through this, scientists were provided with a clear overview of what to do where (such as how to work at the DNA staining and detection place). However, many scientists did not regard the SOPs as particularly useful. The standardization efforts were perceived controversially in that they were not accredited to be "scientific work" or in compliance with academic researchers as "free" individuals. Thus, a 'go and ask' practice was the most common way of getting to know a research method, without needing to read a manual, follow a chain of command or rely on SOPs as written instructions of how to handle lab equipment. Accordingly, the lab members would rather go to their colleagues in person and ask about specifics of the actual method instead of looking up an SOP on the department's database. In this case, scientists relate by resisting rules and regulations of the department and simultaneously creating an alternative form of interaction through opposing rules.

In conclusion, the organization of everyday life and work provides insights into the relationship between the scientists and RASD. Everyday tasks, such as lab housekeeping or engagement in chores, are operated conjointly. Participation in daily tasks, or the spatial distribution of work places regardless of research groups, can thus be understood as tied to the need to counteract prevalent individualization practices in academia today (Müller, 2014a, 2014b). At the same time, RASD forms an environment that is built along clear rules and regulations of how to engage, such as when members are controlled by tight schedules. Hence, the scientists - mostly PhD students and postdocs - are 'enrolled to' and 'enrol' as engaged and dedicated members of a common entity. This further opens up how identity work is oscillating between conformity and resistance, building a self that has to perform well in an orchestrated environment while also forming careful relationships that stabilize the collective in its everyday work.

Relating through enrolling: identity work in between stability and control

The need for reputation-building and visibility has been described as crucial for scientists in order to enhance attractiveness and gain money or equipment (see for example Ylijoki, 2014; Wæraas and Solbakk, 2009). This is even more important since funding possibilities have increasingly shifted from state-subsidized to third-party based, thus intensifying competition and the importance of a distinct well-known image. At the same time, research groups are continually changing their composition of researchers (Hackett, 2005) making it crucial to establish internal coherence. While many studies have carefully paid attention to critically reflect on how academia is constantly changing in response to these demands, I have further shown how a research department resembles important characteristics of a 'brand' and thereby acts on today´s requirements in academia. Accordingly, re-orderings in academia that are oriented towards a regime of selection and competition provide a baseline for 'enrolling', which partially counteracts but also conforms to these demands.

In this paper, I have shown how scientists and a research department relate in a state of crucial dependence, and how this dependence is acted upon in 'enrolling' and the respective identity work it produces. In short, I have demonstrated that scientists 'enrol' their self-identity to RASD as it represents a promising repository for their future career. The department´s exclusive identity as a competitive and 'special' place is brought forth through internal and external references of scientists towards its name. The 'specialness' is also traded in its coherent collective outside representation and within the internal mission statement of the director, who imagines RASD to be an enterprise and a 'family' (for some of its members). RASD's collective structure is further tangible in day-to-day experiences as scientists are expected to 'enrol' through engaging in cleaning and care work. Hence, 'enrolling' demarcates a relationship that produces alliances between scientists and their research department and configures a mutual dependence.

Yet 'enrolling' also indicates tensions. In line with perceiving 'enrolling' as a way to inscribe and control actors (Latour, 1987), I argue that RASD exerts control while scientists become part of it. As in Kunda´s (1995, 2006) work on a Tech company, the "company culture" serves to shape and guide the member´s roles by defining rules and reference frames. In this regard, 'enrolling' also resembles a brand-centred control in which all scientists take part, embracing RASD as proud representatives. Moreover, this enthusiasm - when taken up by outsiders - confirms the scientists' perception of the department as competitive and visible. The resulting tension when scientists 'enrol' and 'become enrolled' has been verbalized through on-going jokes. These jokes potentially allow what Collinson (2003) calls a "resistant self" to flourish – a self that simultaneously allows critique and appreciation of the place. In sum, 'enrolling' provides insight into how commitment, persuasion and control frame the relationship of individual and collective identity work.

In order to unpack 'enrolling' and its impact on identity work further, it is crucial to bear in mind that it is an ambiguous process. 'Enrolling' depicts how scientists relate to a research department that provides an international and competitive reputation and a collective environment. Thereby, the scientists are able to meet today´s academic demands for distinctiveness while also finding temporary stability. Hence, the researcher-group relation brings forth temporary stability for the researchers and counteracts the lack of coherence in groups. However, next to offering stability, the scientists are at the same time becoming part of a controlling environment that conforms to the current science regime and its competitive selective procedures without providing alternatives.

In the last couple of years, RASD has become a top-notch place in the international community, according to the list of publications, honours, fellowships and third-party grants. The director has received national and international honours as "highly cited researcher" and publications of its members have gained far-reaching international acknowledgement. Additionally, a revised homepage and manifold media captures keep RASD well represented and provide a "good image" (Ylijoki, 2014). It can be said that it has successfully established a visible and competitive international landmark despite re-structurings in Austrian universities (Fochler, 2016; Felt and Glanz, 2003; Felt et al., 2017) that intensified short-time contracts and project-based funding.

Along with this, RASD scientists construct an identity as stakeholders of a 'brand' that supports a wider recognition and perpetuation of RASD´s reputation and visibility. At the same time, RASD imitates a "firm-like entity" (Etzkowitz, 2003: 111) as it resembles core characteristics of a 'brand': e.g. striving for uniqueness and commitment, a corporate image and reputation, emotional attachment and a corporate culture that employees can relate to (Balmer, 2001). This imitation is based on efforts to create coherence and distinctiveness for an internal and external vision, attempts to establish measurements for quality control (e.g. cleaning; SOPs), in the distribution of work places, or simply by referring to it as an "enterprise". Consequently, the self-identity of the scientists is tightly knit into the department´s demands, leaving little room for how a member could *be* otherwise while being part of a 'brand'. However, an important question becomes: which scientists does this system select? In order to keep up a highly competitive 'brand', group leaders need to attract prospective members (e.g. through reputation, excellent equipment and facilities), while choosing potentially dedicated scientists in line with the group leaders' imaginations of the characteristics of a good researcher. This might lead to a potential streamlining of who is being employed or gets a (stable) position, which also affects epistemic practices.

Accordingly, 'enrolling' to a 'brand' establishes a mutual dependence between scientists and the department, who similarly have to "sell" their identities in order to take part in the prevailing game of representation and performance. Both scientists and RASD are in a process of capitalisation (to paraphrase Slaughter and Rhoades, 2004), as RASD as a 'brand' is taking advantage from highly skilled international scientists, while researchers are capitalizing the department for their own careers. The 'brand' clearly takes part in the credibility cycle (Latour and Woolgar, 1979) as the growing need for reputation and visibility builds a basis for further investments, collaboration, grants and publications. This is tangible when RASD plays an important part in the review process of a journal and potentially has an impact on the subsequent outcome of the publication, or if future job applications of RASDies rely on the reputation of the workplace. The investment strategies in the credibility cycle benefit and thereby provide stability for both individual scientists and the department.

The relationship between scientists and the department also offered possibilities for counteracting the individualized working mode in the life sciences (Müller, 2014b) through e.g. the collective engagement of members in doing chores for the common good. Yet these chores however excluded members of the metaphorical 'family' at RASD. The 'family', imagined as "a place of closeness, safety and nurture" (Davies and Horst, 2016: 385), consisted of group leaders, staff scientists and professors, excluding less experienced scientists. It expanded over time due to a constant increase in permanent jobs (professorships, staff scientist positions). While the research department resembles an important resource for its members, this had an even stronger implication for members of the 'family'. This is the case when the members that perform well are promised a promotion and are likely to be assigned a stable position, or when the scientists need to have the genes of the metaphorical 'family' and adapt to its culture. Thereby, the scientists and the department also establish an alliance that anchors the scientists into a place (Callon, 1986). But again: what does it mean if a 'family' nurtures some and leaves out others, those who might not have its genes? And how does this potentially streamline knowledge production processes in ways that prefer the most outstanding and excellent scientists, thereby distinguishing between excellent scientists and others? While Latour´s (1987) definition of 'enrolling' does not only focus on those who are part of the 'enrolling' process, but also on those who are not part of it, investigating who is not part of the 'family' would provide an interesting step for further research.

To conclude, the scientists at RASD have to conform to the pervasive principles of today´s academe that constructs a regime of selection and competition, leaving little room for alternative ways of living and working in academia. RASD scientists are competent and engaged, they are a valid resource for and stakeholders of its 'brand', and some of them are part of the RASD 'family' - yet lacking possibilities to develop their own individual portfolios. Accordingly, as STS scholars, we must continuously draw close attention towards asking how scientists relate to their workplace, their groups and research departments. And we need to analyse what guides these relationships in times of austerity measures and prevalent insecurities in science. Further: if there is an overtly dominating motive to conform to the demands of reputation and visibility, how can we counteract this and establish careful and caring relationships that provide stability, coherence and distinctiveness, but also the possibility of non-conformity? It is important to understand and critically reflect on how academia is constantly changing and how this change has an effect on the relationship between scientists and their groups, and on academic culture more generally.

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NOTES

1 “Laboratory“ or “lab“ stands for research group. Throughout the text I use both alternately.

2 I have anonymised all references to the department and its scientists.

3 Since the participants were promised confidentiality, pseudonyms are used for the interviewees’ names and location. In order to mask the particularities of their disciplinary background, I will not refer to specificities of the field in the case of work-technicalities.

4 Biologists of certain sub-disciplines such as molecular biology, microbiology or genetics, typically work at a bench designated for laboratory work and at a (separate) computer terminal that in some cases is also shared with other colleagues.

Virginia Eubanks (2018) Automating Inequality: How High-Tech Tools Profile, Police and Punish the Poor. New York: St. Martin's Press. 260 pages. eISBN: 9781466885967

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The allure of technology looms large in modern societies. Day to day we observe changing social behaviors from the way we type, pay for car rides, order food and dream of endless vacations in random people's homes. In *Automating Inequality: How High-Tech Tools Profile, Police and Punish the Poor*, Virginia Eubanks explores the impact of technology on the lives of the poor.

Eubanks central thesis is penetrating. For as long as we can remember human beings have been the protagonists of decision-making. However, since the dawn of the digital age much of that decision-making power has been handed over to sophisticated machines. Data collection provides the raw material for these machines, a reality "so deeply woven into the fabric of social life that, most of the time, we don't even notice we are being watched and analyzed" (p. 5). The marginalized are dependent on public services and endure more screening and surveillance than any other group. The poor are guinea pigs in a social data-driven experiment that has real impacts on their lives… and deaths.

Automating Inequality documents how poverty is being exploited and perpetuated in America through high-tech data. As a professor of political science, Eubanks is able to weave detailed investigative research with compelling personal stories. She guides the reader on a journey to explore the impacts of technology on the poor in Indiana, Los Angeles and Pittsburgh. While the tone of the book is not academic, it examines public policies and welfare programs in detail. The author is such a gifted storyteller that policy problems come to life in what feels at times like a heart wrenching documentary.

Sophie Stipes, to whom the book is dedicated, was the daughter of a poor family in Indiana. Born with cerebral palsy, 1p36 deletion syndrome and back ridden for the first two years of her life, she eventually received a life-saving feeding tube and critical developmental assistance through Indiana's s Family and Social Services Administration (FSSA). Then, at the ripe age of six, Sophie received a letter (addressed to her) stating that Medicaid was being withheld due to a "failure to cooperate in establishing eligibility" (p. 42). The letter was delivered late, which gave little Sophie three days to solve the issue or lose Medicaid. Her family could not pay for her medical needs, which meant she would die.

In the background was a new 10-year $1.16 billion state contract with IBM for the automation of the FSSA, which the state governor promised would improve efficiency and bring modernization. Automation streamlined processes from local offices to one main building and case inquiries to centralized private call centers. The results were disastrous: appointments could not be scheduled, call operators were not trained, eligibility error rates more than tripled, appeal cases were backlogged and 283 000 documents 'disappeared', a 2,473 percent increase. As a result the state "denied more than a million applications for food stamps, Medicaid, and cash benefits, a 54 percent increase… prior to automation" (p. 51).

In less than three years the governor of Indiana admitted to the failure of automation, cancelled the contract, and sued IBM for $437 million. IBM countersued the state for $100 million, won and later (upon appeal) the Supreme Court of Indiana recognized IBM's breach of contract as well. In the words of the judge who saw the case, the failure of automation was due to "misguided government policy and overzealous corporate ambition… both parties are to blame and Indiana's taxpayers are left as apparent losers" with nothing able to remedy the "personal suffering of needy Hoosiers" (p. 72).

Sophie fortunately did not make up those failed statistics – her mother found help from a well connected advocate who showed up at the governor's office, demanded an in person meeting and had her benefits reinstated the next day. Yet Sophie's triumph was the exception, not the rule. Eubanks guides the readers through detailed accounts of the tragedies of automation for homeless services in Los Angeles. In Pittsburgh, a predictive algorithm determines which children are most at risk from abuse and neglect before they are born; a number on the screen selects the parents that must hand their children to foster care.

In the cases of homeless profiling and especially foster care decision-making in Pittsburgh, Eubanks analyses algorithm development in more detail and teases out their social, cultural, political and racial biases. While there is little discussion about algorithm accountability, we are given a detailed description of the different variables that compose the foster care algorithm and how those variables affect outcomes. In one case, the algorithm employed by Alleghany County automatically triggers a welfare investigation for a child flagged with a high-risk score, effectively overriding human decision-making.

These diverse case studies span America and persuasively illustrate the close relationship between automation and increased hardship for the poor. However, we are also presented with a deeper thesis: high-tech-tools don't "profile, police and punish the poor" unaided. Rather, government officials are using technology as a tool to achieve welfare cost reducing policies through the backdoor. Eubanks argues that the same poverty discriminating logic that formed poorhouses in the early 1800's and scientific charity programs before the Great Depression is at work in the "digital poorhouse" of today. The digital poorhouse was born in the 1970's when elected officials "performed a political sleight of hand" by commissioning "expansive new technologies that promised to save money by distributing aid more efficiently, [but rather] these technological systems acted like walls, standing between poor people and their legal rights" (p. 33).

This historical account of poverty, technology and values constitutes both the greatest strength and weakness of the book. On the one hand, Eubanks is able to paint in broad strokes a cogent history of poverty related policies in America. She aptly describes technological developments and their interactions with poverty, welfare and key social historical contexts. One often finds nuanced distinctions and evaluations of different technologies along with their current and recommended alternative applications. There is a consistent positive and critical engagement with technology that assuages fears of luddism.

Unfortunately, the same distinctions and nuance are not observed when it comes to the engagement of values underlying poverty policies. The prescience of *Automating Inequality* is in part due to its willingness to engage the social welfare debate. However, in some instances, the language of *Automating Inequality* betrays unhelpful conflations and exaggerations. For example, welfare-reducing proposals to date are collapsed into an "expansion and continuation of moralistic and punitive poverty management strategies that have been with us since the 1820s" (p. 37). And a Manichean tone emerges in moments of outrage against the "well-funded" movement that "manufactures and circulates misleading stories about the poor" and the "conservative critics of the welfare state [who] continue to run a very effective propaganda campaign" (p. 38).

Automating Inequality lacks a policy framework for dealing with the discussion of values around poverty and welfare. When values are discussed, they are often conflated into a historical narrative which is compelling but not carefully supported. This careless engagement of values may disenfranchise some readers who could be otherwise receptive and would most likely benefit from the important implications of technology driven social programs. By assuming a set of values and wearing them on her sleeve Eubanks misses an opportunity to broaden the discussion about policy alternatives available for addressing the automation of inequality.

However, there is a change of gear towards the last chapter and conclusion of the book. Martin Luther King's Jr. sermons turn the discussion away from value driven politics towards moral and religious considerations towards the poor. King reminds us that one day we will stand before the God of history who will ask about our actions. "Gargantuan bridges" and "gigantic buildings to kiss the skies" built with "scientific and technological power" will be met with "That was not enough! But I was hungry, and ye fed me not. I was naked, and ye clothed me not. I was devoid of a decent sanitary house to live in, and ye provided no shelter for me" (p. 216). Eubanks draws a powerful parallel to our modern day, where we will similarly claim to have built cars that drive themselves and designed bots that speak like humans, yet still we will be met with the same disapproving words in Matthew's Gospel.

In a religious and ethical key the political framing of welfare assistance is problematized and broadened. As Pope Francis recently remarked, "the Lord does not discuss theories of poverty and wealth". Jesus' commitment to the poor is undeniable, but it is also not absolute. In the very next chapter of Matthew quoted by reverend King, Jesus rebukes the disciples for wanting to sell expensive oil and give it to the poor (Mt 26). Based on this broader non-binary framework, the discussion of the implications of automation for society, democracy and the role of government is rich and thought provoking. The nuance and depth offered by a faith filled vision of poverty shows how complex social problems predate technological intervention, sets a high moral bar for evaluating the impact of algorithms on the poor and points towards different possibilities for integrating data-driven tools with human discernment. We are presented with creative and insightful recommendations for moving forward, such as a first draft of the Hippocratic oath for data scientists.

Most importantly, we are warned against the "magical thinking" that often accompanies technological developments. A technocratic mindset that is afraid or unable to grapple with social ills is too easily drawn to the scintillating promise and power of technological quick fixes. This "myopic focus on what's new leads us to miss the important ways that digital tools are embedded in old systems of power and privilege" (p. 178). Technology will not and cannot wipe away the very human problems that make our societies. Eubanks artfully pulls the veil of technology before our eyes and demonstrates how behind every algorithm and sophisticated model is a human input and ethical decision.

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