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Bodies Translating Bodies: Tackling 'Aesthetic Practices' from an ANT Perspective

Alvise Mattozzi

Faculty of Design and Art, Free University of Bozen-Bolzano, Italy / amattozzi@unibz.it

Laura Lucia Parolin

Department of Language and Communication, University of Southern Denmark, Denmark

Abstract

STS and 'aesthetic studies' share an interest in artifacts and the aim to describe and analyse both artifacts and their agency. The present article contributes to such dialogue, first by reconstructing the relation between Actor-Network Theory and 'aesthetic studies' and then by proposing an analytical model enabling the description of 'aesthetic practices', by considering artifacts as bodies. Such model draws on Latour's (2004) reflection about bodies, on Ingold's (2007) one about materials and especially on Fontanille's (2004) semiotics of the body. To illustrate the relevance of the model, the article offers a description-analysis of the development of a prototype of an electronic circuit designed for a data glove.

Keywords: design, instauration, bodies, affect, core-envelopes, aesthetic practices.

Introduction

STS and 'aesthetic studies', taken here to refer to a diverse field of research concerned with art, artworks, and more generally to issues related to the sensory dimension, possess certain commonalities. The most relevant amongst these, are an interest in artifacts and in accounting for their agency and, therefore, in finding ways to describe and analyse both artifacts and their agency.

Despite sharing such common ground, STS and 'aesthetic studies' have only recently embarked on an intense and sustained exchange, mainly due to the crossing of STS with 'aesthetic practices' (Salter et al., 2017).

This article intends to contribute to such exchange, by proposing a descriptive-analytical method based upon Actor-Network Theory [ANT], to account for 'aesthetic practices', especially for the role artifacts play in them, by considering artifacts as bodies.

Drawing on Latour's (2004) reflection about bodies, Ingold's (2007) one about materials and especially on Fontanille's (2004) semiotics of the body, we develop a descriptive-analytical model based on notions that bodies are constituted by relations between a core and one or more envelopes, through which they interact with other bodies. We subsequently use the model to describe-analyse a portion of a design practice, i.e. a specific phase in the development of a prototype of an electronic circuit, designed for a data glove.

The use of 'aesthetic' in 'aesthetic practices'

We refer to 'aesthetic studies' and 'aesthetic practices' by using the word 'aesthetic' in its adjectival sense. For us, 'aesthetic practices' are neither limited to artistic production and/or fruition, nor does the reflection they elicit only pertain to philosophy. Although we do not neglect, or assert a lesser significance to it, we leave 'aesthetics', intended as a purely philosophical reflection on art, beauty and judgments thereof, aside. Despite the fact that we do not assume aesthetics as our primary domain, we regard the adjective 'aesthetic' to be advantageous for the task of describing the kind of practices we are interested in.

In this sense, we employ the expression 'aesthetic practices' to conjure up a wider possibility of practices and activities characterised by both *aisthesis* and *poiesis*, with *aisthesis* intended as feeling or sensory perception and *poiesis* intended not just as making, but more specifically as making something that may produce *aesthesis* during, as well as after, the very process of making, so as "to *make feel*, and *to make oneself feel*, and also, by the sensations of the body [...] *to feel oneself doing*" (Hennion, 2007: 101, italics in the original). Therefore, in using the term 'aesthetic practices', we refer to any practice that while making, producing, or 'instaurating' something (see below), which includes also allowing something to take place, demands attention to the outcome – what is made, produced, instaurated, allowed to take place –, in order for it to unfold its affective multiplicity (Hennion, 2007).

'Artistic practices' – related either to creation or to fruition – can easily play the role of the prototypical model of 'aesthetic practices'. However, we suggest that the latter can apply in domains other than art, with the proviso that within the practice both *poiesis* and *aisthesis* have a role. This explains our reluctance to label the practices of our interests as 'artistic practices', because too great a limit would by definition have been placed upon the set of practices we want to refer to. Equally, if we named them 'sensory', 'sensitive' or 'affective' practices, we would have lost the relation with *poiesis*; if we named them 'making' practices we would have lost, at least partially, the relevance of *aisthesis*. Likewise, if we named them 'creative' practices, we would have opened our reflection to the many issues related to creativity (Farias and Wilkie, 2016a; Parolin and Pellegrinelli, 2020 a; 2020b), that are not analytically relevant for the practices we are concerned with, which do not necessarily need to be creative, in the sense of producing something new – whatever 'new' can mean.

We note that the adjective 'aesthetic' is both etymologically and historically related to the sensitive dimension in general. As is well known, aesthetics as a mode of philosophical reflection was originally intended in the 18th century as "a science of sensitive knowing" (Davey, 2009: 162), as "a discourse of the body" (Mascia-Lees, 2011: 3, citing Terry Eagleton; see also Highmore, 2010). Alongside, the fact that the term 'aesthetic' is today used in reference to art and artworks, and hence also to their *poiesis*, reaffirms our confidence in the path we have taken. Put simply, we exploit both the etymological meaning of aesthetics and its relation to sensitivity in general, as well as its further development, related to art and the production of artworks.

As we will show throughout the article, there could be less ambivalent ways of indicating the practices we want to refer to. The most adequate one would probably be 'instaurative practices'. For the STS reader, such a term, inspired by Étienne Souriau's (1956) aesthetics, recently rediscovered by ANT (Hennion, 2013, 2016; Hennion and Monnin, 2015; Stengers and Latour, 2009; see below), would most likely been regarded as somewhat obscure. In lieu of a better term, or for Souriau's aesthetics to become more commonly employed, we are content that 'aesthetic practices' provides a good enough description of what we refer to.

Of course, we are not the first to use the expression 'aesthetic practices'[1]. For instance, within STS literature 'aesthetic practices' has been used by Jennifer Gabrys and Kathryn Yusoff (2012: 17), who recover the definition Jacques Rancière ([2000] 2013: 8) provides of 'artistic practices'[2] as 'ways of doing and making' that intervene in the general distribution of ways of doing and making as well as in the relationships they maintain to modes of being and forms of visibility".

Within a more classical canon of aesthetic studies, Hans Robert Jauss ([1977] 1982) too, talks about aesthetic practice (in the singular) and in a similar way to us, since he links it to *poiesis* and *aisthesis* (and *catharsis*). However, he tends to attribute *poiesis* to the production of artworks and *aisthesis* to their reception, despite his acknowledgement that reception also entails, as for us, forms of *poiesis*. Indeed, for us, both *aisthesis* and *poiesis* are constitutive of production and reception (see also, Hennion, 2007).

The mentioned uses of 'aesthetic practices' refer them more or less directly to "artistic practices", a choice that, as we said, we find limiting – a constraint, not just to our framework, but also to theirs, given their consideration of *aisthesis* or aesthetics is, like us, related in a very broad way to "sense experience" (see, for instance, Rancière [2000] 2013: 8). In 'aesthetic practices' then, we implicitly consider a very broad definition of aesthetics similar to the one proposed, within an ANT perspective similar to ours, by Mike Michael, Liliana Ovalle and Alex Wilkie (2018: 243), for whom "aesthetics does not just pertain to questions of beauty nor to the reception of works of art, but rather to sensible experience and form in general"[3].

Like Gabrys and Yusoff (2012) and Michael, Ovalle and Wilkie (2018), we, following Hennion (2007), claim that aesthetic practices, by giving attention to artifacts and unfolding their multiplicity, open up possibilities. However, because we are more interested in everyday practices, than in artistic practices, we are skeptical that this opening has any direct political relevance – especially at the scale of "forms of life to come" as, following Rancière, Gabrys and Yusoff (2012: 17), suggest.

Therefore, in agreement with the "new sociology of art" (de la Fuente, 2007, 2010; Fox, 2015), which "locates aesthetic experience in the flow of everyday life rather than in the sacred space of art institutions" (Kobyshcha, 2018: 481), we gravitate toward those reflections that connect aesthetics more directly to everyday practices, such as those found in John Dewey (1934) or Richard Shusterman (1999). The latter recovered and rearticulated Dewey's reflection on aesthetic experience to develop the new discipline of 'somaesthetics' as "the critical, ameliorative study of one's experience and use of one's body as a locus of sensory-aesthetic appreciation (*aisthesis*) and creative self-fashioning" (Shusterman, 1999: 302). As we can see, also Shusterman tends to think in terms of *aisthesis* and *poiesis* (creation), even if addressed to the self[4]. Similarly does Dewey (1934: 46-47), who distinguishes between 'artistic' that "refers primarily to the act of production" and 'aesthetic', which refers to the act of "perception and enjoyment". Dewey (1934)[5] is interested, as we are, in the intersection between these two aspects, which, besides 'artistic' and 'aesthetic', he calls 'doing' and 'undergoing' or 'perception'. For him, it is this intersection, which characterises the 'aesthetic experience'.

One last point. We do not use *poiesis* in the platonic sense, as creation *ex-nihilo*, or "something where before there was nothing" (Sennet, 2009: 70). Rather, with it, we intend something similar to the lesser known, already mentioned, term of "instauration" (Hennion, 2013, 2016; Souriau, 1956; Stengers and Latour, 2009), that is to say, creation through the transformation of something that is already there, a re-creation, a palim*poiesis*, which cannot but acknowledge the *aisthesis* of what is already there.

Exchanges between STS, 'aesthetic studies' and 'aesthetic practices'

The exchanges between STS and what we have called 'aesthetic practices' and 'aesthetic studies' are not new. However, they have become more intense and systematic. By way of illustration, the most recent (fourth) edition of the *Handbook of Science and Technology Studies* (Felt et al., 2017) has a specific chapter, hitherto absent in previous editions[6], dedicated to "Art, Design and Performance" (Salter et al., 2017). The authors of the chapter list four ways in which STS can engage with three paradigmatic 'aesthetic practices' – namely "art, design and performance" (Salter et al., 2017: 140), as "collaborators" (Michael,2018a: 116; see also, Storni, 2015):

1. an involvement of art and design in exploring science and technologies practices in order to generate enriched forms of knowledge (see, for instance, Boucher et al., 2018; Calvert and Schyfter, 2017; Lury and Wakeford, 2012), which includes aesthetics across all the senses (Benschop, 2009; Salter, 2015);
2. an engagement with "enlarged methodological repertoires" (Salter et al., 2017: 140) provided by art and design, in order to rearticulate how to display and communicate sciences (see, e.g., Lury and Wakeford, 2012);
3. an engagement related to the use of such repertoires in order to enact and communicate STS' research results so to facilitate "the inclusion of wider publics in the reflection on science and technology and contributing to its democratisation" (Salter et al., 2017: 140; see, e.g., Barry and Kimbell, 2005; Venturini et al., 2015; Yaneva, 2013);
4. an engagement with "alternative ways for STS to get involved in sites where science and technology are constructed", given that art and design can operate as "forms of radical political engagement with sociomaterial worlds", which can take part "in the shaping of technoworlds and the formation of technosocieties" (Salter et al., 2017: 140; see, for instance, Domínguez Rubio and Fogué, 2015; Gabrys and Yusoff, 2012; Myers, 2017)[7].

The general focus of Salter et al. (2017: 140) is on how STS could broaden their "ways of investigating and intervening into technoscientific worlds" by engaging with art and design, learning different methods, acquiring different forms of knowledge and by reflecting on it. Although we are interested in all four points listed by Salter, Burri and Dumit (2017) and we are actively engaged in at least three of them (see, e.g., Krois et al., 2017; Moretti and Mattozzi, 2020; Parolin and Pellegrinelli, 2020a), this article is concerned with 'aesthetic practices' as a "subject of enquiry" (Storni, 2015), as a "topic […][:] one object amongst others that can be subjected to [STS] analysis" (Michael, 2018a: 116), like many other STS scholars have done (see, e.g., Dubuisson and Hennion, 1996; Storni, 2012; Strandvad, 2012; Yaneva, 2003, 2009). This has also been the focus of Ruth Benschop's (2009) introduction to four papers about practices related to the art world and to music. More recently, such focus has been developed and expanded also in Farìas and Wilkie (2016b) and in Sormani et al. (2019).

What Benschop (2009) underlined, is not only how addressing art can provide STS with insights about the role of the senses, about the role of materiality and about the boundaries between science and other social realms. But also, she highlighted the specific perspective STS would bring to the study of 'aesthetic practices': STS, having focused on the everyday practices of scientific work, are able to provide the tools to describe the "ordinariness, heterogeneous ensembles and trivial work" (Benschop, 2009: 4) of 'aesthetic practices'.

The empirical investigation of everyday practices through empirical cases, considered the "bread and butter of STS" (Sismondo, 2010: viii, cited in Carbone et al., 2019: 2), is also what is proposed in Farías and Wilkie's (2016b) and Sormani et al. (2019). Farías and Wilkie (2016b) point to the different, but nevertheless analogous, sites of technoscientific and aesthetic production in the form of the laboratory and the studio, thus transposing the STS data gathering practice, namely 'laboratory ethnography' (Knorr Cetina, 1983; Latour and Woolgar, 1979) into the artists' or designers' studio. This, they argue, "turn[s] our gaze to the actual sites in which practitioners engage in conceiving, modelling, testing and finishing cultural artifacts" (Farías and Wilkie, 2016a: 7).[8] Similarly, Carbone et al. (2019) identify 'experiment' as a specific practice shared by scientists and artists and one which characterises contemporary encounters between sciences and arts (on the issue, see also Salter, 2015).

Agreeing with Benschop (2009), we also think that addressing 'aesthetic practices' can provide STS with revealing insights about the role of the senses and the role of materiality. This article, indeed, tackles these very issues through a second aspect, only touched upon by Benschop (2009) and, except for a section of Sormani et al. (2019), seldom addressed in the extant literature to which we have referred: how STS relate to existing research on "the arts" carried out by "aesthetics, art history; psychology and sociology of art, phenomenology of art, etc." (Benschop, 2009: 4), i.e. what we have altogether termed 'aesthetic studies'.

Exchanges between ANT and 'aesthetic studies'

Since we started reflecting on the exchanges between 'aesthetic practices and studies'[9] and STS, we have been confident that "a vast common ground [is] open[…]" (Latour, 1998: 422)[10] between STS and 'aesthetic studies' and that such common ground stretches around the relevance both these fields ascribe to artifacts.

However, whilst STS can provide 'aesthetic studies' with the capacity to investigate 'aesthetic practices' and 'aesthetic artifacts' *in the making,* extending, as we have seen, 'laboratory ethnographies' to other sites, 'aesthetic studies' can provide STS with a sensitivity to artifacts, that, despite the great attention payed to artifacts by STS, STS still lack.

Indeed, as Latour (1998: 422) has noted, there has been "very little in" science studies "at the level of detail and heterogeneity […] of the best social history of art", as for the description of artifacts[11].

ANT and the social history of art: mediation

Latour's (1998: 422) critique draws on the capacity of social history of art to "deploy […] mediations without threatening the work itself – *l'œuvre*". With reference to the work of social historian of art Svetlana Alpers, among these mediations, he mentions specific features of artworks like the "quality of the varnish" or the "narrative of the theme" (Latour, 1998: 422). Of course, Alpers, being a social historian of art considers also other kinds of mediations, external to the artwork, as those carried out by buyers and sellers, market forces, critical accounts, competition among painters, taste, knowledge, with which social scientists are usually more familiar. Nevertheless, she also considers features of the work, to which Hennion ([1993] 2015: 145 and 159) adds "the grain and the thickness of the paste", and "pigments and formats." Moreover, with reference to the historian, theoretician and semiologist of art Louis Marin, Hennion ([1993] 2015: 150) also adds all the aspects of the artwork that furnish it with its opacity, yet within which the transparency of a certain message is built, such as "the manner of […] style, format, grain and frame", as well as the architectural structure in which frescoes, for instance, are painted (see also, Hennion and Monnin, 2015).

These observations emerged from an intense reflection on the methods of (social) history of art, which took place in between the 1980s and the 1990s, carried out by some of the founders of ANT– specifically, Madeleine Akrich (1986, 1989), Antoine Hennion (1993, [1993] 2015; Hennion and Latour, 1993, [1996] 2003) and Bruno Latour (1998) – and which contributed to the constitution of ANT.

Indeed, Akrich, Hennion and Latour took the method of (social) history of art as a model for their approach in progress. For Hennion (1993: 16), the history of art provides a lesson in symmetry, going beyond the dualism of object/society, according to which objects are either abstracted from the social and studied in terms of "pure aesthetics", or they are considered as screens on which social beliefs are projected. Social history of art was able to escape such a dualism, because, according to Hennion (1993, [1993] 2015), it was able to introduce a model of mediation different to the one of social mediation elaborated by Durkheim, which was based on the notion of belief.

In this way, social history of art was able to account for the reciprocal construction of humans by things, and of things by humans (Hennion, 1993: 28). Moreover, by being able to multiply causes considered to have heterogeneous origins (Hennion, [1993] 2015: 29). As Latour (1998: 422) neatly summarised, "The social history of the visual arts could teach historians of scientific activity quite a lot in the matter of mediations".

ANT and Souriau's aesthetics: 'instauration'

The rediscovery of Etienne Souriau's (1956) aesthetic reflection on 'instauration', prompted an updated of Hennion's (2013, 2016; Hennion and Monnin, 2015) and Latour's (Stengers and Latour, 2009) take on mediation and 'aesthetic practices'. On the one hand "Souriau's perspectives echo" ANT scholars' discourses, yet on the other, they "provide a different relevance" (Hennion and Monnin 2015: 9, our translation) to many of the issues tackled by ANT.

The "different relevance" (Hennion and Monnin, 2015: 9) of Souriau's contribution raises the possibility to better frame 'aesthetic practices' *in vivo*, not only by paying more attention to artifacts, but also by heightening awareness of the full body contact between humans and non-humans.

'Instauration' is the process, by which an object is given a, relatively, autonomous existence.

The œuvre, for instance, "once […] created, it [can] escape[…] from its author, it [can] resist[…], it [can or cannot] have effects" (Hennion, 2016: 302).

Therefore, the existence of beings is always relative and gradual.

The relative autonomy and the gradualness of existence are issues that Latour had already addressed prior to Souriau's rediscovery, through notions like "shifting down" (Latour, 1992) and *factishes* (Stengers and Latour, 2009: 15), which account for autonomy, and through the AND/OR relations model (Figure 2), which allows describing the gradual existence of technoscientific entities (Latour, 1999).

What Souriau adds, which is particularly interesting for us, is the way he describes the ongoing instaurative process: he takes into account the little gestures – "each strike of the chisel on the stone" (Souriau, 1956: 12, our translation) – that allows "the gradual passage from one mode of existence to the other" (Souriau, 1956: 12, our translation). It is thanks to the possibility to focus on these detailed aspects of the process, that the 'anaphoric progression'[12], leading to 'instauration' as the terminative step of this process, can be accounted for, together with the "progressive metamorphosis of the one into the other" (Souriau, 1956: 12, our translation)[13].

As Stengers and Latour (2009) asserted, this also means that, through Souriau, *poiesis* is not seen as the outcome of a mind at the origin of all the actions to which matter complies, but as a distributed process where the 'work to be done' (*œuvre-à-faire*) also raises issues, with which the creator has to negotiate – and which need to be taken into account by the scholar interested in describing-analysing these processes. Therefore, in "Souriau's work it is the statue that gives you the hand, that obliges the gesture of the sculptor, as well as the reverse" (Hennion, 2013: § 14, our translation).

Again, this is not totally new for ANT. Hennion and Geneviève Teil (2004: 535, our translation) noted, indeed, that wine amateurs, when enjoying a wine, "move forward through a series of mediations" – the 'anaphoric progression' – in order to let or enable what they enjoy to "open", to "become[…] plural". However, more recently, Hennion and Monnin (2015) by comparing amateurs to Souriau's sculptor had to consider that the object of amateurs' passion, is not only what can open up and become plural, but also what produces an alteration of the amateurs, thus taking more into account the agency of the artifact.

Describing bodies translating bodies

Hennion (2016: 295) underlines that ANT, as well as Souriau, invites to "insist […] on the associations, […], the translations, […] the passages", when addressing artifacts, be they techno-scientific facts or artworks. We deem such invitation key in order to describe-analyse 'aesthetic practices'.

However, we also deem that, in order to actually carry successfully out such invitation, we need to integrate STS's and specifically ANT's methodology.

As we have seen, through the exchanges with 'aesthetic studies', ANT acquired insights in order to tackle practices of any kind, by accounting for the role artifacts have in them and especially for how they contribute to the emergence of mediations. On the other hand, neither the social history of art, nor Souriau, ever tackled aesthetic practices in their actual making. As we have seen, this is the specific contribution that STS, not just ANT, provide aesthetic studies with – the transposition of 'laboratory ethnography' to other sites of instauration (Beschop 2009; Farìas and Wilkie, 2016b; Carbone et al., 2019).

We build on this ground by devising – in this section – and putting on trial – in the following one – some tools, i.e notions, categories and models that should allow us to actually account for the passages Hennion was referring to.

In order to do that, these tools need not only to allow us to describe-analyse 'aesthetic practices' as translations, where artifacts and their "networks within" (Parolin and Mattozzi, 2020) play a role, but they need also to allow us to account for artifacts as bodies.

Describing-analyzing 'aesthetic practices' as situated and distributed translations among bodies

Within STS, ANT has tended to tackle scientific practices by describing how beings come into existence through processes of translations, the latter intended as "[a]ll displacements through other actors whose mediation is indispensable for any action to occur" (Latour, 1999: 311). In this way ANT, has accounted for scientific practices, and the artifacts that are both involved in and result from such practices, as a "series of transformations" (Latour, 1998: 421), where something nevertheless remains constant: the 'immutable mobile' (Latour, 1998).

This is illustrated elegantly by Latour's ethnography of scientists trying to understand the recip-

Figure 1. Few steps of the translation from savannah-forest to scientific diagram (Latour, [1993] 1999: 30-36)

rocal development of savannah and forest in a Brazilian region (Latour, [1993] 1999). In our view, in its brevity, such ethnography epitomises ANT's take of scientific practices, given that through the description they emerge as a series of translations[14]. Latour ([1993] 1999), indeed, accounts for the multiple and heterogeneous mediations – carried out through various instruments (Figure 1b and 1c) – which took place between the field and the laboratory. These mediations allowed passing from a blend of vegetation (Figure 1a) to a diagram (Figure 1d), translating the first into the second and, through such translation, generating knowledge.

A similar ethnographic attempt, albeit explicitly addressing 'aesthetic practices' in the form of 'artistic practices'[15], was carried out by Albena Yaneva (2003: 170-171). She described "a drawing in the process of becoming art", focusing on "the small installation operations and the variety of actors involved: the painter, the artist, chalk, self-adhesive paper, fixing liquid, carpets, curators, and the museum floor", by following "the dynamic of some peculiar assemblies of actors, their movements, dispersions, microscopic changes, and new alliances", by drawing attention to "those tiny, infrasmall differences among objects" (Yaneva, 2003: 170-171).

The present article follows these approaches to practices by adding a more fulsome appreciation of Souriau's insights and, consequently, a more nuanced account of the full body contact that takes place among human and non-human actors within processes of 'instauration'. We argue that this is a necessary step if the task of accounting for 'aesthetic practices', as we defined them, is to be successfully undertaken. In so doing, it is incumbent upon us to tackle issues related to the relevance of bodies in practices, as well as the significance of senses and affect – all issues that have recently been the focus of many STS scholars (amongst others, Burri et al., 2011; Guggenheim, 2011; Puig de la Bellacasa, 2009). More specifically, this article addresses issues related to haptics, intended as a sense "comprising the tactile, kinæsthetic and proprioceptive senses" (Fisher, 1997; see also Myers and Dumit, 2011; O'Connor, 2016), thus engaging with processes similar to those that Joseph Dumit and Natasha Myers (2011; Myers 2006; 2015), have explored for scientific practices, but with two relevant differences.

First, by acknowledging ANT's principle of symmetry, we consider artifacts as bodies, fully unfolding Latour's (2004: 205-206) pragmatist derived proposal that "to have a body is to learn to be affected, meaning 'effectuated', moved, put into motion by other entities, humans or non-humans". Consequently, we assume bodies as "interface[s] that becomes more and more describable as [they] learn[...] to be affected by more and more elements" (Latour, 2004: 205-206).

Secondly, we acknowledge the scholarship that has shown the relevance of bodies, senses and affect in scientific and technological practices, but our purpose through the exploration of empirical cases (Parolin and Mattozzi, 2013; Mattozzi, 2017), is primarily methodological. Methodology, however, is more than a reflection on data gathering methods, in which we concur with the extension of 'laboratory ethnography' to sites and practices beyond sciences. Methodology is also a reflection on the following question, which has been a recurring query for ANT: "how to describe-analyse?"[16] (Akrich, [1987] 1992[17]; Akrich and Latour, 1992 Latour, 1990; 1992; 2005).

In order to answer this question in relation to 'aesthetic practices', we propose tools that should allow to describe-analyse 1) bodies in interaction, transforming each other, 2) affects that result from these interactions and 3) the sensations related to the involved senses.

The tools we propose should then allow also to answer the questions Chris Salter (2015: xi) raised: "how does one write an account" of practices, which include various materials and "sensory inputs such as touch […]? How do you record the unrecordable experience of sense and affect?"

We then suggest a model that allows us to describe-analyse human, as well as non-human, living, as well as non-living instances, as bodies becoming sensitive to one another. Our model is an integration of Latour's infralanguage[18] (Akrich and Latour, 1992; Latour, 2005) – a way to provide actants within a body.

Showing how "bodies are impacted upon by particular circumstances" (Michael, 2011: 55), specifically by other bodies, enables us to account for affect, intended as "change[s], or variation[s], that occur[…] when bodies collide, or come into contact" (Colman, 2005: 11)[19]. This provides an ANT pathway to trace the "affective capacities of objects" (McCormack, 2019: 218)[20].

Beyond 'doing' and 'making do': the role of artifacts as bodies

As we have seen, 'aesthetic studies' have provided ANT with insights, which enable taking artifacts into consideration as mediators. 'Aesthetic studies' have also shown that, in order to take artifacts into consideration as mediators, features of artifacts needs to be taken into account. As Latour also suggests:

> We [social scientists] should not state that 'when faced with an object, ignore its content and look for the social aspects surrounding it'. Rather, one should say that 'when faced with an object, attend first to the associations out of which it's made and only later look at how it has renewed the repertoire of social ties'. (Latour, 2005: 233)

In this sense, we need to account for what, elsewhere, we have called the 'network within' (Parolin and Mattozzi, 2014; 2020: 38 and 48), that is to say, the network constituted by relations between shapes, textures, colours and consistencies and the role they play in outlining cores and envelopes of artifacts (see below). 'Aesthetic studies' can be very useful in describing such relations because they, by attributing relevance to artworks, have elaborated notions, categories and models to describe how these relations take place (Lancioni, [2001] 2012)[21].

Having said this, we are mindful of Hennion's (2005: 140) concern that we should not analyse "properties" of artifacts "straightforward[ly]". And indeed, as we will better see below, we will address features of artifacts as relational, as the outcome of relations, as proposed, among others, by Dewey:

> The conjoined properties that mark off and identify a chair, a piece of granite, a meteor, are not sets of qualities given existentially as such and such. They are certain qualities which constitute in their ordered conjunction with one another valid signs of what will ensue when certain operations are performed. An object, in other words, is a set of qualities treated as potentialities for specified existential consequences. Powder is what will explode under certain conditions. (Dewey, 1938: 129)

More recently, Tim Ingold has reflected on artifacts and materials in a similar way:

> … the properties of materials, regarded as constituents of an environment, cannot be identified as fixed, essential attributes of things, but are rather processual and relational. They are neither objectively determined nor subjectively imagined but practically experienced. In that sense, every property is a condensed story. To describe the properties of materials is to tell the stories of what happens to them as they flow, mix and mutate. (Ingold, 2007: 14)

How then, can such stories be told? How can these relations, their coming together into features and, *qua* Dewey, their translations into different relations or consequences, be more fulsomely describe-analysed?

ANT, and in particular Akrich and Latour (1992) and Latour (1992), developed tools – like the notion of *script* and all related terms part of Latour's infralanguage – in order to describe-analyse what artifacts "do" and "make do" – and so telling some of the stories Ingold was talking about.

However, in order to generate a deeper understanding of 'aesthetic practices' – and telling also other kinds of stories Ingold was referring to – the processes artifacts and other entities undergo, also need to be accounted for.

Latour's (1990; see also, Akrich and Latour, 1992) example of the weight attached to a hotel room key (Figure 2) provides a good illustration of the way we intend to integrate Latour's infralanguage, in order to account for the way entities undergo the action of other entities. Through the utilisation of categories like AND/OR relations and Program/Antiprogram of Action (PoA/APoA), Latour (1990; Figure 2) describes-analyses the relative merits of 1) a verbal request by the concierge, 2) a reminder written tag attached to the key, 3) a weight attached to the key, in persuading hotel clients to return keys to the hotel desk when they go out and, especially, when they check-out. The weight, Latour (1990) says, is more effective, because by operating a mediation (Latour, 1999: 186-187), rearticulates one of the PoAs of hotel clients: from having to remember to leave the key at the desk, to wanting to leave them. Indeed, when the weight is attached to the keys, clients want to get rid of it, since it is cumbersome.

The weight is able to carry out this mediation by continuously affecting the body of the clients: by increasing the weight, they need to carry and, when keys are retained in a pocket, by pressing on the moving leg. Both actions of the weight directly contrast the clients PoA related to moving. The weight's mediation is better understood once the ways in which actors affect each other are taken into account. Both the words of the concierge, and the reminder tag, have a limited perceptible effect on the body of the guests, who are not required to pay attention to them, whereas the weight, being bulky, heavy and pressing over parts of the guest's body, continuously affects them when with them.

A model to describe-analyse bodies translating bodies

Several scholars have reflected on the relations between artifacts and between artifacts and users as contacts among bodies. George Herbert Mead (1932), noted that relations among physical things are nothing but pressures onto respective boundaries and regarded bodies as bounded interiorities, interacting through pressures with other body-things. When conceptualising materials, Ingold (2007) also saw properties as the outcome of relations and, drawing on James J. Gibson, proposed descriptive categories to help describe the kind of interactions among the bounded interiorities Mead was referring to.

For Ingold (2007: 5), materials can be described by taking into account the:

- *medium* – basically air, for humans – which allows the transmission of energy and vibrations;
- *substances* which penetrate and diffuse within the medium - these are more resistant to penetration than the medium;
- *surfaces*, which provide substances with a "relatively persistent layout, a degree of resistance to deformation and disintegration, a distinctive shape".

In addition to these, Jacques Fontanille's (2004) semiotics of the body addresses bodies as constituted by envelopes and internal cores (Figure 3). Such semiotics of the body, developed to account for aesthetic experiences, initially in relation to literature, assumes a topology of the body similar to Mead's and Ingold's one. Fontanille exploits such topology in order to account for how bodies interact with other bodies through pressures, penetrations, expulsions, envelopments and dis-envelopments (Figure 3; see also, Marrone, [2005] 2009). Thus, he is able to account, not only for interactions amongst bodies, but also, for the unfolding of the senses within these interactions as well as the unfolding of passions and emotions[22].

Because Fontanille's proposal is the most articulated and the most tested through empirical analysis, much of which related to artifacts[23] (among others, Festi, 2008; Fontanille, 2001), we suggest, that once lifted from the methodological shackles of its phenomenological legacy[24], it offers adequate categorisation for describing contacts amongst human and non-human bodies, and their outcomes (see also, Mattozzi, 2017)[25].

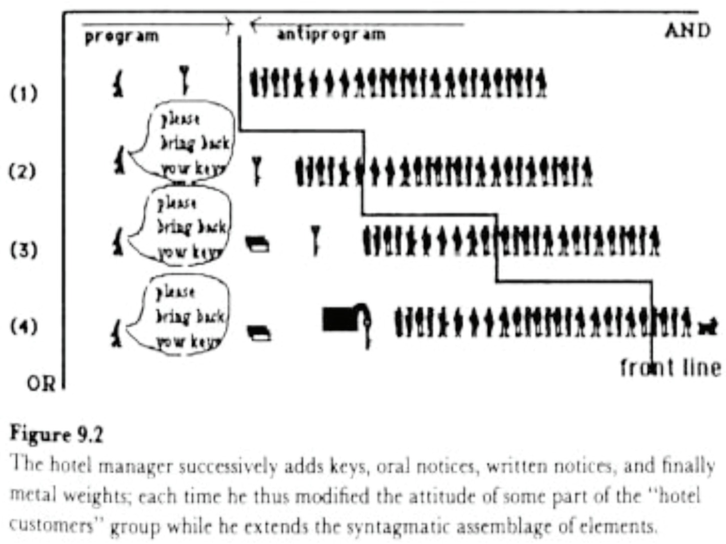


Figure 2. The efficacy of the weight attached to hotel room keys as described-analysed in Akrich and Latour (1992: 263; see also, Latour 1990: 107)

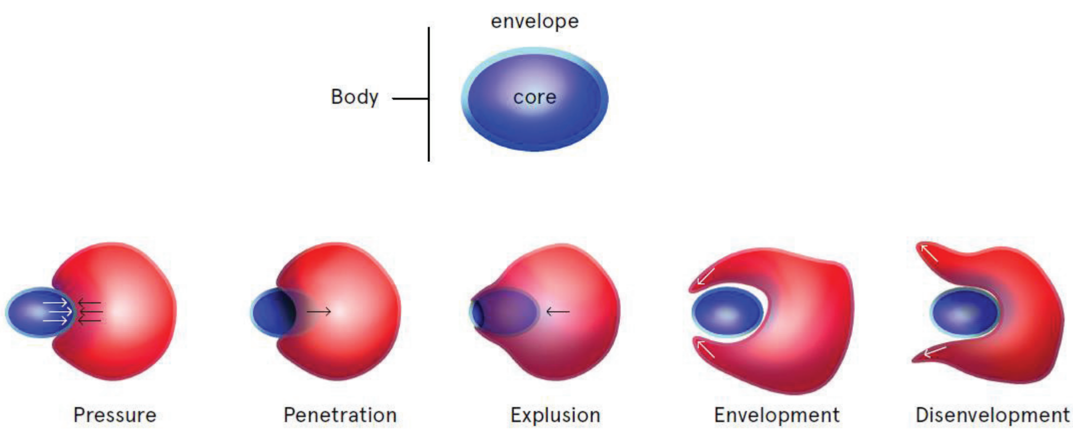


Figure 3. Our elaboration of Fontanille's (2004) model (Our drawing)

With these categories, we can account for 'aesthetic practices' and their mediations, not simply by keeping together *poiesis* and *aesthesis*, but more significantly, by the inevitable inclusion of *aisthesis* into *poiesis*. Indeed, if *poiesis* has to do with bodies and their transformations, their translations, or *qua* Souriau, their metamorphoses, then, it will also, inevitably, have to do with contacts among bodies and their consequential *aisthesis*.

The envelope-core model of the body allows us then to address 'aesthetic practices' as "bodies made translatable" (Parolin and Mattozzi, 2013: 304)[26].

Nevertheless, it does not exhaust all the aspects of 'aesthetic practices'. We only claim it provides an indispensable ground for starting a description-analysis of 'aesthetic practices', intended as translations among bodies. Other categories and other aspects need also to be taken into account for a full account of these practices.

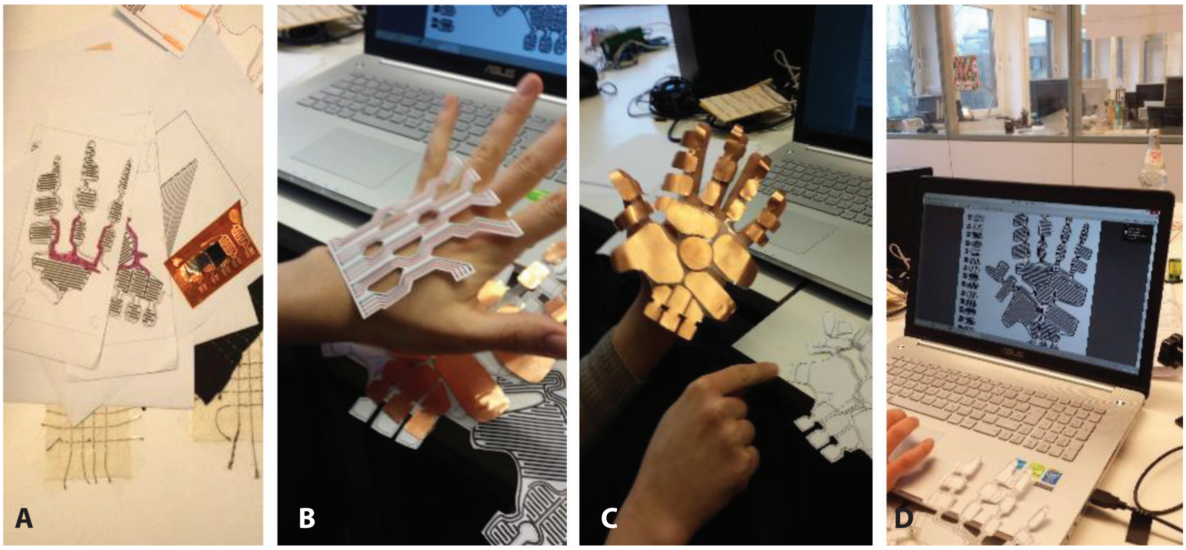
The case

To illustrate the potential relevance of our descriptive-analytical model for empirical research, we use a case study from our own observations of design practices. The observations were carried out at the Design Research Lab (www.drlab.org) of the Universität der Künste, Berlin, by one of the authors over two months of fieldwork in winter 2014, comprising observations and interviews. The specific empirical material used here reflects three full days of observation.

The observation focused on an interaction design project aimed at upgrading a special data-glove that functioned as a mobile communication and translation device for deafblind people. The "Mobile Lorm Glove" project (Figure 4; Bieling et al., 2014, 2016, 2017; Gollner et al., 2012) was intended as a motion sensitive glove with the ability to transform gestural code into alphabetic symbols that can be sent to other devices and, at the same time, receive alphabetic text messages and translate them into the tactual code of the Lorm alphabet.

At the time of the observation, a senior and two junior designers[27] were working on the project. A prototype (see Figure 4) already existed and their task was to redesign the circuit of the sensors to make the glove more sensitive. What follows is series of vignettes that capture complex interac-

Figure 4. The prototype of the Mobile Lorm Glove (Photo, courtesy of Tom Bieling – Design Research Lab)



Figures 5 a-d. Sketches, drawings, printed models, sheets of coppers and other artifacts used in the design process before the etching (Photos: Alvise Mattozzi)

tions between and within bodies, in the attempt to develop a working circuit that was flexible enough to withstand use in the glove, yet sensitive enough to translate movements and pressures into digital data.

Vignette 1: Pressing as part of the etching process

Despite appearances, the person in the picture below (Figure 6) is not ironing her clothes, she is hot-pressing one surface against another. This is part of a wider process related to the etching of a printed circuit board (PCB).

As we can read from a tutorial used by the designer in the picture, "[e]tching is a technique used to quickly make professional looking PCB's with limited resources"[28].

More generally, to etch means "to produce (as a pattern or design) on a hard material by eating into the material's surface (as by acid or laser beam)" (Merriam-Webster onLine). "[E]tching" is, then, a "subtractive method" used "for the production of printed circuit boards: acid is used to remove unwanted copper from a prefabricated laminate. This is done by applying a temporary mask that protects parts of the laminate from the acid and leaves the desired copper layer untouched"[29] (Figure 7).

What is observed in the photograph was not etching, and the action of pressing with the iron was not subtracting anything. Instead, the designer was adding something in what is the preliminary step to etching, namely the application of the "temporary mask" in order to protect parts of the laminate from the acid. Thus, she is not concerned with what has to be removed but, by trying to make the ink outlining a pattern of the circuit printed on glossy paper transpose on

Figure 6. Designer preparing a copper laminated film for etching (Photo: Alvise Mattozzi)

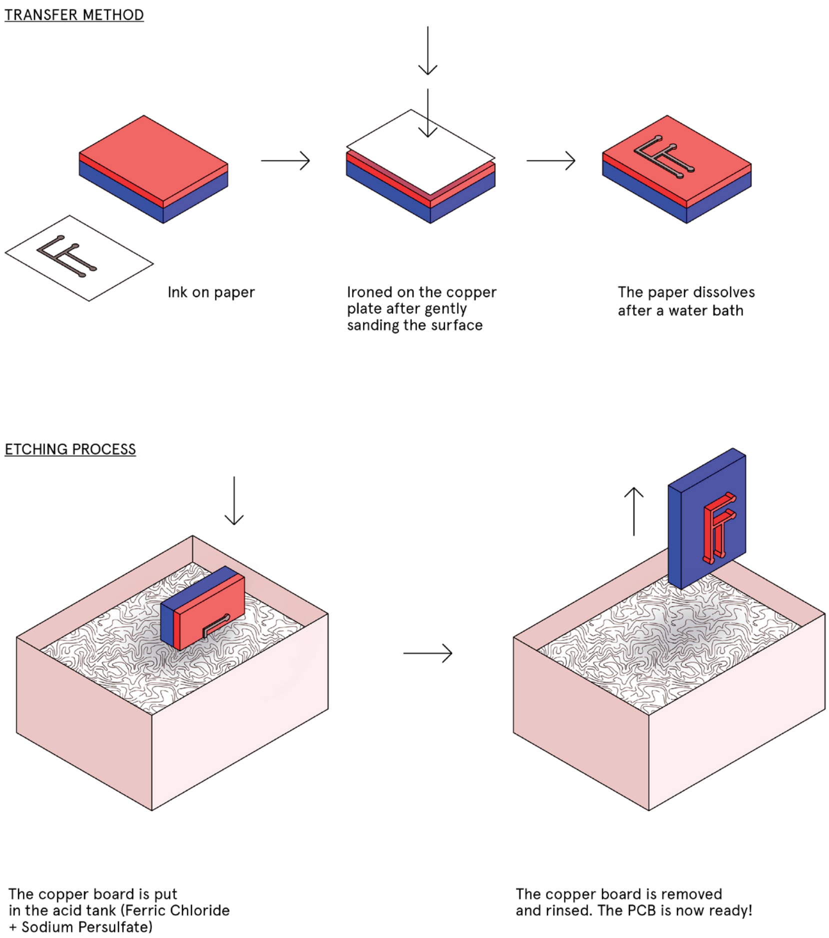


Figure 7. The subtractive method of etching a printed circuit board (our drawings)

to a copper laminated surface, she is concerned with what has to stay. Indeed, the laser printed ink is not affected by the etching acid and protects what is intended to be preserved. What the picture shows was the final step in various translations that took place prior to the transfer of the ink onto the copper (Figure 6). In short, a given configuration of a circuit has to pass from being drawn with a pencil on paper, to being drawn on a computer screen, to being printed on glossy paper, to being transferred to a copper laminated surface, in order to then be separated from the rest of the copper laminated surface (Figures 5, 7 and 8)[30].

Figure 8. The laser printer ink transferred to protect the copper surface that will result in the circuit. Notice how one of the copper laminas has been sanded and another not (Photo: Alvise Mattozzi)

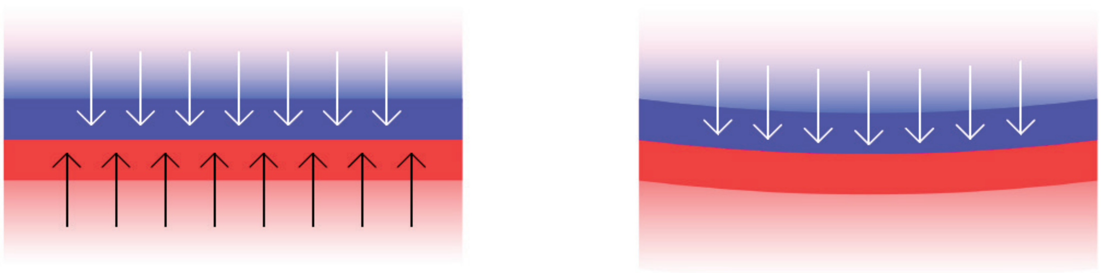


Figure 9. Dynamics of pressure among bodies: a. the pressing body affects only the envelope-surface; b. the pressing body affects also the core-substance of the pressed over body (Our drawings)

In each phase, a new mediation took place, each one entailing a contact between envelopes-surfaces like the contact between the printed surface of the paper and the one of the copper lamina, on which the paper was pressed (Figure 9a). Sometimes there were also contacts that involved the core-substance of both, or only one of the involved bodies, such as the one between the iron and the pressing board (Figure 9b). In this way, one body affects another. This is evident when the printed sheet affected the copper lamina with a trace of ink, which in turn created a further envelope-surface, shielding the lamina from the acid (Figure 7).

Vignette 2: Etching a film

In Figure 6, the designer was making her third attempt to affect the copper laminated surface, by seeking to engender the passage of the ink from the glossy paper to the laminated copper surface, and ensure its retention. Switching attention to Figure 10, we can clearly see that the copper lies on a film, precisely because the circuit needs to be mounted on a flexible substrate.

This particular detail adds a level of complexity and unpredictability to the procedure, since etching a copper laminated surface would commonly be applied to lamina laying on a hard-rigid substrate. Indeed, the tutorial she is using shows the etching of "printed circuit *boards*"[31], i.e. hard and rigid elements.

The changes to the material consistency of the substrate of the copper laminated surface, from rigid to flexible, were enough to make the tutorial much harder to follow than instructions, in general, usually are. The transference of ink from glossy paper to copper film did not work as described, consequently the designers needed to develop a new process to create a repeatable and reliable procedure.

Figure 10. Circuits etched on copper laminated films (Photo: Alvise Mattozzi)

Figure 11. Taking notes about successful trials (Photo: Alvise Mattozzi)

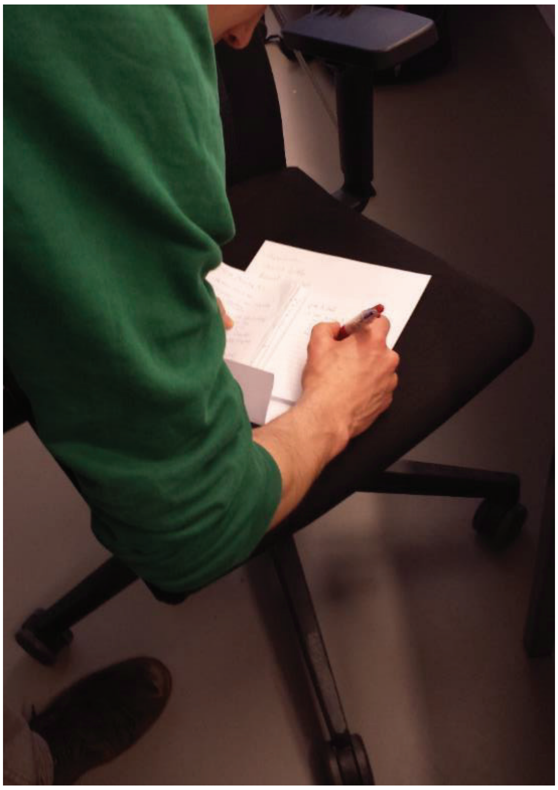


Figure 6 reflects one moment in three days of exploration with materials and testing various operations and procedures, in an attempt to make the copper laminate on a flexible substrate sensitive enough to accept, and retain, the ink from a scrap of glossy paper. The various experiments, especially those that were successful, were duly annotated (Figure 11), and involved all the actors who took part up to the step illustrated by Figure 6: the printed circuit film, the glossy paper, the ink, the printer, the iron and the iron board.

For each of these actors, the team of three designers tried various combinations and modulations: various thicknesses of glossy paper from different magazines; different surfaces of glossy paper (e.g. already printed or blank); alternative printer settings; other ways of treating the copper surface of the film (cleaning, polishing, sanding); setting the iron differently (heat, timings); variant consistencies of board to which pressure was applied, i.e. creating greater rigidity, by adding a ceramic tile (Figure 12).

What is apparent here, is how the change in consistency to the support of the copper lamina, demanded new ways for the pressure exerted by hand through the iron, to affect the envelope-surface of the printed glossy paper so that it will have, in turn, affected the envelope-surface of the copper laminated film. Indeed, the film exerts much less resistance to pressure than a board, thus making the contact between surfaces-envelopes less firm and continuous (Figure 9b).

Vignette 3: Sanding copper

After trials of cleaning and polishing the surface, it was the lightly abrasive sanding of the copper lamina that provided the required result. This proved successful in enabling the copper lamina to absorb, and retain, the ink by effecting change to its property of being even and sliding, to one that was rough and braking (Mattozzi, 2017).

Figure 12. The transfer of laser-printer ink from glossy paper to copper laminated film. A ceramic tile can be spotted below the cloth protecting the glossy-paper–printed-circuit-film coupling (Photo: Alvise Mattozzi)

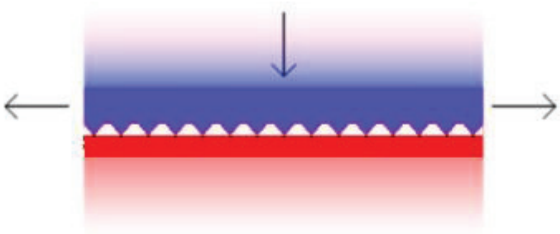


Figure 13. The action of a rough surface-envelope (sandpaper) over another one (copper lamina) (Our drawing)

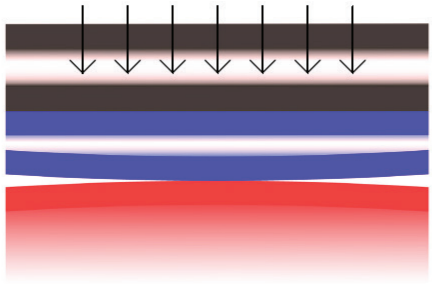


Figure 14. Non effective pressure as result of a lack of resistance from the pressed bodies (Our drawing)

Within this process, the bodies, and especially the hands and arms of the two junior designers, needed to become sensitive to the copper laminated surface to avoid abrading the delicate material too much, but enough to allow it to retain the ink. Thus, the junior designers working on the etching had to become sensitive enough to alter the envelope-surface of the copper laminated film without getting to its substrate-core. Indeed, going beyond the copper envelope-surface and getting to the film, would have disrupted the continuity of the copper envelope-surface, thus preventing the electric signal to flow undisturbed over the circuit board. Importantly, the action of manipulating the sandpaper was not only about pressure, it was also a matter of penetrations: the tiny glass grains constituting the fine sandpaper could, with the sweep of the designers hand, easily penetrate the copper lamina, scrape it, and take part of it off (Figure 13). That is why, a more violent action would not only have altered the envelope-surface of the copper laminated film, but actually removed it, producing an anticipation of the action of the acid, but without the constraints the acid has to undergo.

Within this specific interaction amongst bodies, whereby the designers and the copper lamina became, *qua* Latour (2004), more articulated, and thus, also more sensitive, the sandpaper became less, losing elements of its roughness that was passed to the copper lamina – the "progressive metamorphosis of the one into the other" Souriau (1956: 12, our translation) talked about.

Vignette 4: Finding the right resistance to pressure

The designers also had to develop a sensitivity to the ironing board, which was used as a surface to press together the inked glossy paper and the copper laminated film. They felt that the ironing board, with its soft foam rubber envelope, itself enveloped further in a soft cloth, was too supple for a flexible material like the film. Thus, the right level of pressure to generate the necessary resistance to make the two envelope-surfaces affect one another to the correct degree of adhesion (Figure 14) could not be reached. This accounts for the reason they had to make the surface-envelope of the ironing board harder by introducing a ceramic tile (Figure 12). At the same time, they

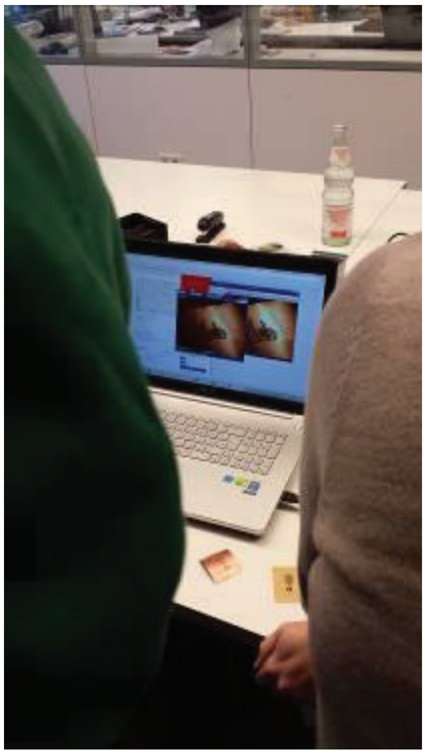


Figure 15. The etched circuit displayed in the computer screen through an electric magnifying glass (Photo: Alvise Mattozzi)

protected the copper laminated film from the hot iron, with a piece of cloth that enveloped the bodies of the paper and of the film, while they were pressed together.

Vignette 5: last steps

When, after a close examination through a computerised magnifying glass (Figure 15), the transfer of ink was judged to correspond with the original drawing (Figure 5d), the copper laminated film was inserted in a tray filled with acid to carry out the actual etching (Figure 7). The results of the etching process were again closely scrutinised to assess the continuity and connective integrity of the circuit.

Discussion

We describe-analysed only a small portion of what was a long and complex process. The project took place over a considerably greater number of phases of production than those, which we have focused on, although many are documented in figures 5a, b, c, d.

It is clear that many translations had to occur in order for a translation of a gestural code into an alphabetic one was able to occur. More specifically, we have shown that, for the glove to acquire the necessary sensitivity, many other bodies, not least the bodies of the designers, had to become more, and sometimes less, sensitive. In other words, many bodies were rearticulated, as outcome of the unfolding of mediations.

In order to account for these translations, we have used the descriptive-analytical model developed in the previous part of this article that drew upon insights of Latour, Mead, Ingold and in particular Fontanille's semiotics of the body. In this sense, we have understood bodies as entities, which are constituted by observable relations between envelope-surfaces and core-substances, and that interact through pressure, penetration, envelopments, expulsions, disenvelopments (Figure 3). In our vignettes, we have focused primarily on pressure and resistance to pressure. However, in everyday activities other kinds of interaction take place all the time, and what we argue is that these interactions need to be described-analysed to account for 'aesthetic practices' and for processes of instauration. We have done this through the rich analytical description of instauration of a specific portion of an electric circuit - or more accurately the prototype of a circuit.

As a means of clarification, we emphasise here that the features that may appear to be attributed to the various bodies involved in interactions we have described as rough, even, sliding, braking, soft, hard, rigid or flexible, are not properties of particular bodies, but the result of their interaction. To wit, something is rough if when in parallel contact with another body, it penetrates the envelope-surface of the latter, which is precisely the result of the contact between the sandpaper and the copper laminated film (Figure 13). Equally, something is soft when it yields to the perpendicular pressure exerted by another body (Figure 9b), and so on (Mattozzi, 2017; Parolin and Mattozzi, 2013). It was because the circuit needed to yield to the various shapes of the hand, and to its contracting and expanding movements, that film was needed as a support for the copper lamina. Such exigencies then, related to the consistency of support for the circuit, influenced the following processes and all the bodies taking place to the overall etching.

Even the latter entailed contact among bodies. The enveloping body of the acid takes away, by dissolution, the envelope-surface of the copper laminated film, leaving the core-substance of the film intact (Figure 10). However, etching is not peeling-off. The acid should not affect the entire envelope-surface but only "select" (Bastide, 1987) what not protected by a further envelope, provided by ink. This explains why the latter has to be translated – translated, in the etymological sense of transferred, in this case – and made to stick, on the surface of the copper lamina.

These transformations changed relations among bodies but also the "network within" (Parolin and Mattozzi, 2014; 2020: 38 and 48) of the copper laminated film – i.e., the network provided by the relations between core-substances and envelope-surfaces, related in this case to the film and the copper over it – as well as other features related to the texture, such as the relation between sliding and breaking. Through each phase of the process, bodies in contact with other bodies were clearly affected by these contacts.

This ought not ignore that there were also bodies that, though taking part in the interactions we have described, took part also in other interactions.

These are especially the bodies of the designers. These bodies were able to detach themselves from the chain of bodies we observed, and to occupy a space tangential to the process, observing and comparing the bodies in the chain of interactions from a distance. They did this by addressing other bodies, and starting other chains, illustrated for example, by the pen and the notebook in Figure 11, or by the electronic magnifying glass in Figure 15.

This last detail clearly demonstrates that such detachments take place through artifacts too. Therefore, the detached observation the bodies of the designers are able to unfold on the chain of other bodies, do not transcend the situation but rather emerge from it. They constitute just a chain intersecting another chain. As we have shown elsewhere (Parolin and Mattozzi, 2013), it is through such shifts between more engaged positioning within a chain and more detached ones that knowledge is produced and can be recorded and fixed (Figure 11). It is produced not only through observation, but especially through comparison of 1) the elements constituting the observed chain and of 2) the relative positionings of the observer, the one engaged in the chain and the one detached from it.

Conclusions

Our research explores the grounds upon which STS and 'aesthetic studies' can develop dialogues around and about 'aesthetic practices' as assemblages of *aisthesis* and (*palim*)*poiesis*. Reconstructing the relatively long history of exchanges between STS, and especially ANT, and 'aesthetic studies', we reflected on how such exchanges have allowed ANT to think about mediation and how to account for artifacts taking part to these mediations. In the light of this, we proposed that such dialogue can be developed further by regarding 'aesthetic practices' as ones that encompass bodies translating bodies, including human and non-human bodies, and that can be further unfolded through a model, able to describe-analyse these complex translations among bodies.

Drawing on Fontanille's (2004) semiotics of the body, we developed the notion that bodies are constituted by relations between a core and one or more envelopes, which repose and dispose interactions among bodies through contacts involving pressure, penetration, envelopment, disenvelopments and expulsions, that can affect the envelope, the core or the entire body. To frame this explicitly within an ANT framework, we claim that through this model we have provided actants with a body.

We have used the model to describe-analyse various phases in the development of a complex prototype of an electric circuit for a digital interactive device (in the form of a glove) by accounting for the translations among bodies. In this example, the model was able to reveal various relations between core and envelopes, by focusing on the way a printed sheet could or could not affect a copper laminated film through pressure, the use of pressure and penetration of sandpaper upon copper lamina, the role of the bodies of designers.

However, whilst the model proved substantively telling in this instance, not all aspects of 'aesthetic practices' and the relations they entail can be reduced to translations among bodies. Other relations, like those to figures, values, meanings, as well as those to contrasts among shapes, colours, consistencies and textures, which we have tackled in part, cannot be ignored when considering 'aesthetic practices'. 'Aesthetic studies' can provide STS with notions, categories and models that can help in accounting for these relations.

Nevertheless, we strongly suggest that describing translations among bodies is an necessary step in accounting for 'aesthetic practices', as well as other relevant aspects related to the interaction among bodies, such as affect and its attendant sensations, passions, and emotions.

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The present paper is a collaborative effort by the two authors. If, however, for academic reasons individual responsibility is to be assigned, Mattozzi wrote the following paragraphs: "ANT and the social history of art: mediation", "Beyond doing and making", " A model to describe-analyse bodies translating bodies", "The case", "Conclusions"; Parolin wrote: "Introduction", "Exchanges between STS, 'aesthetic studies' and 'aesthetic practices';"ANT and Souriau's aesthetics: instauration", "Describing-analyzing 'aesthetic practices'", as situated and distributed translations among bodies "Discussion".

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Notes

1 *Ästhetische Praktiken (nach Bologna)* was also the title of a research project lead by Priska Gisler, Elke Bippus and Monika Kurath (www.aesthetischepraktiken.com). We started the reflection we here present in order to devise a contribution for a conference that took place within the framework of that research project. The conference, called *STS Turns Aesthetic*, took place at the ETH Zurich on the 7th and 8th of November, 2013.

2 Rancière ([2000] 2013: 8) seems to actually distinguish between 'aesthetic' and 'artistic' practices. Indeed, he says that 'aesthetic practices' are "forms of visibility that disclose artistic practices" (Ranciére, [2000] 2013: 8).

3 In Boucher et al. (2018), the same authors refer to aesthetics as mainly related to form, intended as shape (Ovalle, 2018), and hence to visual appearance and, on a lesser extent, to auditory perception. Such relevance given to 'aesthetics' as visual appearance, probably led them to resort a framing of aesthetics related to beauty (Michael, 2018b), for them a relevant issue in engaging 'aesthetic' publics (Wilkie, 2018).

4 Shusterman has preferred the term 'soma-' over 'body-' or 'corporal-', because 'body', as well the latin 'corpus', can refer to human as well as to non-human and even to non-living bodies, whereas he intends to focus on human bodies. Given that, as it will become clear below, we will refer to bodies in general, the practices we are interested could also be called 'corporaesthetic practices'.

5 For the use of Dewey in relation to STS and aesthetic practices, see also Binder et al. (2011).

6 The *Handbook of Science and Technology Studies* is the major publication representing STS as a field (Ienna, 2018), being directly promoted by 4S, the leading association of STS scholars. The previous edition of the *Handbook* had a chapter signed by two of the three authors of the chapter discussed here, Regula Valérie Burri and Joseph Dumit (2008), on "Scientific Imaging and Visualization", which ended by noting the increasing relevance of hybridization between science and art.

7 We could also add a fifth way of STS engagement with 'aesthetic practices': the case of STS scholars working as mediators between scientists, artists and designers, within larger research projects, providing not only translations among different competences, but also a meta-reflections (Calvert and Martin, 2009; Ginsberg et al., 2017).

8 In a similar way Parolin and Pellegrinelli (2020a) propose the term 'creative laboratory' to stress the experimenting in the rehearsal room during a theatrical production.

9 See, note 1.

10 Here Latour (1998: 422) is specifically talking about studies of the "visualization in science and the visual arts". We, following the recent history of STS (see, note 6), extend the argument beyond studies of visualizations.

11 It is no coincidence then that, whenever Latour wants to provide examples of good ways of describing artifacts, he mentions examples of descriptions carried out in the history of art (see for instance Latour, 1992: 255, n. 2; 2005: 237, n. 332).

12 On 'anaphoric progression' or 'trajectory' as a useful notion to enable empirical research on 'aesthetic' production, see also Strandvad (2017).

13 Michael's (2018b) reflection on 'eventuation' is similar to Souriou's and provides a way to think 'aesthetic practices' that is similar to ours. Ingold's (2013) reflection on making is also similar to Souriau's one, even though he seems more interested in the process and the way the maker is engaged in it, rather than in the instauration as the outcome of the process, which produces the possibility of a disengagement.

14 Binder et al. (2011) and Guggenheim (2011) refer also to this article as paradigmatic of ANT as sociology of translation. More specifically, Binder et al. (2011) refer to it for reasons similar to ours. They intend to problematize the transformations of representations in the process of design. For these authors, different design representations change during the design practice, and initial ideas are subject to metamorphoses and further materialization in new representations.

15 Thus not considering 'immutable mobiles', which are a feature of the 'scientific' mode of existence (Latour 2013).

16 Lise Justesen (2020) reminds us that for Latour there is no difference between description and analysis. We agree, given that we can consider an analysis a description of relations (Mattozzi, 2019). On the relevance of description for ANT in relation to 'aesthtetic pratices', see Storni, 2015.

17 It is useful to remind readers that the original version of Akrich ([1987] 1992) was published in French with the title "Comment décrire les objets techniques?", which translates "How can we describe technical objects?".

18 When Guggenheim (2011) criticizes ANT-sociology of translation for not problematizing its own translation of practices, he forgets Latour's infralanguage. The latter allows not only to translate, but also to account for these translations. The present article delves into this issue proposing a way to translate senses, which is not only verbal, but also visual (see below). Therefore, the present article addresses all the main issues raised in Guggenheim (2011), showing that they can be tackled by ANT's descriptive methodology. On similar grounds, we partially reject Michael (2018a: 118) critique of classical ANT as "'too' empiricist". Considering infralanguage allows to acknowledge the ways in which ANT is instrumental in "'making' the object it is studying". Therefore, we do not feel the urge to introduce a 'post-ANT', before having delved into all the aspects of 'classical ANT'.

19 Connecting 'affect' with bodies – regardless if they are human or not – is actually getting back to Spinozian origin of the concept, which also inspired Deleuze (Blackman and Venn, 2010; Clough and Halley, 2007; Gregg and Seigworth, 2010; Massumi, 1995, 2002, 2015).

20 We deem that our attempt is not dissimilar to the one by Ash (2015), even though, differently from the latter, our takes fully place within an ANT framework.

21 The issue we raise, which emerging ANT raised much before us, is not dissimilar to the one raised by Anna Tsing (2017) about learning from natural history how to describe more than human encounters: natural history, which in Tsing broader framework takes the place that (social) history of art and aesthetic studies have for ANT, "requires constant attention to form, texture, and color, constant speculation as to pattern" (Mathews, 2018: 154).

22 As for the present article, we limited ourselves to use Fontanille proposal to account for interactions among bodies and senses, especially touch.

23 As far as we know, Ingold's (2007) Gibson derived categories have not been used in empirical descriptions of artifacts. Ingold himself does not seem to be interested in developing them as systematic descriptive categories, even though he does refer to them here and there (e.g., Ingold, 2013).

24 Heavily influenced by phenomenology, Fontanille (2001) introduces a strong asymmetry between human and other bodies, which, for us, is not only theoretically problematic but, most importantly, severely limiting methodologically.

25 Using Fontanille categories and model to integrate Latour's infralanguage is consistent with what has been done by Latour, given that his infralanguage has been articulated mainly through terms, categories and models taken from Greimasian semiotics (Mattozzi, 2019), of which Fontanille is one of the main continuators.

26 This is clearly a play on Latour's (1990: 103) famous sentence about technology as "society made durable".

27 Respectively, Tiago Martins, Chiara Esposito, Fabian Werfel.

28 http://www.robotplatform.com/howto/pcb%20etching/pcb_etching_1.html, accessed on the 26th May 2020.

29 http://fritzing.org/learning/tutorials/pcb-production-tutorials/diy-pcb-etching/, accessed on the 26th of May 2020.

30 This process works as an exemplification of what Binder et al. (2018) call 'metamorphoses' of representations.

31 See references in notes 28 and 29, italic is ours.

Complexity sciences: A scientific platform

Fabrizio Li Vigni

Sciences Po Saint-Germain-en-Laye, France/livignifabrizio@gmail.com

Abstract

Social scientists have proposed several concepts to give account of the way scientific life organizes. By studying 'complexity sciences' – established in the mid-1980s by the Santa Fe Institute in New Mexico (USA) –, the present article wishes to contribute to interdisciplinary studies and emergent domains literature by proposing a new concept to describe this domain. Drawing from Bourdieusian sociology of science and STS, a 'scientific platform' is defined as a meeting point between different specialties, which, on the basis of a flexible common ground, pursue together shared or parallel socio-epistemic objectives. Most of the specialties inscribed in complexity suffer from a relative marginality in their disciplinary field. The term 'platform' metaphorically refers to what the heterogeneous members of the collective mutualize, both in cognitive and social terms, in order to exist and expand.

Keywords: Complexity, Santa Fe Institute, interdisciplinarity, disciplines, emergent domains

Introduction

Several notions of 'complexity' circulate in science and technology. The communities that coalesce around some of them share a common definition, a set of operational tools and references, an ensemble of meeting spaces, and an institutional project (Li Vigni, 2018a). One of these communities christened herself as 'complexity science(s)', a field that can be defined as an interdisciplinary and transnational association of specialties, whose aim is to computationally model and simulate natural and social 'complex systems' (Waldrop, 1992; Helmreich, 1998; Williams, 2012; Li Vigni, 2018b). These are defined as big ensembles of heterogeneous elements whose interactions produce emergent properties that are not deductible from their microscopic level: because of the vagueness of this notion, basically everything from ecosystems to cities, from epidemics to financial markets can fall within it (Mitchell, 2009). The field has been launched in the mid-1980s by a group of senior physicists from the Los Alamos National Laboratory and other American universities, with the aim of applying computer and interdisciplinarity to life and social sciences. After two years of meetings and discussions, in 1984 the group established a small private research center called the Santa Fe Institute (SFI) in the New Mexican city of Santa Fe. Even if historically this group is not the first reclaiming the study of complex systems, the SFI made organizing "a general science of complexity" its core mission (SFI Arch. #1: 3). The institute succeeded in establishing a standard of complexity sciences through publications and educational devices. Moreover, thanks to the symbolic capital of the founders and to a series of general audience bestsellers (Waldrop, 1992; for a longer list, see Williams, 2012: 194), the SFI has since then generated many vocations around the world and inspired the foundation of several dozens epigone institutes.

Nevertheless, the unity of complexity science is highly questionable, both under an epistemological and a sociological viewpoint (Li Vigni, 2020a; Li Vigni, 2020b). In a precedent work, I have retraced the history of the SFI and argued its failure in establishing complexity as a new discipline (Li Vigni, 2020c). While its cultural influence is undeniable (Thrift, 1999; Taylor, 2003; Urry, 2005), the generalization of an idiom or a set of metaphors such as 'complex adaptive systems', 'networks', 'edge of chaos', 'tipping point', 'emergence', etc. does not imply we face a scientific field in the Bourdieusian sense (Gingras, 1991). If "[t]he central function of the institutionalization of the disciplinary community consists in preserving the permanence of the disciplinary activity through *reproduction* of its potential" (Guntau and Latkau, 1991: 21, emphasis in the original), then complexity cannot be considered as a discipline. Complex systems groups are very common in physics and mathematics faculties – a little less among life and cognitive sciences. But the institutes and degree courses, summer schools, masters, and PhDs that explicitly and primarily inscribe in this label are a few. That is because the academic identity of complexity specialists remains anchored to their disciplines.

At the same time, complexity specialists have theoretical affinities, show reciprocal acknowledgements, meet in thematic conferences, pursue collective funding, and weave research collaborations for example through what the SFI called the "integrative workshops", sort of brainstorming conferences where participants pursue transversal and interdisciplinary theories and models. From an object-driven viewpoint, we face a paradox: if the boundaries of complexity seem soft, undefined and open, its label has nevertheless a consolidated, acknowledged and clear identity. When looking at complexity sciences, it is indeed possible to feel a palpable tension between the solidity of this interdisciplinary field and the openness of its epistemic, social, and institutional boundaries and features. At the beginning of SFI's history, its founders wanted to establish a new discipline. Up to the mid-1990s, they invested their efforts into the creation of a "general theory of complex adaptive systems" – in reference to the evolutive aspect of living and social systems (Cowan et al., 1994). The project was nevertheless abandoned in 1995 after the publication of an article authored by scientific journalist John Horgan and entitled "From complexity to perplexity" (Horgan, 1995). Therein, the journalist bitterly criticized complexity science for being "flaky" and the SFI for being "fact-free". Horgan's article had a huge impact on the New Mexican institute's image and internal organization. Its Board of Trustees and Scientific Advisory consequently operated several changes: some people were excluded and the pursuit for a general theory of complexity was officially abandoned. From then on, the institute's members redirected their efforts towards the construction of local but transversal theories about different phenomena (e.g. robustness, contagion, aging, animal metabolism, ecosystems formalization, city evolution, etc.) (SFI, 1997, 2000b, 2004; Marquet et al., 2014). Albeit this domain is often well recognized by insiders and outsiders, and often qualified as a "paradigm" from which to get inspiration to renovate other disciplines[1], young researchers having spent a period in a complexity institute may encounter problems in the suite of their career. Mavericks and marginal scientists with an unusual path may find there a temporary shelter, but, as it has been observed for other interdisciplinary fields (Prud'homme and Gingras, 2015; Lewis et al., 2016; Génard and Roca i Escoda, 2016), they run the risk of experiencing troubles in finding a permanent job once outside the complexity "free-trade zone", since they "have vast persuasive work to do, for instance in demonstrating that work done in 'sociophysics' has 'enough' physics" (Williams, 2012: 166-167). What kind of scientific organization is then one that confers an "ambiguous reputation", to cite a German biophysicist from the University of Cologne (interview, 18.11.15), but still continues to exist within an environment – academia – where reputation is central (Bourdieu, 2004)? Even if the initial disciplinary project of SFI founders was abandoned, many scientists inscribe in this domain or get inspiration from it. How to explain such a paradox? If complexity sciences are not a discipline, then what are they or, at least, how can they be thought of?

In the present text I wish to address the question of how to characterize this field. From a theoretical viewpoint, social scientists struggle to find a term to describe interdisciplinary fields in general. One can think of the several 'studies' (STS, gender, postcolonial, area, futures, environmental, animal, digital, game, etc.), but also of fields like cognitive sciences, Earth system sciences, nanotechnologies and others. Some scholars prefer to adopt terms like 'epistemic cultures', 'styles of thought', 'invisible colleges' or 'research programs' for they consider that more classical terms like 'discipline' and specialty' are inadequate before such heterogeneity of practices and scales (Granjou and Peerbaye, 2011). Others – followed here – think the disciplinary level can still be pertinent, even if that means we need to go beyond it with new concepts, such as 'interdiscipline', 'transdiscipline' and the like. Drawing from Bourdieusian sociology of science and STS, this article proposes to contribute to the interdisciplinary studies and emergent domains literatures by introducing the term of *scientific platform* to make sense of complexity sciences and, I guess, other similar fields. If on the one hand it must be admitted that the concepts to define meso- or microscale research groups proliferate (see Tari, 2015 for a review), on the other one the terms that take into account the disciplinary level are not as numerous. Moreover, existing concepts fail to grasp the specific social configuration that complexity sciences manifest on an institutional and organizational level. The thesis of this article is that complexity has a specific socio-epistemic existence, partly determined by the conception of science that its members have and partly shaped by the specific historical context in which this domain appeared. Complexity sciences can be defined as an association of fledgling and/or marginalized specialties, which ally under the same label – sharing the same tools, views and spaces – in order to pursue common or similar epistemic and institutional projects.

This article is structured in four sections. The first one describes the materials and methods upon which it relies. The second one offers a general overview of complexity sciences from a historical and geographical viewpoint. The third reports the way complexity scientists self-perceive within the specific historical context in which their field has emerged. The fourth section describes the complexity domain under three axes (epistemic, ontological and social); it introduces and discusses the concepts that social scientists have produced to describe scientific communities by focusing on the disciplinary level; it finally presents the interest of the *scientific platform* concept. The aim of this proposal is not to essentialize nor legitimize complexity, but to offer social scientists a concept to seize a dynamical phenomenon both in its specificity and generality.

Materials and methods

The present work stems from a PhD research in sociology dedicated to the study of complexity sciences. The material of the thesis is composed by scientific literature, institutional archives, a dozen laboratory visits and 198 interviews – systematically transcribed – with 170 different people from Europe and the US. 115 of these were complexity scientists; the rest of interviewees were staff employees, other complexity theories specialists, as well as a few journalists, policy makers and NGO or think tank leaders. Such material contributed to form an overall view of the field under study here.

Interviews were semi-structured – partly open and individualized, and partly following a general framework. Such framework contained a dozen questions about personal pathway, view of complexity sciences, scientific practices and methods, as well as institutional attachments and objectives. The bulk of the interviews was determined by the choice of the pivotal institutions taken as study objects – the SFI[2] and the Parisian Complex Systems Institute[3] – in order to explore the hub of the American and international community on one side, and the hub of the French community – one of the biggest and most active in the world – on the other. The rest of the researchers came from other laboratories inscribed in complexity sciences in Europe and the US.

As for the archives, a support is particularly of help here. From 1986 to 2014, the SFI published 40 issues of its Bulletin. The articles it contained were written by the staff members, resident scientists and freelance journalists. It was addressed to the members of the Board of Trustees, the research officers, the advisors, the scientists, the donors, as well as to university, industrial and governmental directors. Its aim was to inform such a public about the scientific and administrative programs of the institute. The Bulletin was published once to twice per year. Printed in 5000 copies, it was available for free upon request. Later, its publication became electronical and old issues were digitalized, before the bulletin was suppressed for economic reasons. The Bulletins are an excellent material to retrace SFI/complexity history, network and theoretical content.

As for the approach followed here, to study scientific communities' organization in general – and complexity sciences' in particular – it is important to have a multiscale, multidimensional and dynamical perspective (Abbott, 2010). To make sense of the scientific group under study, a specific and a general frame have been adopted. The specific frame is the definition that Gingras (1991) and other social scientists give of the discipline as a *professional autonomy* device (Hufbauer, 1971; Goldstein, 1982; Whitley, 1984; Guntau and Latkau, 1991; Lenoir, 1997; Fabiani, 2006; Bulpin and Molyneux-Hodgson, 2013). Accordingly to the authors, one or another of the following elements can be more or less emphasized: the role of education and degrees, courses, and PhD curricula in order to perpetuate a field by the training of neophytes; the institutionalization of a field through the classical venues of science (societies, conferences, journals, departments, committees, facilities, etc.); and the role of social support, which can come either by the State, the industry, the general public or all of them. From this perspective inspired from sociology of work, the role of scientists is analysed under the professional dimension – certified competences are requested for specific tasks (education, research, industry, governmental needs, etc.) and are rewarded through ad hoc occupational categories, social functions, salaries and budgets. The second frame is more generic and gets inspiration from STS at large, according to which epistemic, ontological and social levels are interdependent and indissoluble (e.g. Felt et al., 2017; Law, 2010; Vermeulen et al., 2012; Woolgar and Lezaun, 2013). This is the reason why, in order to present the specificity of complexity sciences in the fourth section, I have isolated three axes: epistemic (theoretical objectives and inquiry tools), ontological (view of complex systems) and social (institutions and meeting spaces).

History and geography of complexity sciences

This section is dedicated to a quick historical and geographical panorama of complexity sciences, from their inception at the SFI in the 1980s to the present-day, where six dozens institutes scatter around the world.

During the founding meetings that took place in 1982-1984, SFI's architects only agreed on the will of using the computer to foster interdisciplinary research, but diverged as for everything else: the size of the institute, its scope and even its research topic (Li Vigni, 2020c). Some of them advocated for the study of artificial intelligence, a few were for cognitive sciences, while others wanted the institute to focus on life sciences. As one of the founders, physicist Murray Gell-Mann, retrospectively explained in 1994, "In the beginning, we couldn't see clearly what sorts of emerging scientific syntheses we should seek" (SFI, 1994: 25). Only after several discussions, complex systems were established as the general object to make the institute community work on. The institute was then settled in 1984 under the name of Rio Grande Institute, before getting its actual name one year later (Cowan, 2010). The establishment of the "science of complexity", as SFI's founders initially used to call it, was a top-down social engineering process that relied on several strategies. A very important one consisted in mobilizing Senior Fellows' own economic, social and symbolic capitals. Not only the founders were the first important donors to get the institute off the ground, but – as the official bulletin of the SFI later wrote – they also "knew everybody. They could just pick up the phone" (SFI, 2004: 8). Through the founders' social networks, the institute obtained the first public contribution from the National Science Foundation, as well as the first private money from foundations (like MacArthur) and companies (like Citicorp). The symbolic capital of the Senior Fellows had been consciously mobilized to increase the credibility of the SFI's endeavour, as the first president George Cowan explained to one of the bulletin writers: "We have a roster of National Academy types and Nobel winners, which suddenly did something very important for the whole notion [of complexity], that is, to make it look more respectable" (SFI, 1988: 5). Another important strategy consisted in fostering a positive mediatic coverage of the institute[4]. Moreover, SFI has importantly directed its fund raising efforts towards the private world (Li Vigni, in preparation), but has always addressed academia to lay down its scientific existence and continuity, through scientific publications and pedagogic devices like the summer schools[5]. While the scientific society dimension has not been invested by the SFI, it represented one of the most structuring tools of the European community[6].

Today there are more than sixty complexity institutes in the world. Physicist and entrepreneur Stephen Wolfram has published on his blog an approximative list of these centres, which are present on all continents, except Africa, with a particular concentration in the US, in the UK and in France[7]. These institutes have passed from a couple to more than ten between 1980 and 1994 (14 years), from ten to twenty between 1994 and 2001 (7 years), then from twenty to forty between 2001 and 2005 (4 years), and finally from forty to sixty between 2005 and 2010 (5 years). After the boom of the first 2000s – very likely due to the success of network theory (Pastor-Satorras and Vespignani, 2001; Barabási, 2003; Watts, 2003; Newman, 2018) – the curb reached a plateau and is today probably entered in a degrowth trend (in the sense that some centres close down). The SFI self-attributes the credit of such a dissemination: "imitation is the sincerest form of flattery" (SFI, 2007b: 7). Yet, while there is no doubt that it has indeed inspired many of these centres through its mediatic coverage and direct effort in the international outreach, the laboratory visits I realized in a dozen complexity institutes in Europe and the US suggest the need to nuance this point. Among such institutes, there are at least six different types. The first category is that of the centres which preceded the SFI: even if they integrated some of SFI's characteristics after its appearance, these institutes have never shared the totality of the tools, discourses, objectives and organizational features as the American institute[8]. The second category includes the faithful SFI epigone centres, which interpret a restricted version of complexity, sticking to the boundaries that have been established by the American ancestor, and which also follow the original model in what concerns the type of institutional funding philosophy – mainly addressed to private actors such as enterprises and foundations[9]. Some centres have been established or renovated to imitate the SFI, but still keep some distinctions on the institutional level (mainly based on public funds) and on the theoretical one (some SFI approaches are missing and new ones are introduced). While explicitly aligning themselves with the "SFI tradition", these centres wish to innovate complexity sciences[10]. Moreover, some centres know and explicitly get inspiration from the SFI, without sticking to its epistemic discourses and objectives, and settle up very different institutional organizations where, contrarily to the SFI who only hosts theoreticians, the latter coexist with practitioners in the same environment[11]. The fifth category gathers centres that adopt the term of complex systems more for institutional convenience than for adherence to the American ancestor. In these cases, the label is perceived as an efficacious hat that can federate heterogenous and multidisciplinary teams[12]. In the sixth and last place, it is important to mention all the other complex systems institutes that make no reference to the SFI and whose members often ignore and sometimes despise it: in these public centres, the reference to complexity mainly draws from statistical and condensed matter physics, where the term of complexity has been in usage since the 1970s without a flagship rationale[13]. Whatever their category, most of these laboratories operate as visiting institutions, so that the number of resident researchers is often small. The majority of their affiliates are temporary associates that either spend a short stay and then go away, or – like in the case of the SFI and its followers – are formally associated to the institute for a long time, but only spend a few weeks per year there.

While the fieldwork which this article lays upon was limited to some European countries and American states, a quick Internet tour shows that certain complex systems institutes in the world seem not to be active anymore[14]. As for the topics, some seem specialized in physics, others in robotics or engineering, others yet in biomedicine[15]. The variety of the subdisciplines involved and of the institutional forms taken by these networks, as well as their ephemerality, suggest porous and instable boundaries. Furthermore, while more or less technical introductory books on complexity sciences are numerous (Byrne, 1998; Kaneko and Tsuda, 2000; Miller and Page, 2007; Mitchell, 2009; Fieguth, 2016; Thurner et al., 2018; Tranquillo, 2019; Peletier et al., 2019), handbooks (Gros, 2015; Mitleton-Kelly et al., 2018) and university teachings are few[16]. SFI's summer schools in complex systems continue to exist and additional ones have appeared elsewhere, but dedicated PhD programs stay rare[17]. Masters in complex systems appear to be a little more numerous and faithful to the SFI's tool belt[18]. These programs are far from being present in all countries and universities. In general, teachings in complexity sciences seem to focus on a few specialties and never include all those inscribing in the label.

Complexity scientists' self-perception and context

Before analysing complexity sciences, I will investigate how its members think of themselves and how they conceptualize their field. Exploring this question will lead us to evoke the question of the historical moment in which complexity has emerged and developed.

Today almost no-one of the scientists inscribing in this label believe that a discipline of complexity exists or will ever exist: out of the 115 people interviewed, only six still endorsed the project, and they were all scientific entrepreneurs but one. In a 2007 report for the European Commission, one of them wrote that "The promise of the *science of complexity* is to provide, if not a completely unified approach, at least *common tools* to tackling complex problems arising in a wide range of scientific domains" (Weisbuch, 2007: 3, emphasis in the original). But since the end of the 1990s, the SFI bulletin started talking about complexity more as a "way of thinking" than as a discipline (with some exceptions here and there). Moreover, the overwhelming majority of my interviewees use the plural to talk about complexity sciences and employ different formulas to qualify this field. Some talk about it as a "sort of framework or frame of mind" (interview with an SFI bioinformatician, 27.03.15), or as "a philosophy and an approach […] that can be used in many different disciplines" (interview with an SFI bioinformatician, 21.09.16). Others talk about it as a "comfortable umbrella for interdisciplinarity" (interview with a Lyon Complex Systems Institute physicist, 15.09.15), or as a "perspective" (interview with an SFI anthropologist, 23.09.16). A French computer scientist describes complexity as an "a priori on the way [he] see[s] things" (interview with a Parisian Complex Systems Institute computer scientist, 31.01.17).

Like the institutes, individual researchers show different attitudes vis-à-vis the field. While complexity founders can be seen as militants faithful to the initial project of a new science – or to a renovated project of a "transcience" which be capable of synthesizing different fields (SFI, 2011: 2) –, other members of the community have very different postures. Some scientists have jumped into complexity only temporarily in order to operate a disciplinary reconversion, such as from physics to computational epidemiology or to social sciences. Others have used it to renovate their own discipline by applying established physical and computational tools to new study objects – e.g. quantitative geographers applying power laws and agent-based modelling to cities' dynamics. For certain researchers, complexity represents a place where to "have fun" out of their disciplinary frames, within which they need to stay if they want "a career progression" (interview with a Parisian Complex Systems Institute computer scientist, 23.03.16). Yet another category of researchers is that of the scientists who "shy away from mentioning complex systems science" within their (often adoptive) disciplinary community, because "they're afraid, in a way, to be offensive" when bringing their "revolutionary" tools into the welcoming subdiscipline (interview with a European Commission scientific project officer, 20.03.17). Lastly, in all my laboratory visits I have met PhD candidates and post-doctoral researchers who, when asked about their reason of being there, never mentioned the study of complexity in itself. They were rather attracted by the development of a given approach, by the use of a certain technical instrument, by the presence of a particular researcher or an established subdiscipline. Except the scientific entrepreneurs who actively pursued the creation of funds and institutions for the development of complexity sciences as such, many of the researchers interviewed often avoid to employ the "complex systems" keywords, because, as a German biophysicist explained,

> The label "complexity" and "complexity science" sometimes get a kind of ambiguous reputation. [...] We were making a project, then came the question whether to put complexity in the title, and everybody said it was "too oversold, we cannot associate with that, we have to come up with something else". (Interview with a German biophysicist from the University of Cologne, 18.11.15).

The paradoxical existence of complexity sciences lays in the fact that researchers adhere to them intermittently or without a full engagement, as well as in the fact that candidates to project funding can happen to fake or twist their approach to adapt to the call. As a scientific project officer from Brussels explained to me, complexity sciences have sometimes appeared as a "sexy" field so to attract "people saying they have ideas from complex systems science while they don't" (interview with a European Commission scientific project officer, 20.03.17). Such elements are better understood by taking into account the historical context started in the 1980s in which complexity sciences have evolved (Li Vigni, in preparation). According to several historians and sociologists, the technological and scientific worlds have entered, in the last forty to fifty years, a new "regime of knowledge production", characterized by the State retraction from university and research, by the increasing submission of these to market imperatives, as well as by the generalization of a funding strategy based on the logic of projects (Pestre, 2003; Busch, 2017). The latter has in particular been accompanied by a shrinkage of funds for investigation, by an invitation to interdisciplinary work (Gibbons et al., 1994; Weingart and Stehr, 2000), and by a frequent turnover of "fashionable" topics. Similar to fads, labels such as nanotechnologies, Artificial Intelligence, Internet of things or complex systems are submitted to cycles of funding: in Europe for example that corresponds to the different Framework Programmes for Research and Technological Development. In the case of European complexity, the golden age of project funding labelled "complex systems" was the decade going from 2004 to 2015, during which the Commission has supported the field with more than 100 million euros (e-mail interview with a British mathematician and evaluator of such projects, 23.03.18.).

Complexity sciences as a scientific platform

This section describes complexity sciences around three axes – epistemic, ontological and social. The first three subsections highlight, for each of these points, what is shared by the several subdisciplines at presence within the complexity label even before they decide to come together; they also show how these commonalities are strategically used by the scientists in order to make complexity exist and expand. The fourth subsection presents some of the main concepts to think about scientific communities (discipline, specialty, etc.) and points out their limits in giving account of complexity sciences. The last subsection describes this field as a *scientific platform* and indicates other examples which this concept may be applied to.

Epistemic axe

The epistemic elements that complexity sciences share are basically the study object of 'complex systems', the so-called "holistic" approach, a set of numerical inquiry tools and the epistemic project of formalizing all "soft" sciences (Li Vigni, 2020a).

It is notorious that biologists do not agree on the definition of life and that neither psychologists agree on that of intelligence. In the case of complexity sciences, the definition of the common object is left generic, vague and open from the outset, with the aim of letting virtually any discipline get in. While life scientists will put the accent on 'self-organization' and the 'evolutionary' aspects of their complex systems, physicists will mainly address 'phase transitions' and 'attractors', while geographers will focus on cities' 'trajectories' and 'bifurcations'.

Complexity "holistic" approach is intended to overcome the "analytical" one, which is seen as separating inseparable things. Holism is presented as the useful perspective to seize systems "emergent" properties. In such view, the microscopic level is too difficult to be studied in detail, and by the way useless, since what counts is what results from the interactions. The general conviction of complexity researchers is similar to that of deterministic chaos – a philosophy that Murray Gell-Mann has famously epitomized as follows: "Surface complexity arising out of deep simplicity" (Pines, 1988: 3).

Among the generally shared tool belt within complexity sciences, a dozen of mathematical, physical and computer methods appear to be the most recurrent[19]. Except Christopher Langton's agent-based model strain (Langton, 1997; Helmreich, 1998), all these tools have been conceived outside and before the SFI was founded. Complexity scientists have revised, appropriated, further developed and applied these tools in unusual ways. It is important to remark that these methods are ontologically flexible – almost all of them have, at one time or another, been applied to simulate any kind of system, from magnets to stock options, from forests to electors, from proteins to robots.

Interestingly, the holistic study is conducted through a series of tools that physicalize, mathematize and computerize the different kinds of complex systems – an operation that has sometimes encountered internal resistances (Jensen, 2018). Statistical physics and agent or network simulations – today the most spread tools of the complexity belt – are often philosophically based on methodological individualism (O'Sullivan and Haklay, 2000), but actually make sense on a meta-population viewpoint (Colizza and Vespignani, 2008). Complexity scientists indeed focus on "aggregates", "clusters" and "populations". The "individuals" simulated are the computational instantiation of a *class* of individuals. They are a form of statistical embodiment with a fictional singularity. Individuals' freedom of will and/or unpredictable variability are synthetically represented through the introduction of a certain degree of stochasticity. Agents are otherwise strictly submitted to a more or less small number of "rules", "laws", or "mechanisms" depending on the subdiscipline (Treuil et al., 2008).

How is all this used strategically? The vagueness of the term "complex systems" is one of the glues that keep this heterogeneous group together. It can either refer to a cell, an ant colony, a social network, or a financial market. At this intersection, the definition is not directly operational, because every member will mean very different things with the same term. The concept remains sufficiently general to justify the copresence of very diverse researchers in the same place (be it an institute, a research program, a workshop or other). The term is used in federative moments, such as the fund raisings and the outreach. Both at the SFI and in the French community, complexity scientists regularly meet in brainstorming workshops to collectively reflect on, and establish a common definition of complexity (Cowan et al., 1994; Bourgine et al., 2009; Bertin et al., 2011).

By sharing a definition, an approach and an epistemic project, complexity scientists let open the possibility to include into their field as many specialties as possible under the same mission and flag. Their theoretical discourse is presented as a revolutionary novelty in science: according to George Cowan, SFI's vocation was to produce a sort of "twenty-first century Renaissance man […] able to deal with the real messy world, which is not elegant, and which science doesn't really deal with" (SFI, 1988: 4). Complexity approach was also intended to conquer new territories of knowledge through numerical tools: "in recent decades the mathematics of chaos and the ubiquity of computers have produced a convergence of interests between the [social and natural sciences]" (Cowan, 2010: 131). Apparently the exchange between the "two cultures" is conceived symmetrically (Bourgine and Johnson, 2006: 6). In fact, the epistemic framework – strictly numerical – is charged to formalize "soft" sciences: "mathematics, computer science and statistical physics can bring new formalisms for representing complex systems dynamics in an elegant and useful way" (Bourgine and Johnson, 2006: 14).

These tools permit those who master them to either renovate an existing specialty (e.g. quantitative geography) or incept a new one within a given field (e.g. computational epidemiology). To give an example, quantitative geography appeared in the 1960s at the initiative of some Swedish, Anglo-American and French researchers (Berry and Pred, 1965; Robson, 1973; Cuyala, 2014; Varenne, 2017; Pumain, 2020). This subdiscipline of geography gets its main inspiration from physics and, in some cases, aspires to provide decision making support to private and public actors. Starting from the 1980s, this specialty has renovated itself drawing from complexity sciences. On the other side, computational epidemiology was founded in the 2000s by a small number of physicists experts of complex networks and statistical physics. In order to shape their expertise and better integrate the public health array of specialties, computational epidemiologists get inspiration from meteorology and aspire to build up national and international infrastructures for real time epidemic forecasting (Grüne-Yanoff, 2011; Moran et al., 2016; Opitz, 2017). These two domains share the fact of having a relatively marginal position within the larger disciplinary field they are embedded to. Complexity tools can be perceived differently depending on the discipline. In the case of quantitative geography, digital methods are criticized by qualitative geographers for being reductionist, theoretically useless, or ontologically empty (interview with a French quantitative geographer, 17.09.15). In the case of computational epidemiology, public health practitioners were initially reluctant in considering a group of statistical physicists with a computational talent as their peers. Gradually, the predictive success of their models and simulations, and their socialization with public health officers, have brought some of them to be acknowledged as part of the community (Li Vigni, 2021).

Ontological axe

Complexity scientists mostly share the same *mathesis universalis* view of nature (Israel, 2005). Ontology is the other important element that unites different subdisciplines within the same space. According to an important early member of the SFI, "A key property of complex adaptive systems is their ability to process information – to compute – in order to adapt and thrive in an environment" (SFI, 2014: 18). The European roadmap for complexity sciences claims something similar: "Many complex systems can in themselves be seen as implementing computational processes" (Bourgine and Johnson, 2006: 31). In their view, almost everything is a computational network and as such it can be studied; the opposite is also true: since many systems can be studied through network computations, these systems *are* computational networks:

> When you bring networks down to their minimal description and get them rid of the different disciplinary terminologies […] what we discover of, say, biological networks can be partly applied to sociology and computer science. (Bersini, 2005: XVIII-XIX, my translation).

Without a common interpretation of the organization which the different complex systems are made of, it would probably be difficult, for complexity scientists, to share the same inquiry tools. Moreover, the ontological argument can be used to support the epistemological one:

> [The simulation] is an abstraction of the form. If a real form exists, the form of the simulation is an abstraction of the real form. […] When [the simulation] works, it means that the phenomena that I have captured within it are effectively the real phenomena. (Interview with a French computational epidemiologist, 09.05.17).

From a strategic viewpoint, the computational, mathematical and/or physical view of natural and social systems is often opposed by complexity colleagues within their own individual subdisciplines. Nonetheless, this has not prevented the relative institutional success of some digital platforms that have been developed under their label. In the US, Christopher Langton's agent-based model platform called "SWARM" (SFI, 1998a: 19), as well as MIT computer scientist Mitchel Resnick's "Starlogo" (SFI, 1998b: 2), were both open source and have been utilized in several contexts for very different objectives – from optimizing agroindustrial companies production to school science education, from theoretical biology research to military planning at DARPA.

In France, the Parisian Complex Systems Institute has developed a platform which, through a workflow and the lending of computing time at a national or international grid, serves to test, challenge and statistically analyse the individual models of a heterogeneous community of modelers from different university and research departments within the country (Reuillon et al., 2013). The ontological commonality that allows physicists, ecologists, embryologists and social scientists to use the same codes and models, also allows the mutualization of digital platforms for their development and testing – ontology sharing permits economies of scale.

Social axe

Complexity sciences can be seen as a sort of confederation, where each 'nation' keeps its autonomy while associating with other autonomous 'nations'. The label provides an area of intellectual exchange, but also an intermittent alliance in order to reach common social and institutional objectives. Complexity specialists meet at a series of places, such as institutes, conferences, workshops, summer schools and scientific societies, where they can discuss, collaborate, trade and collectively conceive shared strategies in order to exist and expand, all together or individually and in parallel. An important device invented and used by the SFI to create interdisciplinary collaborations is what it calls the "integrative workshop". Halfway between a conference and a brainstorming, such device can last from one to two weeks, and gather two to three dozen participants. Each attendant is a speaker and contributes by presenting his or her contribution. In the following phase of synthesis, attendants propose possible bridges between the different contributions (SFI, 1990a: 10). Complexity institutes are generally conceived as visiting institutions to "legitimate this kind of interdisciplinarity, to give it the means to develop, to allow people to meet, to assert themselves and not to 'hide away'" (interview with a French computer scientist at the Parisian Complexity Institute, 23.03.16). Since the beginning, the SFI self-described "as a growing, extended family whose members stay in touch by phone and computer and who return frequently to sit around the table at [the institute]" (SFI, 1992: 28).

For complexity specialists, the domain launched by the SFI represents a stimulus or a pretext in order to challenge hegemonic approaches in their belonging fields. This is either to "revolutionize" or at least "innovate" a part of their discipline, where they can be minoritarian (which does not necessarily mean marginal and dominated: certain network specialists for example are central and dominant in physics and computer science). These scientists search for allies, inside and outside their own discipline, in order to legitimize and strengthen their scientific efforts. To give a representative example, such a strategic way of thinking is shared by the international members of the Network for Ecological Theory Integration (NETI) – a group of ecologists, mathematicians and physicists from the US, Europe, Australia and Chile, most of whom are SFI's members who periodically meet at integrative workshops and write common publications, to produce general mathematical theories for ecosystems. In their view – inspired from physics –, science has to produce not only local models, but also general theories – where theory is defined "as a hierarchical framework that contains clearly formulated postulates, based on a minimal set of assumptions, from which a set of predictions logically follows" (Marquet et al., 2014: 701).

Terms to conceptualize scientific domains

This section introduces the available concepts to give account of scientific groups on the disciplinary level. In the light of the plethora of texts about subdisciplines, disciplines, interdisciplinary, transdisciplinary fields, etc., it is impossible to provide an exhaustive review of the literature (cf. e.g. Sugimoto and Weingart, 2015; Klein, 2008). In the following, the concepts of 'discipline', 'specialty' or 'subdiscipline', 'interdiscipline', 'transdiscipline' and 'studies' are discussed and the reasons why they do not seem suited to make sense of complexity are given.

In a 2000 paper, French sociologist Gilles Klein realized an interesting review of the literature on the concept of discipline (Klein, 2000). According to him, philosophers, sociologists and historians of science contributions can be organised around three different foci: cognitive[20], institutional[21] and societal[22]. Furthermore, Klein highlights the fact that several authors have deconstructed the concept of discipline, by pinpointing that science is always evolving through competition and collaboration into endless ramifications[23]. These authors criticize the concept of discipline for being too static to describe ephemeral and plastic networks of researchers that reconfigure incessantly. The concept of "specialty" is sometimes invested to show that disciplines are conglomerates of subfields and that researchers work on similar problems with similar practices into local contexts (Favre, 1995; Zuckerman, 1988; Leclerc, 1989; Monneau and Lebaron, 2011). From this perspective, disciplines are associations of specialties, such as, say, biology which differentiates into genetics, microbiology, zoology, etc. Despite the constant ramifications of sciences, many authors still consider the discipline as a useful concept[24]. But if the constitutive elements of a discipline are a common standardized knowledge, a generalized pedagogical cursus at universities and the existence of institutional channels of professionalization, then complexity science is not a discipline. The frontiers of the latter are porous; educational curricula, stabilized handbooks and official professional devices lack. Indeed, while there is no doubt that complexity sciences agglomerate several specialties under their label, these are not coordinated under a homogeneous discipline. Complexity looks like an alliance of a set of subdisciplines which come from, and still operate within, separated disciplinary contexts. In this sense, they operate as a *crossroad* where statistical physicists, theoretical ecologists, computer scientists, quantitative geographers and others meet to share and pursue a common epistemic, ontological and social project.

The second concept to be addressed is less richly covered by the literature, but apparently very pertinent for our case here. 'Interdiscipline' is not to be confounded with the concept of 'interdisciplinarity', whose polysemy and ambiguity makes it impossible to offer a satisfying literature review here (cf. e.g. Klein, 2008, 2010; Porter and Rafols, 2009; Madsen, 2018). American sociologist Scott Frickel defines 'interdisciplines' as "hybridized knowledge fields that are constituted by intentionally porous organizational, epistemological, and political boundaries" (Frickel, 2004: 269; see also Friman, 2010). Frickel explains that interdisciplines are more epistemologically and organizationally variable and instable, less institutionally powerful, as well as more focused on problem solving than disciplines. In his case study – genetic toxicology – he shows that geneticists have retained control of the emergent field, and that the interdiscipline in question has reconfigured existing knowledge in established fields instead of producing entirely new knowledge. Some similarities between Frickel's case and complexity sciences do exist. Like genetic toxicology, the latter have porous boundaries; they are epistemologically and institutionally variable, weak and instable; they also have mainly focused on the reconfiguration of existing knowledge in established fields; finally they are characterized by the internal domination of two fields (namely physics and computer science) over the others (life and social sciences).

Yet, divergences between Frickel's definition and the reality of complexity are more substantial. First of all, the field launched by the SFI does not unite only two fields but many more. Complexity has been clearly conceived as an ecumenic alliance between very many different domains in order to renovate science in general. Second of all, despite the domination of physical and computational approaches over the other subdisciplines at presence, it must be noted that epistemic and institutional conflicts between complexity scientists are quite rare, essentially for two reasons. First, life and social scientists joining the field have an advanced knowledge of numerical tools or wish to gain it through their participation into an interdisciplinary endeavour like this. Second, it is common that complexity exponents, at least in the initial phase of their commitment into the field, suffer from a relative marginality within their own discipline, and have an interest in associating to other scientists in order to gain legitimacy and create the conditions of their existence and expansion[25]. Finally, even if some of the specialties which avail themselves in complexity are now frequently welcomed or even solicited by governmental, entrepreneurial and civil society instances, complexity sciences have been conceived and organized since the beginning as a theoretical domain, not as a problem-solving field like genetic toxicology.

Let us focus on the term 'transdisciplinarity' now. This is defined in different ways: *a*) the study or the action "on real world challenges in a mode of inquiry commonly referred to as problem solving"; *b*) "a practice of transgression that challenges existing institutional structures and disciplinary methods of research that are not apt to deal with complex real world problems"; *c*) "the quest for unity of knowledge by integration and synthesis using concepts of holism, systems thinking and deep structures" (Lawrence, 2015: 2; see also Alvargonzález, 2011 and Zierhofer and Burger, 2007). While the first two meanings imply the collaboration between scientists and extra-academic actors for the resolution of complex sociotechnical issues and parallel the concepts of 'Mode 2' (Gibbons et al., 1994) and of 'post-normal science' (Funtowicz and Ravetz, 1993), the third one corresponds to the epistemological project pursued by some thinkers (Morin, 1977, 1980; Nicolescu, 1997; Klein, 2004). In all these cases, the normativity of this term does not suit the descriptive goal of the present article.

Less common, the concept of 'transdiscipline' has not been rigorously thematized by sociologists of science, but circulates in certain streams of evaluation studies, informing science, engineering and psychology (Coryn and Hattie, 2006; Ertas, 2010; Cohen and Lloyd, 2014; Moir, 2015). In particular, Scriven (1991, 2003) considers it as a useful term to characterize logic, statistics, ethics, computer science, information science, evaluation studies, and other similar fields which are standalone disciplines, but are at the same time used as tool belts in several other disciplines. Scriven (2008: 65) distinguishes a second similar meaning of transdiscipline: "a theory, point of view, or perspective that has some application in several disciplines. This […] was applied by people in reference to both Marxism and feminism, since both points of view can affect one's stance in many traditional disciplines such as sociology, psychology, and economics". Either way, complexity sciences make use of three transdisciplines – i.e. mathematics, physics and computer science – but cannot be considered as a transdiscipline in themselves. Even if the current president of the SFI aims at fostering what he calls "transcience" (SFI, 2011: 2), the different subdisciplines at presence in complexity institutes and conferences remain anchored within their disciplinary fields.

Another term which deserves attention for its application to interdisciplinary domains is the concept of 'studies'. Such term has been increasingly used to name all sorts of pluri-disciplinary conglomerates that get together for the inquiry of the same theme. It is important to say that not all pluri-disciplinary and object-oriented fields are qualified as studies – a term particularly employed for social sciences. Fields such as nanotechnologies, biotechnologies, cognitive sciences, and complexity sciences are not called 'studies', even if they can contain social sciences. Yet, all these examples share the same characteristics of being pluralistic – since many disciplines, methodologies, paradigms, professional roles and institutional forms co-exist within them – and of having a common interest for the same phenomenon. The problems with this concept is that it is mostly used by the studies members themselves as a backup solution to qualify their association and that it remains weakly theorized by social scientists (Monteil and Romerio, 2017). While few scholars belonging to this or that field of studies aim at transforming it into a discipline, it is evident that in the vast majority of cases the disciplinary identity of their exponents stay strong. The term of studies can thus be seen as a synonym of 'interdisciplinary fields'. Yet, these domains have some recurrent cognitive and social characteristics that deserve to be isolated and highlighted. For example, as the readers of this journal know well, STS regroup basically all the humanities working on technoscience. They do it with very different, sometimes mutually exclusive approaches. Yet they fundamentally agree on a set of basic tenets (see below). Exploring complexity is useful to conceptualize this kind of interdisciplinary fields that couple a loose unity with an ineliminable heterogeneity.

Scientific platform, a general concept?

"To give a name to a scientific domain, to make it exist, and to align oneself with it, is not a neutral enterprise" (Popa, 2019: 114). Defining a field is at the same time an epistemic and a political act (Bourdieu, 1975). It implies the construction of boundaries, the designation of adversaries, the struggle for the legitimation of new institutions and for the creation of new professional roles and competences (Gieryn, 1983; Favre, 1983; Feuerhahn, 2013). But, while complexity scientists do create new boundaries and struggle for legitimation, their frontiers are more permeable than those of classical disciplines and specialties. Also, they fail to establish a certified professional category.

What is thus its *raison d'être*? The label can federate and reinforce individuals who are isolated and weakened in their respective domains. In this sense, complexity is not a 'field' in the Bourdieusian sense, since internal competition is dozed off and rather replaced by collaboration for reinforcing the individual struggles of participants against what they sometimes call "disciplinary inertia" or "institutional conservatism". To describe complexity sciences, I thus conceive and propose the concept of *scientific platform* as an articulated description of such multidimensional strategy. If the indigenous qualifications of 'sciences' and 'studies' do not suit for the description, it is because these terms tend to put the accent on their study object more than on their social and institutional strategies of existence. The term of *scientific platform* is intended to re-politicize the emergence of this interdisciplinary domain. As Casilli (2019) remarks, the term of platform was firstly used in the military and architectural fields, it then entered the political and theological spheres, and it recently became widely used to refer to economical actors such as Facebook or Uber, whose digital platforms connect people and make them function on a large geographical scale. Here the term is mainly used metaphorically with reference to its initially architectonical meaning. Similarly to what Popa (2019: 115) has remarked for the 'area studies', complexity sciences appear capable of "offering an intellectual and institutional 'flagship' and at the same time enough margins of manoeuvre to the actors that seize it". A certain "fragile coherence" (Schut and Delalandre, 2015: 84) can be observed in disciplines in general, but, in the case of complexity sciences, the weakness of the glue that keep them together can paradoxically represent a form of strength, for it permits to certain mavericks to have a social space instead of nothing. While often marginal or minoritarian in their disciplinary homes, the researchers that inscribe within this label seem to believe and realize the proverb "there is strength in unity". A platform as intended here is a meeting point where people ally temporarily to get back to their home with more strings to their bows. The term is a rich metaphor because of its polysemy. In train stations a platform is the raised structure from which passengers can enter or leave a wagon; in astronautics it is a structure which dispatches resources; in car industry it is a set of components shared by different vehicle models; in short, it generally refers to a common foundation. The complexity label and the concrete spaces it recovers permit to its heterogeneous members to mutualize resources and increase collective legitimacy. Complexity meeting spaces are indeed used by scientists as a trampoline to carry on different kinds of struggle in the academic field at large – e.g. competing for federal or international funding such as NSF scholarships or as European Commission research programmes –, and in the specific disciplinary fields where they are individually inscribed. Nonetheless, researchers' inscription in complexity comes – if at all – at the second, third or fourth place in their CVs and self-presentations. A French quantitative geographer testifies of this in a way which is representative of basically the totality of my interviewees: "I guess that [complexity] is a totem to make people working on very different topics gather together […] I don't feel more complexity scientist than geographer" (interview with a French quantitative geographer, 12.04.17). Yet, when the "complexity" etiquette is important to attract funds, it can be used in the first place, as the following quotation from the European roadmap illustrates:

> The new science of complex systems […] is part of every discipline. […] It will benefit industry, the public sector, and all social actors. Complex systems science will be the foundation of Europe's wealth and influence in the 21st century. (Bourgine and Johnson, 2006: 2).

Now, if we take other interdisciplinary domains, we may find the same strategic operations as those observed with complexity. For lack of space and in the absence of an ad-hoc empirical fieldwork in other fields, I will speculate on the possible generalization of such a term by taking the example of STS. While its study object is sciences and technologies, the disciplines at presence include virtually all social sciences. From an epistemic viewpoint, in STS – like in complexity sciences – a set of principles, inquiry methods and approaches are recurrent despite the intellectual pluralism of its scholars: for exemple, the role played by non-humans and the importance of empirical fieldwork as compared to classical philosophy of science. From an ontological viewpoint, several nuances exist but science and technology are generally seen as inseparable from the rest of society. The sphere of ideas is always described as embedded to material, sociocultural, economic and political ones. From an institutional viewpoint, few STS departments and degrees exist in the world, and there again the power of disciplines remain strong, albeit some researchers aspire to overcome them (Cozzens, 2001). Like complexity, STS community has not managed to create a professional autonomy: its students are hired, in academia or outside, for their sociological, anthropological, historical backgrounds. At the same time, STS, like complexity, struggle for legitimacy and, because unity is strength, they often manage to confer a better touch to social scientists who inscribe in them. In many cases, STS scholars remain minoritarian in their home disciplines and such label is for them a second skin, both inside the STS community and outside. Functioning as a platform, STS exist intermittently, because researchers can retract from it when felt appropriate. Ultimately, I guess that many "studies", as well as cognitive, Earth system and sustainability sciences – among others – can be apprehended as *scientific platforms*. Such fields benefit from different degrees of success (e.g. STS and cognitive sciences seem to be better implanted than complexity), but they all seem to have the same instable, intermittent and strategic existence that get them closer to confederations than to thoroughly new nations.

Conclusion

Complexity sciences appear at the same time as a compact and well identifiable but at the same time crumbly and floating domain. Scholars passing by it may have trouble in finding a job, which, within the professional autonomy frame, is the clearest example of why complexity is not a discipline. After the profusion of research projects launched by the European Commission between 2004 and 2015, and after the wave of complexity institutes foundations around the world in the first decade of this century, the push of this field seems to be slowing down. Such a fact – along with the others exposed here – seem to give reason to some of my most critical interviewees, and to certain observers who have defined complexity as a "fad" (Sardar and Ravetz, 1994). Yet, complexity has not disappeared: there is still a community which finds there a second identity. How then to explain the persistence of complexity sciences over the decades and its relative institutional instability?

This article has showed that complexity can be seen as a socio-epistemic space where scientists from different subdisciplines meet and collaborate intermittently to reach a series of common objectives (increasing legitimacy, exchanging knowledge, searching for funds, etc.), on the basis of the loose commonality of a series of discourses, practices and values. Complexity is a heterogenous and loose space, which – despite its fuzzy boundaries and institutional weakness – provides a discursive unity that can function as a strategical foothold. This allows the specialties at presence under its label achieve a series of theoretical, social and political objectives. Complexity can also be seen as a "conglomerate" more than a unique and coherent entity (Favre, 1983; Popa, 2019). Yet, this term is too static to give account of the existential processes that lean upon the common ground represented by the label. The aim of the present article was to propose a concept which be sufficiently large and descriptive so to grasp the dynamism of a social phenomenon, without normatively reifying its boundaries, strategies and intellectual contents. Interdisciplinary domains adopt different tactics according to their objectives and sociohistorical contexts. Those that work similarly to complexity sciences configure themselves as socio-epistemic spaces, whose unity is loose enough to embrace variable and pluralistic discourses and practices, with the aim of providing a temporary refuge or a perennial home to scientists who may be hardly classable. The concept of *scientific platform* may be useful to mean that complexity scientists find in their intermittent alliance the intellectual and institutional resources to return strengthened to their disciplinary fields, where they generally occupy a minoritarian position. Scientific platforms also provide theoretical, social and political support through which to carry existential or expansive efforts. In conclusion, whether the concept proposed here is pertinent to apprehend other similar interdisciplinary domains can only be answered through new empirical fieldworks.

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Notes

1 Like medicine, biology, sociology, economics, or political sciences (Urry, 2002; Foster, 2005; Martin and Sturmberg, 2009; Castellani and Hafferty, 2009; Geyer and Carney, 2015).

2 https://www.santafe.edu/.

3 https://iscpif.fr/.

4 The prestige of the Senior Fellows and the ambition of the institute's promissory discourse attracted more than one scientific journalist to tell the history of the fledgling 'complexity science' in a captivating way (Waldrop, 1992; Lewin, 1992; Kluger, 2008). Some of SFI's founders and first members also contributed to the fabrication and spread of this promotional narrative (Kauffman, 1993; Casti, 1994; Goodwin, 1994; Gell-Mann, 1994; Holland, 1996). Besides books, the research centre has always given much attention to media in general, because of their cascade effects on funding, members enrolment and credibility (e.g. SFI, 2006: 2; SFI, 2007a: 0).

5 In 1990, some of its members launched a scientific journal called *Complexity* through John Wiley (SFI, 1990a). The journal lacked of success because, as several interviewees explained, they prefer to publish in traditional specialized journals with higher impact factors (see also Williams, 2012: 171). Another kind of publication had more success. For the first fifteen to twenty years, the institute published a book series in joint venture with Addison-Wesley first and the Oxford University Press later on (SFI, 1987; SFI, 1998a). Some of the most sold titles were the proceedings of the Complex Systems Summer Schools (CSSS) – another important strategic device to establish the field started in 1988 (SFI, 1988). From the start, the institute attributed to this educational device an important place – first to produce new complexity adepts in the US and around the world, and second to fix the international standards of complexity science tools (SFI, 1991: 14). These have varied through time, but a certain number of them are now considered as paradigmatic. At the beginning of 2000s, the institute exported its summer school to other countries in the world, with the aim of extending its influence abroad (SFI, 2000a, 2001, 2005, 2008). Several summer and winter schools were organized in Eastern Europe, Asia, and South America, which indirectly led to the founding of new complexity institutes in these countries.

6 In 2004, a small group of scientific entrepreneurs – essentially polytechnicians and physicists, with the support of two scientific program officers from the European Commission in Brussels – organized in Turin the first European Conference on Complex Systems, which triggered the foundation of the

European Complex Systems Society (https://cssociety.org/events/15). The conference was the first of a long series and was financed, along with several international research projects, by different European programs. As one of the interviewees explains, the conferences were "a powerful instrument which became a place for visibility, a place for real discussion, a place for lobbying", capable of creating "a public notion of group identity" (interview with an Italian physicist and data scientist, 17.02.17).

7 http://blog.stephenwolfram.com/2012/05/its-been-10-years-whats-happened-with-a-new-kind-of-science/.

8 One can think of the Interdisciplinary Center for Nonlinear Phenomena and Complex Systems founded in Brussels by physicist Grégoire Nicolis around the figure of Ilya Prigogine in 1991 (http://cvchercheurs.ulb.ac.be/Site/unite/ULB164UK.php), or of the defunct Centre de Recherche en Épistémologie Appliquée founded in 1982 at the French École Polytechnique by philosophers Jean-Pierre Dupuy and Jean-Marie Domenach (Lavallée, 1992).

9 It is for example the case of physicist Yaneer Bar-Yam's private centre called the New England Complex Systems Institute, based in Cambridge (MA) and founded in 1996 (http://necsi.edu), and that of physicist Ricard Solé's Complex Systems Lab, based in Barcelona (Spain) and founded in 1998 (http://complex.upf.edu).

10 Some examples of this type are Paris and Lyon Complex Systems Institutes, launched in 2005 by French polytechnicians Paul Bourgine and by French physicist Michel Morvan, as well as the Institute for Scientific Interchange of Turin (Italy) which has a much longer history and which specialized in complexity since the beginning of the 2000s.

11 The Center for Complex Systems and Dynamics, affiliated to the Illinois Institute of Technology in Chicago (https://web.iit.edu/ccsd), belongs to this typology. It was founded in 2003 under the impetus of two chemical engineers – Fouad Teymour and Ali Cinar – who conduct agent-based modelling to simulate biochemical and chemical-physical processes in collaboration with laboratory and industrial experimenters of the IIT.

12 It is for example the case of the Complex Systems Department of the Computer Science Laboratory at Pierre-et-Marie-Curie University in Paris (https://www.lip6.fr/recherche/team.php?acronyme=SysComp), as well as of the Namur Institute for Complex Systems at the University of Namur (Belgium) (http://www.naxys.be).

13 One can think of the Max Planck Institute for the Physics of Complex Systems in Dresden (Germany) (https://www.pks.mpg.de/institute/), and the Matter and Complex Systems Laboratory at the Diderot University in Paris (http://www.msc.univ-paris-diderot.fr).

14 https://www.phy.ncu.edu.tw/~ccs/research.html; http://english.ia.cas.cn/rd/200908/t20090807_27605.html; http://www.accs.uq.edu.au/index.html.

15 https://www.mq.edu.au/research/research-centres-groups-and-facilities/healthy-people/centres/australian-institute-of-health-innovation/Research-Streams/Complex-systems.

16 https://gradschool.duke.edu/academics/programs-degrees/non-linear-and-complex-systems.

17 The Open University in Milton Keynes (UK) offers one, with a focus on design and engineering (http://www.open.ac.uk/postgraduate/research-degrees/topic/complexity-and-design); the Vermont Complex Systems Center at the University of Vermont (USA) proposes another one with a focus on data science (https://vermontcomplexsystems.org/education/phd/); only the Department of Information Science and Technology at the University Institute of Lisbon seems to offer a program which resumes the main SFI's theories and tools (http://complexsystemsstudies.eu/?page_id=140).

18 For example, the international master in Physics of Complex Systems – jointly operated by three French universities and three Italian ones – is mainly focused on statistical physics and network theory (https://physics-complex-systems.fr/en/). The same is true for the Master in Complex Systems held by the École

Normale Supérieure in Lyon (France) (http://www.ixxi.fr/enseignement/master_systemes_complexes). The Master in Complex Systems Modelling at the King's College in London (UK) has a broader array of applicative fields – mathematical biology, nanotechnologies, financial markets, machine learning, etc. –, but remains focused on network theory (https://www.kcl.ac.uk/study/postgraduate/taught-courses/complex-systems-modelling-msc). The Master of Complex Systems at the University of Sidney teaches several computational techniques focusing around three majors – biosecurity, engineering and transport (https://sydney.edu.au/courses/courses/pc/master-of-complex-systems.html). The same is true for the Master held by the Centre for Complexity Science at the University of Warwick (UK) (https://warwick.ac.uk/fac/cross_fac/complexity/study/msc_and_phd/#phdprojects).

19 1. Dynamical systems, fractals and chaos; 2. Cellular automata; 3. Statistical physics; 4. Spin glasses; 5. Neuronal networks; 6. Genetic networks; 7. Network theory; 8. Graph theory; 9. Agent-based models; 10. Self-organized criticality; 10. Genetic algorithms; 11. Machine learning; 12. Statistical tools for Big Data. This list has been built using different sources, such as some scientometric and qualitative works done by complexity scientists themselves (Cointet and Chavalarias, 2008; Grauwin et al., 2012; Deffuant et al., 2015), complex systems summer schools, research projects, conferences and interviews with practitioners.

20 Some authors see the discipline as a logical space of construction of arguments which has an internal coherence and cohesion that excludes the researchers who do not share the same assumptions (Kuhn, 1962, 1977; Lakatos, 1970, 1978; Mullins, 1972; Mulkay and Edge, 1973; Law, 1976; Gilbert, 1976; Laudan, 1977; Berthelot, 1996; Galison, 1997; Bird, 2001).

21 For another group of authors, a discipline is characterised by the stabilization of a set of theories, practices and communities through their institutionalization in the form of university teachings and professionalization, scientific societies and journals, laboratories, certification procedures, etc. (Crane, 1967; Merton, 1973; Bourdieu, 1975; Long et al., 1979; Price, 1986; Ben-David, 1991; Cole, 1992; Dubois, 2014; Gingras, 1991; Schut and Delalandre, 2015).

22 Another group of authors focus on the societal control over disciplines which are seen as responding to social, economic and political interests (Foucault, 1969, 1980; Habermas, 1973, 1976; Van den Daele and Weingart, 1976; Krohn and Schäfer, 1976; Desrosières, 1998; Van Lente and Rip, 1998; Borup et al., 2006; Heilbron, 2004; Aguiton, 2018; Raimbault, 2018).

23 Such ramifications occur as a consequence of specialisation and interdisciplinarity (Holton, 1972; de Certaines, 1976; Gieryn, 1978; Collins and Restivo, 1983; Barnes and MacKenzie, 1979; Knorr-Cetina, 1982; Gibbons et al., 1994; Weingart and Stehr, 2000; Barry and Born, 2013; Grossetti, 2017)

24 They underly for example the fact that interdisciplinary collaborations can give rise to new specialties; that scientists struggle for the acquisition of the specific capital of a disciplinary "field"; and that the educational and recruiting institutional processes stabilize and perpetuate the traditional big bodies of knowledge (Cambrosio and Keating, 1983; Lenoir, 1997; Gingras, 1991; Fabiani, 2006; Bulpin and Molyneux-Hodgson, 2013). The definition of a new field is indeed the terrain of power conflict, because of its performative effects on intellectual and social boundaries, grant obtaining, institution building, recruitment, etc. (Gieryn, 1983; Klein, 1996; Small, 1999; Borup et al., 2006; Owens et al., 2006; Miller and O'Leary, 2007; Laurent, 2010).

25 Think for example of Stuart Kauffman in biology, Christopher Langton in computer science or Brian Arthur in economics.

Interdisciplinary Projects as an Expert-Network: Analysing Team Work Across Biological and Physical Sciences

Neil Stephens
Social and Political Sciences, Brunel University London, United Kingdom/ Neil.Stephens@Brunel.ac.uk

Phil Stephens
School of Dentistry, Cardiff University, United Kingdom

Abstract

We report an analysis of how an interdisciplinary project bringing together biologists, physicists and engineers worked in practice. The authorship team are the Principle Investigator who led the project, and a social scientist who studied the project as it was conducted by interviewing participants and observing practice. We argue it is accurate and productive to think of the interdisciplinary team as an Expert-Network, which means it was a managed set of relationships between disciplinary groups punctuated by specific junctions at which interdisciplinary exchange of materials, knowledge, and in limited cases, practices, occurred. We stress the role of trust in knowledge exchange, and document how hard sharing knowledge – and especially tacit knowledge - between disciplines can be. Key is the flexible management of the network, as the membership and required skill set change. Our analysis is embedded within, and contributes to, the Sociology of Experience and Expertise (SEE) framework. We close by suggesting advice for others seeking to manage a similar interdisciplinary Expert-Network.

Keywords: Interdisciplinarity, Interdisciplinary, Expert-Network, Biology, Physical sciences

Interdisciplinary work is an increasingly visible feature of science. This given, what it actually means has long remained ambiguous or contested both among those who engage in practices under its name, and scholars who analyse its use in practice (Dogan and Pahre, 1990; Jacobs, 2014; Klein, 2009; Callard and Fitzgerald, 2015; Madsen, 2018). In this paper we discuss and analyse practical issues in delivering projects constructed as interdisciplinary, through a case-study analysis of a particular consortium addressing issues of accurately tracking cell lineages, employing experts from a set of physical and life sciences. This paper is, itself, interdisciplinary, being co-authored by the cell biologist who was Principle Investigator on the grant, and a sociologist who collected data and analysed the progress of the project. As part of this sociological work, a set of interviews and observations were conducted with consortium members across the four-year lifespan of the project to understand the opportunities, challenges and broader experiences of working together. This work is reported here.

Empirical context: The consortium

This consortium was assembled following a funding call from the UK Engineering and Physical Sciences Research Council (EPSRC) to stimulate new research into the area of Novel Technologies for Stem Cell Science. The call was explicitly designed to support interdisciplinary work, mentioning both cross-disciplinary and multidisciplinary approaches in the proposal information. The consortium members themselves were drawn together through an existing infrastructure within the lead institution that had also been developed to support interdisciplinary engagements.

In conducting the project, the consortium's main aim was to develop a set of technologies to help cell biologists track cells and their differentiation state inside the body without opening the body up. The underlying principle of the application was the hypothesis that such research would profit from an interdisciplinary approach, drawing together areas of complementary expertise across the life and physical sciences. The team assembled was cross-School and cross-University, with most members in City 1 and those specialising in rheology in City 2, a similar sized city 55km away.

The project was concerned with developing solutions to overcome a major barrier impeding the translation of stem cell science: the inability to accurately follow cell lineages (the pathways along which stem cells move to become the end-stage cells of our bodies) and also to track them deep inside tissues in a non-destructive way. Specifically, the aim was to develop novel ways of non-destructively labelling stem cells by manipulating molecules within the cells so the consortium can follow both their position and their eventual fate. In order to image the cells, the consortium aimed to develop new microscopic techniques that allow researchers to view these cells in a non-invasive, non-harmful way (unlike prior approaches) and therefore utilise technologies that will eventually enable imaging of these cells deep within patient tissues. Being able to follow these stem cells would also allow the consortium to examine the mechanical influence of surrounding tissue environments. Armed with such knowledge the consortium could then mechanically manipulate the surrounding environment to direct stem cells into a tissue of choice in order to deliver custom designed tissues on demand.

The cell biologists produced two types of cells for the other consortium members to use in developing their novel technologies: neurons and adipose (fat) cells. The cell tracking techniques being developed were the microbiology-led approach of Chemical Exchange Saturation Transfer (CEST) and the chemistry-led approach of non-natural amino acids. The planned visualisation techniques were the physics-led Coherent Anti-Stokes Raman Shift (CARS) and Magnetic Resonance Imaging (MRI). The consortium also included the engineer-led rheological work that would measure the stress and strain readings of cells in a machine called a rheometer. The eventual aim of all of these processes was to provide cell biologists with better tools for observing and controlling cells inside the body.

The project was structured around three work packages across which five 'Post-Doctoral Research Assistants' (PDRAs; 3x biologists, 1x physicist, 1x engineer) worked in collaboration. Work package 1 collated the work on non-destructive stem cell imaging, including both the CARS and MRI/CEST research. Work package 2 contained the rheological work on the microstructural studies of cell differentiation. Work package 3 aimed to extend the rheological work to 3D tissues. Across the consortium project meetings were held every three months to discuss progress and consider next steps according to a project delivery schedule and defined milestones.

In what follows we analyse the work of this consortium over a four-year period through our novel concept the expert-network. First, we review a subset of the existing literature on interdisciplinarity so we can subsequently show how the expert-network concept contributes.

Interdisciplinarity

A large and diverse literature exists considering interdisciplinarity. This includes work from Science and Technology Studies (Nowotny et al., 2001), cognitive science (Bruun and Sierla, 2008), science policy (NAS 2005), scientometrics (Tomov and Mutafov, 1996), philosophy of science (Andersen and Wagenknecht, 2013), the history of science (Graff, 2016), as well as practitioner accounts (Newell et al., 2008). One strand of this work seeks to develop a definition of what counts as interdisciplinarity. Porter et al. (2004), for instance, argue interdisciplinary work involves research by teams that integrate perspectives and concepts, and/or tools and techniques, and/or information and data from two or more sites of knowledge or practice. Parts of the literature set interdisciplinarity alongside similar categories of practice. In this vein, Fiore (2008) reviews interdisciplinary policy literature to identify three existing categories: 'cross-disciplinary' (different disciplines without qualifying the type of interaction), 'multidisciplinary' (coordination of efforts for a common goal), 'interdisciplinary' (systematic integration of ideas) work. Similarly, Nersessian and Newstetter (2014) discuss engineering examples of a multidiscipline, interdiscipline, and transdiscipline (work that transcends discipline through synthesis). While these authors seek generalizable definitions of interdisciplinarity, others suggest that what counts as interdisciplinarity can vary depending upon the disciplinary context of each case (Riesch et al., 2018). Others again choose not to seek closely delineated definitions, instead simply using the term interdisciplinarity to capture all work between people with different expertises (Barry et al., 2008; Jacobs and Frickel 2009). A full review is beyond the scope of this paper, but we direct readers towards Huutoniemi et al. (2010), Klein (1990) or Frickel, Albert, and Prainsack (2016) as valuable resources.

An important theme in the literature is the observation that interdisciplinarity can take different forms. Bruun and Sierla (2008) identify three knowledge networking strategies – modular, integral and translational. *Modular knowledge networking* captures practices in which tasks are divided between autonomously operating agents with a single coordinating site that draws them together. *Integral knowledge networking* describes settings in which a group jointly and holistically addresses a task collectively. *Translational knowledge networking* combines elements of both, as autonomous groups focus upon tasks that have been allotted to them, but then come together to synthesise findings without a central coordinator. Articulating a different typology, Andersen and Wagenknecht (2013) develop the work of Rossini and Porter (1979) with four categories of interdisciplinary work. The first, *integration by leader*, has commonality with Bruun and Sierla's modular knowledge networking, in that a group leader is key to drawing tasks together. The second, *common group learning*, describes a situation in which the research process is characterised by sharing, interlocking intensions and mutual responsiveness which ideally leads to shared mental models and concepts. The third, *negotiation among experts*, involves a shared intention, but less integration with no commitment to genuinely shared final analysis. Andersen and Wagenknecht's (2013) fourth and final category is *joint integration*, which involves continuous integration of intentions and ways of working towards joint results, in a form akin to Nersessian and Newstetter's transdiscipline.

Bruun and Sierla (2008), and Andersen and Wagenknecht (2013), define generic categories of interdisciplinarity. MacLeod and Nagatsu (2018), in contrast, report discipline specific modes of interdisciplinarity. Their study focuses upon environmental sciences to argue that within this discipline interdisciplinary practices have crystallised around four principal integrative methodological platforms that site manageable modes of working across specialisms and allowing interdisciplinary affordances. As discipline distinct cases, these four strategies are all specific to the forms of modelling work undertaken in environmental science. Their relevance here is as an example of cases where interdisciplinarity in practice is limited by practical social issues, a finding also seen in climate science by Duarte (2017).

While some literature articulates the benefits of interdisciplinarity (Nissani, 1997; NAS, 2005), we should also note the minority of texts that explore its problems, weaknesses, and the negative aspects of the political economy of the drive to interdisciplinary work. Jacobs and Frickel (2009) question the soundness of the move towards interdisciplinary research, particularly at the risk to existing and successful disciplinary knowledge, and remain sceptical that interdisciplinary work really does deliver privileged knowledge. Callard and Fitzgerald (2016) explore the role of power asymmetries of real-world interdisciplinary practice, and the emotive effects of these. This theme also features in Albert, Paradis, and Kuper's (2016) study of humanities scholars in medical schools, and Stephens, Khan, and Errington's (2018) analysis of sharing and surveillance among interdisciplinary teams in the life sciences. Barry, Born, and Weszkalnys (2008) embed a recognition of power dynamics within their three-part categorisation of interdisciplinarity, that features the *integrative-synthesis* mode (where multiple disciplines work together), the *agonistic-antagonistic* mode (where intellectual opposition frame exchange) and finally the *subordination-service* mode, where one discipline asserts authority over the other (see Lewis, Bartlett, and Atkinson (2016) for a recent example of the relationship between biology and subordinate bioinformatics). Further engaging with the political economy of interdisciplinarity, both Mody (2016) and Cassidy (2016) detail how institutions use interdisciplinary practice strategically to attract funding (work complemented by Lyall et al.'s (2013) analysis of the role of funding bodies in bringing interdisciplinarity into being). Finally, as Cuevas-Garcia (2018) shows, those conducting interdisciplinary work can also construct those practices as both positive and negative.

Akin to the work presented here, there are examples of studies of interdisciplinary collaboration in the life sciences (Fujimura, 1987; Parker, Vermeulen, and Penders, 2012), with two pertinent examples of stem cell science and tissue engineering consortia (Morrison, 2017; Osbeck and Nersessian, 2010). In the most recent of these, Morrison (2017) reports interviews within a large cross-sector group seeking to produce 1,500 disease-specific induced pluripotency stem cell lines for toxicology testing. Morrison shows his interviewees articulate an ethos of reciprocity set within trust relations across a division of labour. In the context of this large consortium, some collaborative efforts were deemed 'formal', in that their nature and extent were defined in legal documents, while others took on an 'informal' character, which was premised upon different types of trust relationship. Importantly, and similar to the work reported here, Morrison notes the forms of exchange in this setting include the movement of material and data, as well as expertise. This theme is also analysed by Osbeck and Nersessian (2010) in their five-year ethnographic study including a tissue engineering laboratory. Their focus is upon discursive strategies scientists use to position themselves within interdisciplinary groups, related to professional or disciplinary affiliation, knowledge construction, and their relationships to objects and artefacts, particularly the cells themselves. On this last point in particular, Osbeck and Nersessian (2010) argue the scientists' disciplinary identification is related to both their identity as caretakers of the living - and often anthropomorphised - cells, and the relationship of that to the cells' agency as living entities. These scientists' skill, concern, and relationality to the cells as living beings are entwined with disciplinary identities. The themes from both Morrison's (2017) and Osbeck and Nersessian's (2010) work on the role of the material, trust, and identity in interdisciplinary tissue engineering-focused projects will feature in our account of our consortium as an expert-network.

Theoretical perspective: the Expert-Network

The work presented in this paper is informed by Science and Technology Studies, in particular the Sociology of Experience and Expertise (SEE) framework (Collins and Evans, 2002, 2007). We integrate our explanation of this approach into explication of our own novel theoretical contribution, the notion of the 'expert-network'. The SEE perspective was adopted early in the research process of the project described here, and N. Stephens discussed concepts from SEE with consortium members, including P. Stephens, as the project was conducted. Specifically, the notions of tacit knowledge, and contributory and interactional expertise were discussed with consortium members as the project progressed, with a view to developing a reflexive analysis by some consortium members during the work. The sociological component of the project was written into the original research proposal, to support thinking around interdisciplinary practice. This given, the key original theoretical contributions of this paper, discussed below but categorised under the term expert-network, was developed during analysis after the project was completed. In this work we do not seek to define what interdiscipinarity is, or divide it into subcategories, but instead we study how interdisciplinarity is enacted as a practice by those engaged in its pursuit.

Our key argument is that it is productive and accurate to describe the consortium's operation as an expert-network. By this we mean it comprises a managed set of relationships between disciplinary groups punctuated by specific junctions at which interdisciplinary exchange of materials, knowledge, and in limited cases, practices, occurred. We call these junctions 'disciplinary exchange points' to denote where, when, and how interdisciplinary exchange happened. Through these, the expert-network functions as a form of 'collaborative' or 'collective' interdisciplinarity, in which individuals from different disciplines seek to work together on a project, as opposed to a form of 'individual' interdisciplinarity, in which one person themselves seeks to become expert in multiple domains (Calvert, 2010; Lewis and Bartlett, 2013). Identifying key exchanges of the three types – material, knowledge and practice – is an important step in an expert-network analysis. An important insight from this perspective is that interdisciplinary work can function as much to re-establish disciplinary boundaries as to blur or break them (see also Centellas, Smardon and Fifield (2014) for a similar argument in the field of cancer biology). In an expert-network, scientists retain their status as experts within the disciplinary scope of their own area. Across the network, scientists seek to learn more about the work of other experts through an interest driven by a combination of pleasurable curiosity, trust and bond-building through attentiveness, and a utilitarian requirement to understand each other's work to allow the project to progress. Importantly, this utilitarian interest is informed by a concern over 'how much do I need to know about their work to do my work', or, in some cases, 'when do I know enough to know I can stop learning more about their work'.

In the SEE framework, Collins and Evans (2007) make the distinction between 'contributory expertise' – the full capacity to *do* the work of a scientific discipline (conduct and publish research as a contributor) – and 'interactional expertise' – the ability to communicate in some kind of meaningful way on the topic (but not being able to *do* the work directly) (see also Collins and Evans, 2015, and for use of this concept in other work on interdisciplinarity, see Gorman and Spohrer, 2010, Nersessian and Newstetter, 2014, and Andersen and Wagenknecht, 2013). Expert-networks such as those studied here contain scientists who are contributory experts in their own field, and who are, or are working to become, interactional experts in the fields of their consortia members. For example, as part of work package two, the cell biologists and physicists worked together to develop a functional CARS image analysis system for biological systems that produces non-invasive cell imagining. This could not be achieved without the contributory expertise in cell biology and spectroscopy, and a level of interactional expertise between the two. The disciplinary exchange points here related to knowledge and material, as the cell biologists needed to provide the physicists with (i) cells they could image, (ii) the knowledge to accurately write about this in publication, and, importantly, (iii) a clear sense of what was important and useful for a cell biologist to be able to see in the images produced. Whilst these requirements involved attaining a level of interactional expertise, at no point did the cell biologists engage in building or altering the CARS microscope, just as no physicist worked to culture cells. Both remained within the disciplinary boundaries of their contributory expertise and as such worked to reinforce these boundaries even through this expert-network.

This work of doing and engaging in contributory expertise involves learning some 'tacit knowledge' (Collins and Evans, 2007; Collins, 2010a, 2010b) of the practical craft skill of cell culturing. Knowledge that is tacit cannot be easily articulated in words either because it is an embodied skill, or because those who have knowledge do not recognise the importance of a particular part of their practice. Cell culturing craft skill is an example of specialist tacit knowledge – that of any expert in any domain (scientific or not), and is an essential component of both contributory and interactional expertise that, according to the SEE framework, can only be gained through immersion among those active in that domain. In the empirical sections that follow we will show the role and the challenge of tacit knowledge within the expert-network.

Finally, the SEE framework (Collins and Evans, 2007; Collins et al., 2007) has also drawn upon Galison's (1996) analysis of communication between people who do not share a language and specifically his theorisation of the formation of new languages through jargons, pidgins, and creoles, with each being an example of increasingly complex inter-languages that groups, including scientists, use to exchange ideas. As we will show, the interlanguage developed by the consortium is limited, as it is based upon experts accepting more simplistic terms and characterisations of their ideas as opposed to the development of a novel set of terminologies. The relevance of SEE to studying interdisciplinarity has been noted by Gorman (2002) and Goddiksen (2014). By using this set of ideas in a detailed case-study analysis of interdisciplinary work we believe the notion of the expert-network offers a productive mechanism to orientate the SEE framework towards these ends. In the discussions section, we identify key aspects of this approach that subsequent researchers may also choose to follow in their own work.

Methods

The analysis reported here explores empirically the practical experience of interdisciplinarity across the consortium in a detailed case-study approach. Twenty-nine semi-structured interviews were conducted by N. Stephens over a four-year period with team members from across the range of expertise. Interviews lasted between one and three hours and were recorded and transcribed. Interviewees were asked about the challenges and successes of working in an interdisciplinary context. Ethnographic observations were also conducted and recorded in fieldnotes by N. Stephens at the three-monthly project meetings and during laboratory visits over the four-year period. These day-long three-monthly project meetings in particular were key moments for data collection as the team communicated their progress and negotiated challenges in conducting interdisciplinary work (Stephens and Lewis, 2017).

The project was approved by Cardiff University School of Social Sciences research ethics committee. As part of this, ethical assurances were made to participants that they would be given personal anonymity, so the detailed accounts presented here do not identify the individuals involved. Quoted interviews have been edited for clarity and to retain anonymity. All team members were observed and approached for an interview by email, with seventeen people interviewed, eight more than once, and two members not agreeing to be interviewed for undisclosed reasons.

Interviews and observations were analysed through a thematic analysis by N. Stephens. As PI on the consortium project, it would be inappropriate for P. Stephens to see the data, so all data work was conducted exclusively by N. Stephens to protect the other participants' anonymity. Furthermore, P. Stephens is both participant (as both an interviewee and as a subject of observation) and author. As such, he is represented here in quotations, and as a contributing perspective on the analysis. By remaining reflexively aware of this relationship we believe we have retained the essential ethical guarantees to other participants, and provided a rich analysis to inform both social scientists interested in interdisciplinary work, and natural scientists seeking to be better informed about how they can approach their own interdisciplinary work. As part of this process, as noted earlier, P. Stephens, along with some other members of the consortium, discussed elements of the SEE framework with N. Stephens as the project progressed. These discussions informed the theoretical development as N. Stephens could see which elements of SEE resonated with participants' experience, and used this to formulate the notion of an expert-network. In dialogue with N. Stephens, P. Stephens then led on developing the advice for practitioners of interdisciplinarity that closes this paper.

The stem cell consortium as expert-network

In the following sections, we analyse our empirical material to further develop and substantiate our use of the expert-network concept. We will show the value of studying the interplay of material, knowledge and practice at disciplinary exchange points. We consider three related themes in turn: language and understanding, contributory expertise and tacit knowledge, and managing and reconfiguring the expert-network.

Language and understanding in the expert-network

The interactional form of this expert–network is evident in the following interview quotations. A physicist interviewed early in the project describes the difficulty of understanding the technical practices of others, and how the division of labour across the consortium allowed this to be manageable:

> "I'm quite confident on what I'm doing in my technical part… but I'm quite lost in the whole picture of the consortium in terms of what is interesting to measure… all this biology part is really something that I'm quite lost on."
>
> Interviewer: "Do you feel that's a problem?"
>
> "Hmm, I would say it would be better if I could understand it, but probably as *I don't need other people in the consortium to do my job*, probably other people in the consortium *don't need me to do their job*." [Emphasis added]

This clear identification of disciplinary roles and associated actions were key to how consortium members self-identified and located themselves in relation to others. Speaking towards the end of the project, an engineer explained how a level of knowledge exchange had occurred, but also reiterated the previous physicist's focus upon a division of labour across the expert-network:

> It's good to have an interest and appreciation of a lot of these different techniques, especially when you come to reviewing papers etc. But I would never want to become an expert at that kind of thing. *I think you need to focus on what you know already*. [Emphasis added]

Both these accounts, from a physicist early in the project, and an engineer late in the project, demonstrate how individuals in the expert-network use the network itself to assert the boundaries of their own disciplinary identities and practices, in terms of 'doing their job', or focusing upon what they 'already know'. In contrast to the fluid interdisciplinary identities reported in Brew (2008), the experience of collaboration here worked to further embed existing roles as the expert-network defines and delineates their expertise, not blur it into other domains. This given, a number of consortium members did have previous experience of other members' expertise, via previous projects or teaching together on University courses, but they retained a sense of, as one interviewee described, a "fundamental home" discipline.

During the three-monthly project meetings, a local and situated interlanguage arose as experts in different disciplines formed a basic shared vocabulary to explain their thinking to each other (Stephens and Lewis, 2017). Key to doing this successfully was knowing both what needed to be known by others and what did not need to be known by others. In a clear example of this, the presentations by the physicists at the three-monthly meetings in the early part of the project provided detailed accounts of the mathematics of spectroscopy and the computer algorithms used by the CARS system. Over time the presenting physicist chose to include less of this detail because, as interviews revealed, the physicist felt the broader consortium did not need or want to grapple with this discipline specific technical information (see also Stephens et al., 2018). By this stage, across the group, a shared understanding had arisen as to how much consortia members needed to know of the physics, and, equally importantly, that the group trusted the expertise of the physicists to continue appropriately. The physicists were themselves learning how much they needed to know of the expertise of others, and making judgements as to why, as articulated by another physicist in an interview half way through the project:

> Of course, it is frustrating if you don't fully understand, so where is the balance between how much I really need to understand that aspects of biology in detail, how much I can rely on what someone tells me, how much someone needs to know about how I do CARS microscropy. Someone doesn't have to know all the details but it's enough if they understand which kind of images we can generate and I think this process is very much also

> depending on people. So… having them explain enough so that *I have enough of an understanding but also knowing that, okay, at some point I don't need to know all these details*, but I have enough understanding to say, "I think this is something we can do together. This is an interesting problem; let's go in this direction". [Emphasis added]

Here judgements are being made as to what constitutes 'enough' knowledge for an individual to have about others, and what others need to know about them. Ascribing 'enough-ness' is linked to practical issues of establishing shared visions and expectations to allow each to do what they need to do and achieve what they need to achieve within the limits of their disciplinary interest. When communicating outwards across disciplinary boundaries, the focus is on simplicity to facilitate progress. A similar account was provided by an engineer below, that, like the example above, shows the inter-language of the consortium was premised upon contributory experts accepting a loss of accuracy and nuance in the terms used by others to explain their work:

> That has been a challenge, converting the general kind of language into correct rheological language. But I'm used to that, I work with clinicians (laugh), people call things sticky just because it's thicker, more viscous. Viscoelasticity isn't stickiness at all *but you kind of forgive them because you know what they are on about*. [Emphasis added]

Forgiveness here recognises the inherent limitations and challenges of disciplinary ties, an appreciation of the need for a shared resolution to the situation, and a willingness to forego the level of accuracy normal within their own discipline in order to pursue practical solutions. This given, while the knowledge-focused disciplinary exchange points were typified by simplified understandings, there were still levels of differentiation within the consortium, as interviewees often articulated which expertise domains they needed to understand better, based upon those disciplinary exchange points within the project, as evident in this cell biologist's account:

> I probably need to understand what [the chemist] is doing more than I need to understand the rheology or the CARS. The rheology and the CARS is more technical, the constructs that [the chemist] is going to provide are actually going into the cells so I do have to understand that bit.

Equally, respondents had a view on which other expertises they were best placed to understand, based upon a sense of which disciplines are closer to their own disciplinary identity and contributory expertise, as articulated by an engineer:

> Being an engineer, I like to understand the physics to a certain extent. Biology is a different language again.

The three-monthly meetings continued to be sites for disciplinary exchange points on the knowledge of each other's practice, although the inter-language that arose was limited to the core ideas that each expert felt they needed to know to progress their own work. Describing this in terms of the interactional experience of being at the meetings, one scientist recalled:

> I think sometimes in our consortium meetings, people easily end up - because it's natural - talking with their own language and other people don't always want to really stop them and say, "well, I don't really understand a word. Can you really explain everything again in a completely different way," because partly you maybe don't want to be rude, partly you maybe think, '*well, I don't have to know all those details*,' partly you don't want to demonstrate that you still haven't understood these things. So there are all these combinations where out of laziness mixed with maybe being a bit shy, mixed with maybe thinking, '*well, you know, I don't have to know all of that*.' I think in these meetings, especially with time, people are less and less prone to ask questions. [Emphasis added]

Attaining a workable model of what counted as enough knowledge and understanding of other expertises was essential to the group's progress, as this utilitarian approach to interactional expertise was used across the expert-network, premised upon a simplified inter-language. In the next section, we explore the limited case in which the disciplinary exchange point required the translation of practices in a limited form of contributory expertise.

Contributory expertise and tacit knowledge in the expert-network

Inherent to our notion of the expert-network and the SEE perspective is the recognition that scientific work and contributory expertise is premised upon specialist tacit knowledge. This was described by one cell biologist - the project PI and second author on this paper - in an interview during the early stages of the project, in terms of an often used gardening analogy: "it's the nuances, it's having green fingers and knowing how to do certain things in certain ways, especially with tissue culture, is really important". Another team member, a chemist showing their awareness of the craft skills needed, described stem cells as "very finicky things", to capture their fussiness, and that they are difficult to please. This framing captures the craft skill of cell culturing, and the specific, sometimes idiosyncratic, behaviours attributed to in vitro manipulation (Stephens et al., 2011; Osbeck and Nersessian, 2010; Meskus, 2018). Cell biologists frequently assess the state of cells by describing them as 'happy', as evident in this account: "I think with the cells you get used to the way they look, and shape and size of the cells change depend on whether they're happy or not happy. So that's just general morphological features." Later, the same cell biologist explained the relevance of this for the expert-network, in the context of the differing needs and existing knowledges of the team members, to show how experts across the group were able to gain enough understanding of what 'happy' meant, and how it was achieved:

> One of the issues is that we're trying to adapt the [CARS] instrumentation so that we can do life cell imaging. Which means the cells have to be kept happy, which is temperature and gas. But because the MRI group… are aware of the modification of the environment [through their previous projects]. It's just the same skills. But I did take [a physicist] up to our laboratory to show him the incubators that we use to incubate the cells to keep them happy. I showed him the cells down the microscope... So they have seen my lab I've seen their lab which is good.

Equally, an engineer drew upon a different analogy from popular culture to capture the tacit craft skill of instantiating their expertise in practice:

> It's a bit of a dark art, you know, rheology. I've been working on rheology for many years now and I'm still learning. I appreciate these different artefacts which come in to rheological measurement for example surface tension, things you haven't appreciated before which can make slight inconsistencies in your measurement, inaccuracies and things… The thing is, with rheometers anyone can come along, put a sample in, do a measurement – what comes out of it might be rubbish. You need to programme a rheometer precisely to get the information which you want and is correct. And that is the 'dark art' if you like.

Expert-networks are replete with tacit knowledge with each discipline having its own articulated in a distinct way. It is a key element of why gaining interactional or contributory expertise is so difficult, and why expert-networks can function to reinforce identity work around existing boundaries as opposed to break them down.

As noted above, almost all the disciplinary exchange points across the expert-network involved knowledge or materials, meaning they involved only interactional expertise. However, there was one distinct example in which a scientist was required to take on a level of tacit knowledge and contributory expertise from another discipline in order to deliver their work. This involved a rheological engineer active in work packages two and three who needed to conduct some basic tasks from cell biology. Essentially, they needed to keep murine lipid cells alive for one-to-two weeks in order to conduct their experiments with the biorheometer. This requirement involved successfully conducting only basic cell culturing tasks, and in no way constitutes the full contributory expertise of designing, conducting, and publishing complete cell biology research projects. Yet, as this example shows, the attainment of even limited contributory expertise required significant labour and support from across the expert-network.

Around three months into the project the rheological engineer, based at the City 2 site, made repeat visits to a cell biologist in City 1 over several months, first to watch, and then repeatedly conduct, the basic work of passaging (growing and splitting) cells. It was a disciplinary exchange point of knowledge and practice, which occurred in preparation for cell passaging in City 2 during rheological experiments. Due to the work required to prepare the rheometer, it was around a year later before the engineer needed to commence the cell work. Two things happened in this time. Firstly, the engineer became more distant to their training. Secondly, the first cell biologist on the project left the expert-network for personal reasons and was replaced by a new cell biologist. When it became time for the experiments to begin the new cell biologist produced a stock of cells for the rheologist to use as they re-immersed themselves in the practice of cell culturing. At one point, they faced a problem when the cells would not grow on the metal petri dish designed for the rheometer, so they decided to use a standard tissue culture petri dish instead. However, again, the cells would not grow and the engineer could not ascribe why. Detailing the problem, and its solution, the engineer explained:

> I think one example of where my lack of experience might have cost us a little bit of time, talking about a few weeks, is that when the metal petri dish on the rheometer about didn't work. For whatever reason, the cells weren't happy with this metal petri dish on the bottom of the rheometer… So we decided to go back to just using a standard tissue culture petri dish on the rheometer. We found a method of making sure that it was flat on the rheometer and it wasn't a problem, and that's the technique we use now, just a standard tissue culture petri dish. But after several attempts I could not get the cells to grow on the rheometer in these petri dishes, on the rheometer or in the incubator... And it turned out in one of the consortium meetings when I mentioned this, somebody put their hand up and said, "Are you using bacteriological-grade petri dishes?" I said, "*Well I haven't got a clue*." *I didn't know there were two different types*. [Emphasis added]

This example again highlights the significance of the three-monthly project meetings. Many potential causes were considered during this discussion before a cell biologist asked whether the engineer was using bacteriological grade petri dishes. As apparent in the extract, the engineer was not aware petri dishes came in different types, and had simply used the dishes available, which did, in fact, turn out to be bacteriological grade and thus would never support sufficient adherence for the cells to grow. Here we see an example of tacit knowledge in that the use of the correct type of petri dish was such a taken-for-granted given by the cell biologist that it was not even shared with the engineer until after several weeks of unsuccessful culturing (see also Stephens et al., 2018). The role of geography here was not lost to this cell biologist:

> One of the most difficult things with the collaboration with [the engineer] is actually, strangely enough, that they are in City 2. Yes, we can have meetings. We can go down. We can talk over the phone. But being able to walk down the corridor and say, "you're using the wrong plates, do you know that?" would have saved us weeks of time.

Once passaging commenced the engineer needed to confirm the cells were 'happy'. Initially this involved emailing photographs of the cells to the cell biologist for confirmation, before the engineer could recognise on their own that these wild-type fibroblast cells are 'happy' when they look star-shaped or bi-polar, while 'unhappy' or dead cells look more like a ball. This ongoing learning process extended to the engineer autonomously retrieving and implementing the manufacturer's protocol for the nucleus stain DRAQ7 to identify when cells were dead or not, although the rheological engineer noted they could only have done this because the cell biologist suggested it, as they were not aware of the dye before being prompted.

Here we have seen multiple disciplinary exchange points as the cell biologist and engineer share ideas, opinions, materials, and practices. It resulted in the engineer being able to conduct a set of basic cell biology procedures with a level of confidence and competence. However, it is vital to note that the engineer's contributory expertise in cell culturing operated only across a limited set of procedures and remained highly dependent upon sustained disciplinary exchange points with established cell biologists who work to support and scaffold the engineer in acquiring a tacit knowledge-based skill set and troubleshooting problems. Despite these challenges, the end result pleased the cell biologist involved enough for them to describe the work as "really neat, cross-disciplinary experiments, really truly". Given this success, the example still works to show that it is difficult to share full contributory expertise across an expert-network.

Managing and reconfiguring the expert-network

A key task of the project life cycle was keeping the expert-network together and retaining its focus upon successful outcomes. Like many research efforts, the project was challenged by (i) some experiments providing less successful outcomes than anticipated (e.g. the non-natural amino acids and the MRI work), and (ii) changes in the personnel within the expert-network as some people left (through a diverse set of professional and personal circumstances) and new people joined. The reconfiguration of the expert-network both shaped, and was shaped by, the successes and challenges of the consortium. In some instances the replacements were 'like-for-like' (as in the employment of a new cell biology PDRA to replace the existing cell biology PDRA who left the consortium for personal reasons), in others they were not direct replacements (as in the discontinuation of the non-natural amino acid work when the lead researcher relocated to another University), and in others new people joined bringing new expertise as new disciplinary exchange points entered the network (as with the bioinformatician who joined the consortium after it had commenced its work).

Reflecting upon first an instance of replacement, and then one of no replacement, the PI (and second author on this paper) articulated in an interview towards the end of the project:

> when [cell biologist 1] left and [cell biologist 2] joined, [cell biologist 2] was very capable and was able to get out there and talk to people and chase people and hassle them. I think when the non-natural amino acids expert left… I think at that stage we probably worked out that we weren't going to get it to work anyway. So that was less disruptive.

Success here is defined relative to sustaining and shifting research goals for the consortium as a whole, and the disciplinary groups within it. In the cell biology case, the communicative capacity of the new researcher to engage with those already in place was deemed key to the successful replacement. The switch from non-natural amino acids, in contrast, was dealt with by reconfiguring the network's research plans.

There were also cases of personnel shifting within the consortium and focusing upon a new area of expertise and new goals (e.g. moving focus from MRI to PET). This example repays further examination. In the initial research proposal, one team member – a chemist based in the University MRI scanning facility – was included to work on non-invasive MRI of the trackers for cell lines produced by the non-natural amino acids team and on using Chemical Exchange Saturation Transfer (CEST) for live cell imaging. These elements of the overarching project encountered technical difficulties and neither could be made to work, as described by the chemist involved:

> we were hoping that these non-natural amino acids would be really good for MRI imaging. But unfortunately, MRI is a fairly insensitive imaging methodology in terms of being able to pick up injected tracers, but there's just no way, the technologies just don't meet in the middle. There's no way you can make an MRI sensitive enough to pick up the levels of proteins with non-natural amino acids the technology could produce. And there's also no way that the non-natural amino chemists could bump up the amount of protein produced to sort of match the MRI insensitivity. So, unfortunately, there's a little gap in the middle that meant those two technologies wouldn't really meet.

As this was becoming apparent, organisational issues also arose because, as just noted, the lead of the non-natural amino acids team relocated to a different (overseas) University, and the overarching University funding for the MRI support team was downsized. Subsequently the chemist became based in the University's PET centre. This necessitated further reorganisation of the expert-network, and with it, some research activities within the project. Unlike the cell biology staff change, a 'like-for-like' replacement was not deemed necessary, as the MRI work had proved unsuccessful. Instead, the chemist continued to contribute to the project in their new role by exploring PET tracking, which made an unexpected yet productive contribution to the project and those designed to follow it, as articulated by the cell biologist who was PI on the project (and second author on this paper):

> It actually made it slightly easier for me, that changing of position, because s/he ended up being [physically situated on the same campus]. So I could meet with them and talk to them a lot more readily… And it turned out absolutely to be more beneficial because they went from working with [the MRI centre] where we were doing some work, mostly related to the CEST, which then subsequently didn't work, obviously. Then the move opened up the avenue for PET. So that was actually a fortuitous move. I'd like to say planned (laughter), but unfortunately not.

In terms of the micro-organisation of work, this example shows how managing the project involved the curation, maintenance, and creative reconfiguration of the expert-network in response to the (often unanticipated) circumstances that arose. Such practices can be necessary in any team-based work, but take a specific form in interdisciplinary expert-networks where maintaining or creatively reconfiguring disciplinary exchange points remains important.

Discussion: Learning points for STS and interdisciplinary practitioners

> *So it's difficult to do*, interdisciplinary science. What you want to try and do is you want to try and manage the people so that they feed into each other but they don't necessarily overlap. And the reason why I'm saying that is because they have to learn a whole new bunch of skills. Now, you could argue that's a good idea, *but that takes time*, and it's more than a three-year grant allows. [Emphasis added]

The above quotation is from the final interview conducted in the project with the consortium PI (second author on this paper). It shows the aspiration of interdisciplinary work, as well as the real-world constraints, and captures the value of the expert-network framework. While the project had successes, the consortium did not achieve all it set out as some aspects failed (CEST and non-natural amino acids) and other aspects did not achieve all that was proposed in the timeframe available (rheology and CARS). This given, it did achieve some things it had not originally planned, as the expert-network was reconfigured.

In this paper, we have sought to understand how the consortium operated as an interdisciplinary group by articulating and demonstrating empirically the notion of the expert-network. An expert-network is a set of managed relationships between one or more disciplinary groups who are collaborating towards a broadly shared goal. Within the expert-network, researchers retain expert status in their own discipline, premised upon trust relationships and demonstrations of ability, and make ongoing judgements about how much of the technical detail of their expertise needs to be shared with others, and how much of others' technical expertise they need to learn. As such, the expert-network is sustained by ongoing negotiation and mutual trust.

We suggest the notion of an expert-network is valuable in two contexts: first, that it is useful for social scientists seeking to analyse interdisciplinary groups, and second, that it is useful for those conducting interdisciplinary work to make sense of their context and its management. On the first point, we argue the expert-network notion provides an analytically productive social science perspective on understanding how interdisciplinary work operates when researchers from different disciplines collaborate. It leverages insight into how disciplinary exchange points facilitate movement of materials, knowledges, and sometimes practices between expert groups, while also allowing these expert groups to retain, and in some regards, reinforce, singular disciplinary identities. Equally the recognition of potential reshaping within the expert-network captures the interrelatedness of issues such as the personal life choices and challenges of members of the network (who may leave or adopt a new role) and the permeability of the network as new members join, and bring with them new perspectives on existing disciplinary roles, or entirely new areas of expertise. In this way, the expert-network accounts for both the potential flexibility and stability of disciplinary teamwork. As an analytical framework, the expert-network also shows how hard interdisciplinary work can be to accomplish. This can be both due to the tacit component of interactional and contributory expertise that requires time, immersion, and scaffolding to transfer, and to the affective and lived experience of all human interaction here framed in an interdisciplinary context.

The distinctiveness of the expert-network concept - in the context of the existing work on interdisciplinarity noted earlier - comes through the relationship between six components, none of which are unique on their own, but bring value and novelty in their interrelation. The first is that our use of the expert-network concept does not seek to define interdisciplinarity or delineate it from other modes of practice, but instead thinks through how interdisciplinarity is enacted by practitioners operating under its name. The second is the focus on the relatedness and non-relatedness of material, knowledge, and practice exchanges, with the novelty compared to some other approaches to interdisciplinarity specifically found in the focus upon material elements of group work. The third is the inherent recognition within the expert-network concept of the fluid and non-static capacity of interdisciplinary work, capturing how ideas, goals, and relationships can (and perhaps should) shift. By accommodating change over time, and embedding it within the account, our concept enables analysis of altering network make-ups, and how disciplinary exchange points are maintained or reconfigured over time. The fourth is the inclusion of disciplinary identity work, and by extension the potential study of power (cf. Stephens et al., 2018), that this concept facilitates, recognising participants as lived-beings with experiences and demands beyond the expert-network that shape practice. The fifth component, quite simply, is the simplicity of the concept itself, making it amenable to application by analysts and practitioners. The sixth, and final, component of its distinctiveness is its integration into the SEE framework, both providing the expert-network approach with further intellectual grounding, and contributing to further elaborating SEE itself.

As noted above, none of the components are unique within the interdisciplinarity literature alone. Resisting singular definitions of interdisciplinarity is found in Jacobs and Frickel (2009) and to a lesser extent in Barry, Born, and Weszkalnys (2008). Osbeck and Nersessian (2010) and Morrison (2017) also draw our attention to materiality in addition to knowledge and practice. MacLeod and Nagatsu (2018) show that disciplinary interactions can shift overtime, with their focus upon crystallisation on integrative platforms showing how expert-networks can, eventually, become stabilised in context specific ways. Multiple authors have pointed to interdisciplinary contexts as sites of power (Stephens et al., 2018; Callard and Fitzgerald, 2016; Albert et al., 2016), while Callard and Fitzgerald (2016), as well as Osbeck and Nersessian (2010) also make explicit the identity and emotionality of such experiences. Finally, Gorman (2002), Andersen and Wagenknecht (2013), and Osbeck and Nersessian (2010) draw upon the SEE framework in some regards within their work. However, in its totality, the expert-network approach configures these elements in a form that potentially yields value for others who creatively deploy the concept in novel contexts and novel ways.

The expert-network concept is rooted in the analysis of this single case, and the intended utility driving it is to illuminate the work of this specific consortium. This given, it could have applicability beyond this specific context, specifically because of its flexibility. Clearly, not all interdisciplinary groups operate in the same way (Fiore, 2008; Huutoniemi et al., 2010), yet the focus upon a non-static network that can be reconfigured and required maintenance is likely to prove valuable in contexts different to the one specified here. Subsequent application in additional contexts could further strengthen the analytical breadth and robustness of the expert-network approach as other variables and insights could be incorporated. Indeed, further learning could even be gained from analysts' experiences in which their specific setting proves ill-suited to an expert-network mode of analysis. This aim here is not to assert which contexts the expert-network framework is or is not well suited, but to offer it as a possibility for others to consider.

As such, the expert-network conceptualisation makes a contribution to social science analyses of scientific practice. It provides a model for subsequent STS researchers to analyse other examples of interdisciplinary work. If doing so, the researcher should map the contributory and interactional expertise across the network, and document the intended and actual flows of knowledge, materials and practice, and the disciplinary exchange points through which these occur. The analyst can then identify in what form any interactional expertise or interlanguage formation occurs (if any) and describe the practices that support this, as well as the judgements as to when researchers believe they know 'enough'. Linked to this, it is also valuable to identify when and how tacit knowledge and the challenges in exchanging it frame the expert-network's practice. Finally, the analyst should document changes in the network overtime, and document how the curational and maintenance work is accomplished. Corollary to this, the notion of the expert-network developed here also contributes to the SEE framework by providing a mechanism by which it can be applied to assessing interdisciplinary research projects.

The second reason we suggest the notion of the expert-network is valuable is for framing thinking about the conduct of interdisciplinary work, and informing its management. By encouraging those engaged in collaborative projects with multiple expertises to consider themselves part of an expert-network, and the implications this brings, we hope the experience and productivity of doing such work can be increased. Subsequently, in closing this paper we contribute to this by articulating some learning points for those managing interdisciplinary teams that reflect upon the experience of the project described through our expert-network approach. This is specifically from the perspective of the project Principle Investigator, P. Stephens, summarising key learning points from the consortium and the engagement with the expert-network concept, in order to offer advice to like-minded colleagues. These points address the overlapping themes of management and leadership, interdisciplinarity, flexibility and logistics:

Management and leadership:

- A well thought out project plan by the Principle Investigator is beneficial from the start, as managing large projects requires someone with the overall vision and clarity of thinking across, what are often, quite disparate research disciplines. This plan would benefit from mapping the expert-network, and making explicit the disciplinary exchange points where knowledge, materials and practice cross disciplinary borders.

- A Principle Investigator on a large grant does not need to be a contributory expert in all disciplines involved. Instead it is valuable to ensure contributory expertise to all the disciplines involved exists in the project through the Co-Investigators.

- Principle Investigators should not seek to overly direct the work of others, but should instead believe in the Co-Investigators. Micro-managing all elements of the project is unlikely to make for a successful collaboration or outcome. The trust upon which this is based is best formed through interpersonal interaction, and ideally, this will be in place before the research proposal is submitted.

Interdisciplinarity:

- Working across disciplines is challenging, especially across biological and physical sciences, as each discipline has its own technical language. Creatively producing a shared set of terms and references that respond to the specific context of the disciplinary mix and project goals is productive for developing the interactional expertise across the project that fosters improved understanding.

- Training people to work across disciplines is also challenging, especially when dealing with disciplinary tacit knowledge. This is knowledge that is crucial in order for something (e.g. an experimental protocol) to work properly, but is difficult to effectively communicate to someone else over the short period of time that is associated with a research grant.

- Learning contributory expertise in a new discipline requires a significant investment of time. It is possible to pass on limited practical experience – such as the rheological engineer who could conduct certain cell culturing procedures – but it requires ongoing support and scaffolding from an established contributory expert.

- In practice, the expert-network can reinforce disciplinary boundaries within an interdisciplinary team more so than break these boundaries down because contributory experts remain authorities within their specialisation with limited engagements funnelled through the disciplinary exchange points.

Flexibility and logistics:

- Being clear about risks in the planning and communication with project members is important. Principle Investigators should have the confidence to re-direct research (mid-project) to other areas if needed to ensure a successful overall outcome.

- Meeting physically and regularly as a project team improves understanding and builds trust relationships. The project reported here found three-monthly intervals valuable, but other projects with different timelines or geographical contexts may opt for a different cycle.

- If research personnel need to change it is best to view this positively as new people bring new expertise and new insight. Hence, embrace the flexibility of the expert-network, but continue to retain an up-to-date plan that makes clear the disciplinary exchange points where knowledge, materials, and practices pass between disciplines.

- Principle Investigators should not underestimate the time needed to commit to running the wider project and making it a success. Often the Principle Investigator has to deal with not only scientific matters but also personnel ones in order to ensure a smooth running of the overall project, and these personnel matters can themselves reshape the expert-network.

To conclude, interdisciplinary research is both challenging and productive. By recognising the different skills and knowledges across a project as an expert-network the research team can identify risks and scope for flexibility. In so doing, project teams can discover more about the practicalities of interdisciplinarity, as well as discover more about their science. As a framework for the social scientist, the notion of an expert-network offers a perspective on analysing detailed datasets on interdisciplinary cooperation in practice.

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Affordance, Role, and Script as Complementary Concepts of Artefact-User Interaction, Illustrated by the Example of an Egg Separator

Ingo Schulz-Schaeffer

Institute of Sociology, Technical University of Berlin, Germany/schulz-schaeffer@tu-berlin.de

Abstract

This article suggests employing the affordance concept, the role concept, and the script concept in a complementary manner as analytical tools for investigating artefact-user interaction at three different levels of stability, abstraction, and interrelatedness. It argues that the affordance concept is best suited to describing general possibilities for action constituted by common technical features in combination with common taken-for-granted knowledge of how to use them. The script concept, in contrast, is best suited to analysing the most concrete situations of interaction between artefacts and users: those situations in which the interaction is defined by one particular course of action. In between, there is a middle level characterised by artefacts and users being involved in several interrelated activities for which the role concept provides the tools for analysis.

Keywords: affordance, role, script, role theory, artefact-user interaction, actor-network theory

Introduction

The concept of affordance has become popular as a concept for analysing and understanding the interaction between technology and users. It is valued as a conceptual tool that allows the material dimension of sociotechnical constellations to be taken seriously and, thus, for social determinism to be avoided without falling back into technological determinism (Hutchby, 2001: 444-445, 453; Treem and Leonardi, 2013: 146-147; Davis and Chouinard, 2016: 246; Evans et al., 2017: 37). Early social constructivist approaches such as Pinch and Bijker's (1987) social construction of technology approach indeed leaned towards social determinism by exclusively focusing on how new technology is shaped by social factors and ignoring how the technology in turn shapes social settings.

However, this missing part was soon added to the picture, most prominently by actor-network theory (ANT). To describe the heterogeneous ensembles of sociotechnical constellations in a way that equally considers social and material agency, the authors of ANT and related work developed a concept of script and referred to concepts from role theory. Similar to the concept of affordance, these are relational concepts for describing the interaction between technological artefacts and users, developed with the explicit intention of providing an alternative to both social constructivism and technological determinism (Latour, 1988: 307-308; Akrich, 1992b: 208).

The affordance concept and the script and role concepts follow different paths. Norman (2002 [1988]) introduced the affordance concept into design studies to refer to the most general and enduring relational properties in the interaction between artefacts and users. In contrast, Akrich (1992b), Latour (1988) and Callon (1986a) introduced the concepts of script and role into ANT to show how there are no artefact-user relations based on stable properties of humans or nonhuman objects. By pointing out how the relations between artefacts and users are based on scripts and depend on how users and artefacts comply with the roles assigned to them, they disclosed how both these relations and the properties of the human and nonhuman entities involved are co-constructed and are continuously "in the making" (Latour, 1987: 1-17).

In the meantime, the scopes of both the affordance and the script concepts have changed. The affordance concept has been expanded considerably, including more specific and changeable relational properties of artefacts for users. Scholars now include relational properties that depend on individual perceptions and capabilities, and on social positions (Davis and Chouinard, 2016: 245-246), on diverse goals, and on different contexts (Treem and Leonardi, 2013: 146). Additionally, affordances are construed as properties that may occur at multiple levels of scope and abstraction (McVeigh-Schultz and Baym, 2015).[1] Conversely, the script concept has been applied to quite general and enduring aspects of artefact-user relations. The concept of gender script, for instance, focusses on how gender stereotypes and long-established gender relations are inscribed in and reproduced by technological objects (van Oost, 2003: 195).

Contrary to the tendency to extend the concepts, I advocate using them in a narrow and focused manner, applying each of them to capture those particular aspects of artefact-user interactions for which they are best suited. Accordingly, this article suggests employing the concepts of affordance, role, and script in a complementary manner to distinguish three different levels of stability, abstraction, and interrelatedness in artefact-user interaction. The affordance concept can best be used to analyse general possibilities for action where the artefact has common technical features aligned with the users' culturally shaped common knowledge about how to use that artefact. The script concept can best be used to analyse the most concrete situations of interaction between artefacts and users where the interaction is defined by one particular course of action. The role concept covers analysis of artefact-user interactions at a middle level of level of stability, abstraction, and interrelatedness where artefacts and users are involved in several interrelated courses of action within particular fields of action.

The remainder of this paper is structured as follows. In the following section, I describe the roots of the affordance concept. It discusses some conceptual problems related to applying this concept to the relation between artefacts and users and suggests a use of the concept. Then, I cover the roots of the concepts of script and role and analyses similarities and differences between the two concepts. This analysis leads to viewing them as closely related but focusing on the interaction between artefacts and users with respect to either particular courses of action (script concept) or particular fields of actor positions (role concept). After that, I amalgamate these considerations and present my suggestion of how to employ the concepts of affordance, role, and script in a complementary way. The final section briefly summarises the paper.

Affordance

The affordance relation

Gibson invented the term affordance to name a particular relational notion of how the environment provides resources to animals (Gibson, 2015 [1986]: 119). According to his original definition, "[t]he *affordances* of the environment are what it *offers* the animal, what it *provides* or *furnishes*, either for good or ill. These affordances *have to be measured relative to the animal*" (Gibson, 1979: 127 [emphasis in original]). The physical properties of the environment become resources or restrictions only in relation to the characteristics of an animal species. For heavy terrestrial animals, for instance, terrestrial surfaces provide support and enable them to walk or to run while a water surface does not. For water bugs, however, water does provide a surface, which they can stand on and cross (Gibson, 2015 [1986]: 119-120). Thus, the stand-on-ability provided by a surface for an animal is a relational property, an affordance.

In Gibson's view, affordances result from the interaction between physical properties of the environment and species-related properties of animals. Species-related properties include shared physical attributes and abilities such as weight, size, or locomotion abilities. They also include shared behaviours as defined by the species' way of life. These attributes, abilities, and ways of living determine how the environment with its physical properties becomes valuable for the animal (Gibson, 2015 [1986]: 130-132). For Gibson, affordances are invariant to the actual needs and perceptions of the individual animal (Gibson, 2015 [1986]: 121). They exist for the animal whether or not it pays attention to them or feels the need to refer to them in the actual situation. This invariance arises from affordances reflecting the relation between environmental properties and properties of animal species and not the relation between environmental properties and the individual animal with its actual perceptions and views.

Gibson also applies his affordance concept to humans, which is unproblematic as long as the affordance relation is a relationship between environmental properties and the attributes and abilities of the human body. For instance, because of the morphology of the human hand, certain objects afford grasping them (Gibson 2015 [1986], 34-35). However, most of the characteristics and capabilities of human actors that make objects valuable to them are not just characteristics and abilities of the human body itself but are acquired by learning and training within and as part of particular cultural contexts. Thus, most of the time, the value of objects for humans is not defined by characteristics humans share as a species but by particular sets of cultural knowledge, skills, beliefs, values, etc. The same applies to the ways of life of humans. Since "man is by nature a cultural being", as Gehlen (1950: 86) puts it, even the most basic species-related needs are culturally shaped. Consequently, the affordance concept is not easily transferable from animals to humans, as we will see in the next section.

Affordances of artefacts for users

The rise of the affordance concept in design and technology studies began with Norman utilising it as a tool for distinguishing between good and bad design of objects. In Norman's reformulation of Gibson's concept, "the term *affordance* refers to the perceived and actual properties of the thing, primarily those fundamental properties that determine just how the thing could possibly be used" (Norman, 2002 [1988]: 9). Affordances are the "possible uses, actions, and functions" (Norman, 2002 [1988]: 82) of objects for users. And they are "jointly determined by the qualities of the object and the abilities of the agent that is interacting" (Norman, 2013: 11). Norman's view on affordances has strongly influenced the concept's subsequent development. Most of the current definitions focus on the interaction between artificial objects – mainly technological artefacts – and human actors in their capacity as users. Most of them share the view that affordances are possibilities for action and that they are relational properties (Evans et al., 2017: 36, 39; Hutchby, 2001: 444; Treem and Leonardi, 2013: 146; Davis and Chouinard, 2016: 241).

Norman is especially interested in what he calls "perceived affordances" (Norman, 1999: 39), affordances that can be deduced directly from the visible[2] structure of the objects without the user needing further information: "Affordances specify the range of possible activities, but affordances are of little use if they are not visible to the users. Hence, the art of the designer is to ensure that the desired, relevant actions are readily perceivable" (Norman, 1999: 41). Designed objects are generally supposed to be used in particular ways. The task of the designer is to enhance the visibility of the respective possibilities for action but not of all the other affordances and especially not of those seen as unwanted ways of using the object. For example, a designer would want to render visible the particular kind of graspability of a porcelain cup but not necessarily its throwability.

Perceived affordances are affordances that are advertised directly by the physical shape of the object (Norman, 2013: 18). According to Norman, a flat plate mounted on a door signals by its physical appearance that the door affords pushing. In the same way, a doorknob signals that the knob affords turning, pushing, and pulling; or a slot signals that it is for inserting things into (Norman, 2013: 13). Norman sharply distinguishes between perceived affordances and perceptions of possibilities for actions that are based on cultural knowledge of some kind: "A doorknob has the perceived affordance of graspability. But knowing that it is the doorknob that is used to open and close doors is learned […] The same devices on fixed walls would have a different interpretation […] The interpretation of a perceived affordance is a cultural convention." (Norman, 2013: 145)

There are two major conceptual problems with Norman's strict distinction between perceived affordances as "properties of the world" and the "arbitrary, artificial and learned" (Norman, 1999: 42) cultural conventions:[3]

(1) Not one of Norman's examples actually supports his claim that the physical shape alone without additional knowledge-based interpretations allows people to figure out how to use the object. Consider, for example, the physical shape of a slot, which according to Norman signals that it is for inserting things into. Actually, however, there are many objects with slots, where inserting things into would not be the best of ideas – the slots of a radiator grill for instance. Whether slots really are for inserting things does obviously not follow directly from their physical shape but requires learned knowledge. Nor does the physical shape of a slot indicate which kind of things to insert. Even if, as in the case of coin-operated machines, the slots are precisely adapted to the size of the accepted coins, a number of other objects could still fit into the slots (e.g., foreign coins, folded bills, chewing gum).

Truly, most people do not need further instructions to use coin slots properly; however, not because the slot itself signifies how it should be used but because people have become accustomed to using coin slots and the corresponding knowledge has become part of the tacit everyday knowledge of our technological civilisation. Harry Collins (1990: 106) nicely illustrates this by comparing different generations of slot machines:

> What were once explicit rules can become part of a society's unexpressed *taken-for-granted-reality* […] Shifts of this sort can be seen by looking at the changing instructions on simple machines in the public domain. For example, an elementary pinball machine, built in the 1830s […] has instructions that include the following: […] '1. Place coin or free play token in coin slide and push slide all the way in until balls have cleared then pull slide all the way out. 2. Push RED knob to elevate ball to playing surface. 3. Pull back BLACK knob on plunger and release'. Nowadays, everyone knows how to put money in a pinball machine and how to make the balls run. The 1980s version has only the following rudimentary instructions in the place of what went before: 'Insert coin to start machine', 'Insert coins for additional players'. (Collins, 1990: 106)

(2) Norman does not clarify whether possibilities for action provided by artificially fabricated physical properties of artefacts can count as affordances. Consider, for instance, the mechanism enabling users to unlock doors by turning doorknobs clockwise or counter clockwise. Without doubt, how to turn the doorknob is learned knowledge and, thus, a cultural convention. Changing this convention, however, would also require changing the mechanism itself. Consequently, the mechanism is part of the cultural convention. Thus, according to Norman's binary distinction between affordances and conventions, the "unlock-ability" provided by the door locking mechanism is not an affordance. More generally: If a possibility for action can be technically implemented in different ways, and thus requires different learned knowledge of how to use the respective artefact, that possibility cannot be considered an affordance. For good reasons, Norman avoids raising this consequence. In a world filled with artefacts of this kind, it simply makes no sense to distinguish in this way between affordances and conventions.

Both of these conceptual problems point in the same direction: When analysing the affordance relation between humans and their environment, it is not helpful to distinguish between properties of the world and artificially fabricated properties. This distinction creates more problems than benefits, especially when the objects of interest in the environment are artefacts. For objects with artificially fabricated properties, cultural knowledge necessarily affects the affordance relation. If an artefact's mechanism represents the technical side of a cultural practice, the respective possibility for action exists only for users who know this practice. Thus, culture-specific needs, views and practices are involved in defining the range of possible activities offered by the design of these objects as well as the range of possible uses considered by their users.

Affordances and taken-for-granted knowledge

The strength of the affordance concept lies in its ability to capture the most stable and context-independent use-related properties of objects and, nevertheless, to conceive of them as relational properties. For Gibson and Norman, affordances are simultaneously stable and relational properties because they conceive the affordances "as organism-environment relations" (Davis and Chouinard, 2016: 244). However, as argued above, due to the cultural orientations and perceptions involved, the relation between users and their artefacts is not of this kind. Attempts have been made to deal with this problem by counterbalancing the relativity of user perception with the fixedness of materiality. Accordingly, Treem and Leonardi argue "that the affordances of one technology are often the same or similar across diverse organisational settings because the material features of the technology place limits on the kinds of interpretations people can form of it and the uses to which it can be put" (Treem and Leonardi, 2013: 146 with reference to Leonardi and Bailey, 2008 and Leonardi, 2011).

Artefacts' perceivable material features do matter as signifiers of possibilities for action. But they do not work as unmediated as assumed by Gibson and Norman (Bloomfield et al., 2010: 415). Obviously, the physical shape of an artefact can be used in design to narrow down the options of how to handle it. Consider, for instance, a door without any bar, knob, or handle providing a grip for pulling but instead equipped with a metal plate where the average-sized standing human would put their hand to push. Most people will quite naturally push this door to open it. Its physical shape narrows down the options of how to physically manipulate the door from pulling or pushing to pushing only (Donald A. Norman, 2013: 15, 60, 133-134). However, it has this effect only because most people know what doors look like, what their intended use is, and that they are usually opened by either pulling or pushing.

The more such use-related knowledge is part of users' tacit and taken-for-granted everyday knowledge, the more effortless it comes to mind when people perceive the artefact's corresponding feature. The more common this knowledge is, the more general can these features be employed as signifiers. Many technical features are so common and so closely related to common everyday practices of use and to the corresponding tacit knowledge that they have turned into universally understandable signifiers. An example is the coin slot mentioned above. Not only physical features but also symbol-based technical features turn into universally understandable signifiers in this way. Consider, for instance, the technical feature for deleting files, which is provided in countless computer programs by a small space on the screen with a symbol showing a wastebasket to where files are dragged and dropped to delete them.

In some crucial respects, the relation between users and an artefact is different to the organism-environment relation. Consequently, the concept of affordance is not simply transferable without losing conceptual clarity. The considerations presented here are an attempt to preserve the core content of the original concept while adapting it to the characteristics of artefacts and humans (in contrast to objects of the environment and animals in general). Accordingly, the term affordance should be reserved for those relational properties offered by artefacts for human users that result from common technical features in combination with common taken-for-granted knowledge and know-how, making them easily understandable and useable for large user populations.

There are other concepts that address the importance of established common understandings of how to use technological artefacts. Williams et al. (2005) argue that the influence of established understandings on "both the design and the appropriation of new technologies" (Williams et al., 2005: 123) is comparable to the influence of genres in film production and consumption. "Film studies emphasized the elaborate codes, grammars and rules of production developed by cinema and the mature ability of viewers to decode the film text" (Williams et al., 2005: 123). In a similar way, the familiar and widely applicable knowledge about particular classes of technological artefacts such as the slot machines or the typical elements of graphical user interfaces mentioned above also represent genres. "Such genres serve as an important resource for designers (in reducing uncertainty about consumer acceptance) and for users (in terms of facilitating understanding of the uses and affordances of artefacts and thus their ease of uptake and usability)" (Williams et al., 2005: 123-124; cf. Löwgren and Stolterman, 2004: 103, 166). Based on these considerations, Hyysalo (2010) characterises the "genres of prevailing technological culture" as "cultural stabilization of meanings" or "cultural maturation" (Hyysalo, 2010: 13). The "conventions, images, 'grammars,' and narrative structures" provided by cultural maturation, he argues, "can be trusted by designers to be decoded in fairly nuanced ways by all those people who have basic competency in a given technological culture" (Hyysalo, 2010: 13).

The concepts of genres of technological culture and cultural maturation share with the reformulation of the affordance concept I have suggested above the view that the taken-for-granted knowledge of established technological cultures matters. Because it provides orientation at a general level where it is applicable to the many situations of using technology that presuppose the respective technological literacy. However, the main focus of the concepts of artefact genre and cultural maturation lies on cultural stabilisation of meanings, while the affordance concept allows for a more explicit account of the socio-material character of the general and generic possibilities for action discussed here. From this perspective, these possibilities for action are not simply a result of common knowledge that informs both the design and the use of technological artefacts. Rather, they result from common technical features in combination with common taken-for-granted knowledge. Though it is true that these technical features are not just physical affordances in Norman's sense but are also shaped by cultural conventions, it is also true that they are not entirely conventional. The technical features also rely on the material properties of the artefacts' components and processes. The possibilities for action they provide are also a result of material agency (Pickering, 1993) which is beyond the reach of cultural conventions. For instance, cultural conventions have prompted designers to construct bicycles for women on which the user sits aside just like the equestriennes of former times sat on the side-saddle (Pinch and Bijker, 1987: 38). In the early days of the bicycle, this materialised cultural convention may have given some groups of women the option of riding bicycles without violating the conventions of modesty of their time. However, without the conservation of angular momentum that prevents the moving bicycle from tipping over – a physical property of the bicycle's spinning wheels that exists independent from any cultural convention – this option would not exist at all.

Script and role

Callon and Latour developed ANT to overcome shortcomings of earlier approaches in the social study of science and technology. Their "general symmetry principle" (Callon and Latour, 1992: 348) results from a critique of how the social constructivists privileged social factors (Latour, 1987: 143-144; Callon, 1986b: 197-198). To describe technological innovation in a way that takes social and material agency equally into account, Callon and Latour draw on notions from role theory. With the same intention, Akrich (1992b: 206) developed her concept of script, which soon became part of the analytic tools of actor-network theory.

According to Callon (1986b: 211), a successful innovation is a result of a process "by which a set of interrelated roles is defined and attributed to actors who accept them". Innovators at first envision a scenario (Callon, 1986a: 26; Akrich, 1992a: 174, 1992b: 208), which defines roles for a set of human and nonhuman entities that are supposed to assume them. Developing and implementing a new technology then is a process of enrolling these entities, that is, of making sure that they adopt the proposed roles. This does not mean that innovators are necessarily successful in enrolling the relevant entities according to their plans. But when a successful technological innovation eventually occurs, it is because, somehow, a sufficiently consistent and coherent set of interrelated roles has emerged.

Similar to Callon, Akrich argues "that when technologists define the characteristics of their objects, they necessarily make hypotheses about the entities that make the world into which the object is to be inserted. Designers thus define actors with specific tastes, competences, motives, aspirations, political prejudices, and the rest, and they assume that morality, technology, science, and economy will evolve in particular ways." (Akrich, 1992b: 207-208) Accordingly, designing technical artefacts means inscribing "this vision of (or prediction about) the world in the technical content of the new object" (Akrich, 1992b: 208). Inscribing a particular role into a technological artefact implies prescribing corresponding roles to human actors. Adopting Akrich's terminology, Latour (1988) points out that prescription "is very much like 'role expectation' in sociology, except that it may be inscribed or encoded in the machine" (Latour, 1988: 306). Just as role expectations cannot guarantee that people behave in a role-compliant manner, prescriptions also do not determine behaviour (Akrich, 1992b: 208; The Berlin Script Collective, 2017: 13-15).

Both the script concept and the role concept are relational concepts. Just like the affordance concept, they describe the possibilities for action provided by technological artefacts as relational properties. The backdrop against which this notion is established is the relation of distributed agency. The script and the role concept capture the relation of distributed agency from two different perspectives. From the perspective of the script concept, it is a relation of distributed action while, from the role concept's perspective, it is a relation of distributed actor positions.

The script relation

According to Akrich, the script is the innovators' idea about how a new technological artefact shall work as inscribed in its technical content. The artefact's technical features and properties embody the designer's concept of how and for which purposes the artefact should be used. "Thus, like a film script, technical objects define a framework of action together with the actors and the space in which they are supposed to act" (Akrich, 1992b: 208; Akrich and Latour, 1992). The script as inscribed in the artefact "implies a sharing of competences between the artefact proper, its user, and a body of social and technical elements constituting their common environment" (Akrich, 1992a: 174). Depending on how the users subscribe to what is prescribed to them or try to negotiate adjustments or changes (Akrich and Latour, 1992: 261), the script becomes stabilised, modified, changed, or even abandoned.

The script concept has been criticised for overestimating the importance of the designers' intentions and interests. Together with Woolgar's notion of the designer configuring the user (Woolgar, 1991), it has been accused "to convey a somewhat mechanistic 'linear' view of how those embedded values and scripts are likely to be reproduced when those artefacts are subsequently consumed." (Williams et al., 2005: 96) It thus "remains at the level of materialized interests and influences and does not reach into what happens in the encounters between materials and humans" (Hyysalo, 2010: 246). To some extent, Akrich has anticipated these objections by emphasising that "the user, as imagined by the designer" (Akrich, 1992b: 209) is at first just a hypothesis and that it is subject to "the negotiations between the innovator and the potential users" (Akrich, 1992b: 208) if and how these hypotheses become reality. However, as long as the script is construed as being primarily the brainchild of the designer and as long as inscribing scripts into technology is considered the main way of implementing them, the script concept still reflects a designer-centred view that hinders to pay due attention to other sources of scripts and other ways of inscribing them.

This bias can be avoided by acknowledging that every script able to govern a particular kind of distributed action as a whole will have to be sufficiently inscribed into all of the main components that make up the respective interrelated set of distributed activities. It will have to be inscribed not only into technology but also in human practices and in situational requirements of the action. Accordingly, it should be obvious that every script of this kind is the result of heterogeneous engineering (Law, 1987). It may rely on forms of human conduct as much as on technological means. And since "design is rarely a process of invention *ab initio*" (Williams et al., 2005: 118), it may rely on pre-existing routines or new ideas of how to do things as much as it may rely on pre-existing or new technological means. Thus, who and what the 'authors' of a script are, if they can be identified at all, is an empirical question.

The most striking examples of artefact-user interaction governed by scripts are provided by single-purpose technologies, that is, by artefacts that are designed to be used in one particular way, for one particular purpose, and in one particular situation. Consider, for instance, the bulb-shaped egg separator. It looks similar to a honk ball, consisting of a silicone ball that fits into the hand with a small opening at one side. The single purpose of this artefact is to separate the yolk of an egg from the white. To separate the egg, the user has to squeeze the ball, to place the opening of the device directly over the yolk of an egg that has been cracked into a bowl and then to release the ball. This action causes the yolk to be sucked up. To empty out the yolk, the user has to squeeze the ball again.

As this example shows, the script of a distributed action can precisely prescribe what users have to do and what conditions must be met to make use of the possibilities for action inscribed in the artefact. And vice versa, the script can precisely prescribe the technical features of the artefact that are required to fit with the corresponding human conduct. This is because all the inscribed and prescribed activities are the components of one particular course of action that is governed by the script. From the perspective of the script concept, the properties of technological artefacts are relational properties because they contribute to particular courses of action. Being useless and meaningless on their own, these contributions become useful and meaningful as components of the overall courses of action to which they contribute.

Another criticism of the script concept is that artefact-user interaction is seldom governed by individual scripts because users nowadays interact much more often with complex heterogeneous ensembles than with single-purpose technologies. As Hyysalo puts it:

> The 'stage' of socio-technical encounters is almost never cleared to include only the designers' script (or "program for action") and users' response to it (e.g., possible anti-programs or "compliant nonalignment"). Further, many technologies indeed are heterogeneous ensembles that tend to have more complex affordances rather than clear scripts (Hyysalo, 2010: 245).

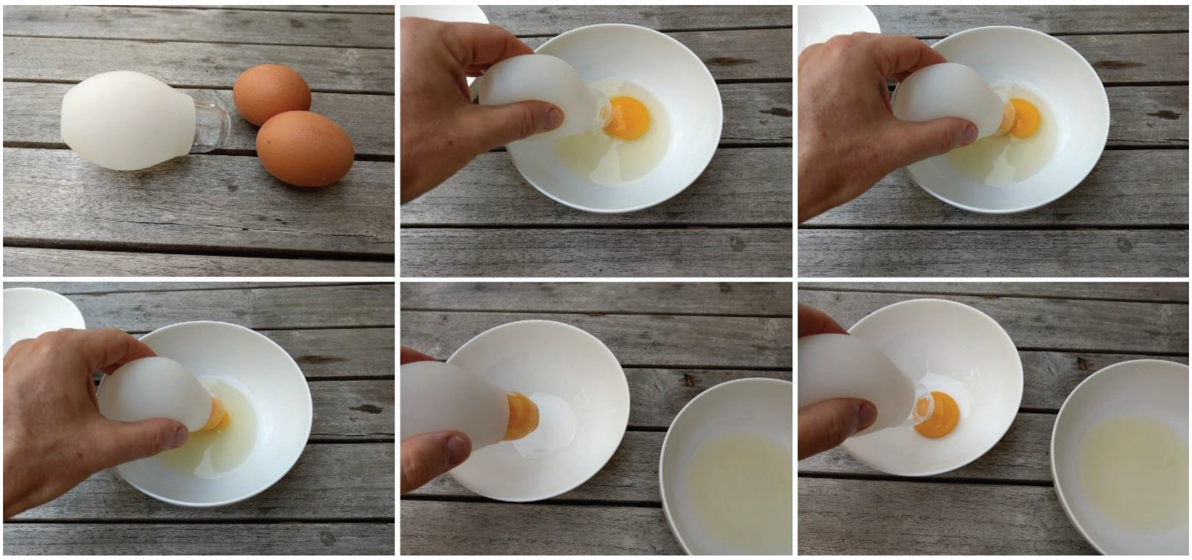

Figure 1. The script of the bulb-shaped egg separator

I agree and disagree with this criticism. I disagree because there actually are numerous single-purpose technologies not only in the world of today's physical artefacts but also in the world of digital artefacts where they are often less visible because they manifest as individual functions within more comprehensive software systems. However, the spell-checker of a text processing software, the file management subprogram of office software, or the noise filter of audio editing software are single-purpose technologies just like the washing machine, the tooth brush, or the railway gate.

On the other hand, I agree that many technological artefacts are components of more complex socio-material ensembles in which users are addressed in different ways and which have different meanings for the different groups of actors who are involved in them. As Williams et al. put it:

> Users are not unitary […]. Different aspects of the representation of the same users are important for different players in the development process. […] They can also be interpreted in different ways. For example, while commercial managers may be concerned with the activities of an organisation, interface designers are concerned with activities of individuals. (Williams et al., 2005: 117)

These heterogeneous ensembles are necessarily the result of "different layers and different modes of configuration" (Hyysalo, 2010: 245). Thus, the scripts in which the human and non-human components of these ensembles are involved become interrelated and intermingled in more or less complex ways. To capture this aspect of artefact-user interaction, I draw on sociological role theory.

The role relation

Sociological role theory (Linton, 1936: 113-131; Merton, 1957; Dahrendorf, 1968 [1958]) describes and analyses how the behaviour of human actors is shaped by "patterned expectations of others" (Merton, 1957: 110), which are linked to the social positions the actors occupy, a social position being a "place in a field of social relations" (Dahrendorf, 1968 [1958]: 34). The patterned expectations of others to holders of positions are called role expectations and the corresponding bundles of behaviour are called role behaviour. Role expectations and role behaviour are relational phenomena. They result from relations between social positions. Role expectations are expectations that individuals, as holders of interrelated positions, have of each other. Roles, therefore, are bundles of position-related behaviours where human actors react to position-related expectations of other actors. "Positions merely identify places in fields of reference; roles tell us about how people in given positions relate to people in other positions in the same field" (Dahrendorf, 1968 [1958]: 36). Social positions are also relational phenomena. They are defined by the role expectations directed at them from other social positions in the same field of positions. Positions are, so to speak, the nodes of a network that results from the relations between theses nodes.

What is a prescription from the perspective of the script concept is a role expectation from the perspective of role theory. What is an inscription from the first perspective is the implementation of a role behaviour from the second perspective. The notion that inscriptions imply prescriptions translates into the notion that the role behaviour of the holder of one position implies role expectations regarding the behaviour of the holders of other interrelated positions. Applying role theory to the relation between artefacts and users, however, requires modifying the original sociological concept and viewing not just humans but both humans and artefacts as holders of positions. Accordingly, both have to be construed as entities that direct role expectations at other entities and are subject to role expectations directed at them (Schulz-Schaeffer, 2016: 6-11). And thus, the behaviour of both humans and artefacts can be described as role-compliant or role-deviant behaviour. Obviously, this extension of role theory to nonhuman actors fits well to ANT's general symmetry principle.

As an example, consider Akrich's (1992b: 217-218) case of a particular type of electricity meter that failed to fulfil a small but crucial part of the expectations placed on it by the electricity company. The role assigned to electricity meters in customers' households is to measure the amount of current consumed. The electricity meter in question was perfectly suited to this task. However, it possessed a feature the company really did not want: it could easily be deactivated by tapping it, allowing customers to consume unbilled electricity. Thus, “the meter failed in its prescribed role“ (Akrich, 1992b: 218). In terms of role theory, not to be easily manipulated by the customers is a role expectation directed at a meter by the supplier. Conversely, allowing easy deactivation is a role-deviant behaviour.

I suggest referring more thoroughly to role theory than do the proponents of ANT and especially putting more weight on distinguishing between roles and positions (Schulz-Schaeffer, 2016: 10-13). This distinction reflects how, in a network of interrelated roles, every actor at one of the network’s nodes is subject to different bundles of role expectations from the actors at other network nodes. In this way, role theory takes into account that holders of positions do not face a single homogeneous set of expectations from others.

To return to Akrich’s example: Not to be easily manipulated is an expectation of the electricity meter from the electricity company’s position as a seller of electricity. From the customers’ position as buyers, however, it is probably more important that the device is correctly calibrated and is not used by the company as a means of overcharging them. These different role expectations of the meter are closely connected to the role expectations predominant in the relation between the positions of seller and buyer. This relation is constituted not primarily by trust but by contractual rights and obligations and by the corresponding possibilities and limits of enforcing them. This in turn shapes the different role expectations addressed from both the company’s and the users’ positions to the position of the device that measures the households’ electricity consumptions.

Sociological role theory focusses on relationships actors have as holders of interrelated positions. Thus, role theory is not interested in every behaviour the holder of a position shows but only in those behaviours that correspond to role expectations of other positions. Applied to the behaviour of artefacts, this means that only those materialised functions and features deserve attention that are related to patterned expectations of end-users, service-providers, installers, maintainers, producers, connected artefacts, and other interrelated positions. However, designers of technological features do not just react to pre-existing expectations from one of these interrelated positions but develop functions and features for imagined future users. These functions and features thus do not reflect existing role relationships but rather assumptions about or suggestions for role relationships that have yet to be established. To put it another way, such functions and features assume or suggest future role expectations. As such, they are relevant from the point of view of the role concept because the dynamics of role relationships is defined by the stability or change of the role expectations involved.

There is a significant difference between physical artefacts and information technology with regard to functions and features that are not actually met by corresponding role expectations. With physical artefacts, it is much more likely than with software that features for which no usages evolve will eventually vanish from the artefact because of the effort it takes to physically produce and maintain the respective features. With software, however, it requires little extra effort to keep technological features of previous versions, and it is often easier to keep them than to remove them. Also, it takes much less effort to add functions and features that have been already developed elsewhere. Consequently, software programs often resemble toolboxes leaving it to the users, which tools to use or to ignore. Especially with respect to software, role-based analysis thus has to take into account that artefacts may include features that never have been and never will be relevant for most of the users. It also has to take into account that when artefacts resemble toolboxes, different users may choose quite different sets and configurations of the available tools (DeSanctis and Poole, 1994).

Differences between script-governed and role-governed artefact-user relations

The distinction between positions and roles reveals some important differences between the script concept and the role concept. The script concept focusses on the interrelatedness of distributed activities in particular courses of action. The role-concept draws attention to the fact that the interaction between artefacts and users are rarely defined just by one script. Rather, as soon as the relation between artefacts and users includes more than two interrelated positions, the holders of these positions are involved in different courses of action in different actor constellations. Accordingly, there are different constellations of interrelated role expectations, which become manifest in different scripts, inscriptions, and prescriptions.

For instance, in its position as a seller and in relation to the customers as buyers, the electricity company employs the electricity meter to implement a script that is intended to prevent the customers from consuming unbilled electricity. The customers in their position as buyers, on the other hand, are interested in scripts that prevent the seller from overcharging them. They may mobilise the support of regulatory bodies (yet another position) to ensure that the meters are properly calibrated, thus inscribing a script in the device that prevents the seller from cheating on them. There are several other positions, which, in relation to the meter's position, lead to further scripts, inscriptions, and prescriptions, such as the company's interest as the provider and maintainer of the meters in devices that are easy to install and maintain.

As artefact-user relations are embedded in increasingly complex sociotechnical constellations, the number of positions and role relationships also increases. The development from conventional electricity meters to smart meters and to the smart grid infrastructure provides a good example of this. When conventional meters are replaced by smart meters capable of transmitting data about power consumption in real-time and these smart meters become part of smart grid infrastructures "a number of new roles are available for the future smart grid 'users' across the energy supply chain" (Silvast et al., 2018: 10). Between producers and users, a number of intermediate user (or producer) positions evolve such as service providers, which use the data from the smart meters to provide producers, suppliers, and end-users with new options of monitoring and managing energy production, distribution, and consumption. Moreover, in many complex sociotechnical constellations "the" end-user is no longer just one position. In smart grid infrastructures, for example, the private household as a user position may become quite different from the position of commercial energy users. These are differences, which "the umbrella term 'user' masks" (Silvast et al., 2018: 11). They become visible only from a perspective such as the role concept, which allows analysing the co-evolution of the components and the relationships of such constellations.

Artefact-user relations that include several interrelated positions differ in two major respects from those that are constituted essentially as relations of distributed agency with respect to a particular course of action. First, artefact-user relations that are constituted by a single script are much easier to establish, to change, and to dissolve than those that include several interrelated positions. The latter require more effort to coordinate the distributed activities. Not only are there different courses of action to oversee but it must also be ensured that they are sufficiently adjusted to one another so as not to interfere with each other. However, when they become adjusted to one another, then the resulting constellations of positions, roles, and distributed activities tend to be more stable and more resistant to change than constellations defined by a single script. This stability arises from each position being defined by the role expectations directed at them from several other positions. Thus, if one script fails and one role relation is destabilised, the corresponding positions do not dissolve automatically. They are still involved in several other role relations with other positions, which also define them. When the positions involved and the corresponding roles and scripts are sufficiently adjusted to each other, it also becomes more difficult to successfully change, remove, or add a position or relation without having to modify a number of other positions and relations. This adds stability to the positions and role relations.

Second, the role concept draws attention to the fact that new technologies and the associated new scripts and role relations are most often not created ex nihilo but build on already existing positions and role relations. Artefact-user relations that are defined by several interrelated positions may (and mostly actually do) include already existing positions. In Akrich's electricity meter example, this is the case for the positions as seller and buyer. Obviously, these positions and the corresponding roles precede the development of the power supply infrastructure. At the same time, they strongly influence many of the role expectations addressed to the meter.

Though it is true that positions are stabilised by the role relationships between them and vice versa, it is – at least in post-traditional societies – also true that they are constantly subject to change. Thus, to some extent the components of sociotechnical constellations and the relationships between them are still in the making. Thus, even after a sociotechnical constellation has been stabilised to some extent, it is still subject to "series of configurational movements" (Hyysalo et al., 2019: 13-14). This is not only because the introduction of new technology may lead to new or changing positions (Barley, 1990), but also because of the active involvement of users of all kinds in innovation processes (Kohtala et al., 2020). Again, the smart meter provides a good example. As a device that allows suppliers and users remote readings, the smart meter occupies a position that is in many respects still similar to that of the conventional meter though it enables new uses such as remote monitoring of household consumption and raises new concerns e.g. with respect to privacy issues. As part of future smart grid infrastructures, however, the position of the device may change dramatically and its original determination as a device for measuring power consumption may become but one of its many new uses (Silvast et al., 2018: 8-10).

Affordances, roles, and scripts: different levels of stability, abstraction, and interrelatedness

The strength of the affordance concept is that it grasps the most stable and common use-related properties of artefacts. As argued above, the affordance relation results from common technical features in combination with common taken-for-granted knowledge of how to design and to use them. The script concept, in contrast, is especially well suited to describing the most fluid, unstable, and arbitrary aspects of artefact-user relations. Thus, it is best suited to analysing how new sequences of distributed activities are negotiated and built by attempts to align new technical features with new practices of use. The role concept comes into play when analysing distributed actions, which gain in stability to the extent that they are related to other courses of action within a field of positions. Integrated sets of interrelated courses of action represent a level of stability and durability of artefact-user relations that lies between the script level and the affordance level, thus necessitating a different, role level, analysis.

Affordances, roles, and scripts represent not only different levels of stability but also different levels of abstraction and interrelatedness. Affordances are the most abstract artefact-user relations. They are general possibilities for action with a wide range of possible applications including different artefacts and contexts of use. In contrast, the script of a single-purpose device represents the most concrete and situation-specific artefact-user relation. The meaning of the artefact's technical features and the meaning of the corresponding user activities are largely derived from their contribution to a single course of action within a particular situation. The middle level arises for artefact-user relations that are part of a set of interrelated positions and roles. To the extent that the holders of positions are involved in role relations with the holders of different other positions, they are involved in different situations and their existence and their behaviour becomes less situation-specific. Accordingly, the artefact-user relation will be defined by a number of possibilities for action for different situations of use. However, the range of possible applications is limited by the boundaries of the sociotechnical constellation described by the respective set of interrelated positions. Within these boundaries, a common basic understanding of the artefact-user relation can emerge.

For an illustration of how the concepts of script, role, and affordance support analysing artefact-user interaction at three different levels of stability, abstraction, and interrelatedness, consider once again the egg separator. Separating eggs is interrelated with many other courses of action in the field of cooking. The corresponding actor positions are well integrated. Many of them are rather stable, such as the position of the cook, the stove, the cookware, or the consumer of meals.

However, though separating eggs is a deeply embedded activity, there are different procedures to achieve this. Probably, the most common procedure uses no other devices than the two eggshells between which the egg is tossed after cracking it open. But there are numerous other procedures. Besides the procedure with the bulb-shaped separator described above, there is, for instance, a procedure where a separating device that looks like a small coarse mesh sieve is used. Here, the intended use is to spoon the yolk of an egg cracked into a bowl and to drain the white by lifting the device from the bowl. Each one of these procedures is unstable in the sense that they are easily replaceable by another. Why is that?

The reason is that, first, the differences between these procedures are mainly determined by the script of the respective procedure and, second, these scripts do not substantially affect other positions and role relations within the field of cooking. This is not to say that the actor positions defined by the egg-separating procedures do not come with role expectations towards other positions in the field of cooking. On the contrary! All of these procedures presuppose people as cooks who are skilful enough to crack eggs without damaging the yolks; all of them presuppose that eggs and suitable bowls are at hand; all of them presuppose consumers willing to eat food that contains egg yolk (or egg white), etc. All these role expectations, however, address already existing role behaviours of already established positions in the field of cooking. Thus, the existing network of positions does not have to be significantly adjusted to include one or other of the procedures of egg separation. In turn, this means that the existing network of positions does not contribute to defining the actor positions specific to the different egg-separating procedures. Consequently, the existing network does not stabilise any of them more than any other one. This puts the different egg-separating devices into positions where they are easily replaceable.

In this respect, the position of any of the artefacts serving as egg-separating devices is quite different from, for example, the position of the kitchen bowl. Though it is surely possible for several cooking activities, where one usually uses bowls to use something else, this exchange would not endanger the overall position of the bowl. This is because the bowl's position is stabilised by its roles in many different courses of action and its role relations with several other positions. The kitchen bowl plays a role not only in separating eggs but also, e.g., in mixing ingredients, in serving as dinnerware, or in storing food leftovers in the refrigerator. Thus, removing the kitchen bowl from one or another of these tasks or implementing new scripts, which prescribe additional roles to it, may cause adjustments, but would probably not substantially affect the bowl's position in the kitchen.

Even for people who often cook, it is far from obvious what the intended use of a bulb-shaped egg separator is when they first encounter the device. In contrast, the bowl's property to hold non-solid ingredients such as liquids and powders in place while providing a wide opening allowing manipulation is made use of in many common cooking practices of combining, mixing, and portioning ingredients. Thus, even people who only cook occasionally share a common basic understanding of the intended and other possible uses of bowls.

Moreover, one can reasonably argue that this knowledge is not only shared within the field of cooking but that it is universal knowledge. Consequently, the possibilities for action provided as described by the physical shape of the bowl in combination with common practices of processing non-solid materials and the corresponding know-how are affordances in our analytical framework. To say that bowls afford combining or mixing non-solid materials, thus, is to say that the respective sequences of distributed action and the corresponding artefact-user relations depend neither on a particular script nor on a particular network of positions and role relations. Rather, they depend on the taken-for-grantedness of common knowledge of how to make use of particular physical features of bowls.

In a similar way, the possibilities to suck in and to press out nonsolid materials by releasing or squeezing the rubber ball are affordances of the bulb-shaped egg separator that exist independent of the script and the role relations in which this device is involved. Based on common knowledge about squeezable containers with narrow orifices, such as squeeze bottles or pipettes, unknowing users exploring how to use a bulb-shaped egg separator will eventually conclude that its proper use might somehow include squeezing and releasing the ball. However, this conclusion still leaves countless options open. Affordances can reach a level of abstraction that requires additional field-specific knowledge or knowledge provided by a particular script that relate them to particular contexts of use.

Affordance, role, and script as analytical tools

Affordances, roles, and scripts, as I have presented them in this paper, are meant to serve as analytical tools for investigating artefact-user relations. As analytical tools, they are abstractions, simplifications of an empirical reality, which obviously is more entangled and less well sorted than these concepts reflect. The rationale for using concepts of this kind is to construct pure types of the empirical phenomena under observation, which "compared with actual historical reality [...] are relatively lacking in fullness of concrete content" but "compensate for this disadvantage" in that they "offer a greater precision of concepts" (Weber, 1978 [1922]: 20). In the previous section, for the purpose of demonstrating how the three concepts are complementing each other I chose an empirical example, which in itself is relatively simple and well-sorted. This section provides a few considerations to support the claim that the approach suggested here is also apt for analysing artefact-user interaction within more complex sociotechnical constellations.

A characteristic of more complex settings is that the new technological artefacts involved are rarely developed from scratch but mostly rely somehow on pre-existing technological components, on routines of use established with technological predecessors, and on other more or less given aspects of the social or material world. Thus, not all components of such socio-material ensembles are "in the making" but some of them are "ready made" (Latour, 1987: 1-17; Schulz-Schaeffer, 2008, 146-148). How do all these "preconfigurations" (Hyysalo, 2010: 247) influence the emergence of new patterns of distributed action and how adequate is it then to describe these patterns as scripts?

When new technologies rely on pre-existing components, for instance on off-the-shelf components, they also inherit, as Williams et al. (2005: 118) argue, the scripts inscribed into them. However, though the original intention thus is still inscribed into the design of the re-used components, new layers of meaning will obscure them and they will eventually be forgotten. Consequently, neither the designers, nor the users or the analysts "are in the position to read off these 'imported scripts'" (Williams et al., 2005: 118), which in the opinion of these authors speaks against the usefulness of the script concept. However, the problem raised here looks different when scripts are conceived as patterns of meaning that govern particular distributed actions as a whole and are not inscribed only into the technical components. From this perspective, characteristics of technological (or other) components that reflect prior scripts may relate in different ways to current scripts. They may influence current scripts by making it easier or more difficult to implement them or they may be irrelevant for current scripts.

Take for instance the Ferraris meter, an electromechanical electricity meter, which is still by far the most common electricity meter in German households.[4] The device has an aluminium rotor disc, which via electromagnetic induction is accelerated in proportion to the electricity consumed. For measuring the consumption, the device counts the rotations of the disc. Long ago, the designers of this device decided to make the edge of the rotor disk visible to the users and to provide it with a scale. This technical feature visualises power consumption in real-time, which may have been used in particular ways in the past. But for today's usages the visible scaled rotor disk has become rather irrelevant. Thus, any attempt to derive assumptions about how Ferraris meters are actually used today from this technical feature would be misleading. However, this would pose a problem for the script concept only if one believes that for identifying scripts it is sufficient to look at what is inscribed into technology.

The electromagnetic meter is an ancient component of the power system, a heterogeneous ensemble par excellence (Hughes, 1983, 1987), which has changed considerably, since this kind of meter became part of it. Inscribed into the device is the practise of pricing power consumption based only on the quantity consumed. Meanwhile, other billing scripts have been developed, for example novel tariffs for dynamic pricing, which are expected to lead to considerable energy savings by peak load reduction (Faruqui et al., 2010). However, they prescribe tasks to electricity meters, for which the conventional meters are unfit. Thus, with the main intention to promote energy efficiency (Kochański et al., 2020: 18) the EU has implemented since 2009 a policy to replace these meters by smart metering systems. According to present estimates, 43% of all electricity metering points in the EU-28 will be equipped with smart meters by 2020 (Tounquet and Alaton, 2020: 19-20). However, together with the conventional meters the old billing script is still in place in many European households, being an obstacle for establishing energy saving consumption practices. As this example shows, prior scripts surviving in re-used components may still play an important role and should be taken into in account in analysing current artefact-user interaction.

Another characteristic of heterogeneous ensembles is that the "the trajectories of artifacts become mingled with the trajectories of other artifacts, people, procedures, and so on. The scripts in the artifact become intertwined (added to, contested by) other scripts" (Hyysalo, 2010: 247). This poses the problem of possible differences between the expectations and requirements at artefacts (and other components) that are associated with the respective scripts. Admittedly, the example I used in the previous section did not allow to address this problem sufficiently since it was about an already established set of interrelated actor positions. Role theory, however, is a very suitable concept for analysing how the different scripts within more complex socio-material settings interrelate. With the concept of role conflict and of social mechanisms for dealing with role conflicts (Merton, 1957), it provides useful tools for analysing these issues.

A role conflict occurs, when the holder of a position is confronted with conflicting expectations represented by other actor positions. One of the social mechanisms of dealing with role conflicts is by differences of power of those representing the different expectations (Merton, 1957: 113-114). The fact that Germany lags behind in smart meter installation is in part a result of such differences of power. Smart metering is not only about promoting energy efficiency but also about data protection, privacy, and cybersecurity since smart metering requires electronic data communication between the smart meters, power providers, and users. Defining the respective regulatory framework, however, lies in the power of the national regulatory agencies and not in the power of the European policy makers. Thus, though the technological means and the related use strategies for saving power via smart metering already existed for years, it was not until the end of 2018 that the first smart meter was certified for use in Germany. Only then, the agency responsible for IT security in Germany had specified and approved the quite concrete scripts for the performance of the devices, their operation, and the data transmission that shall ensure the security of smart metering (BSI, 2020). Everybody and everything else had to wait.

Another mechanism for reconciling different expectations (as long as they are not contradictory) is to employ technological (or other) components in different scripts so that they fulfil different expectations at the same time (Pinch and Bijker, 1987: 44-46). An example is the claim raised by European energy market policy, that smart metering not only helps to safe energy but also promotes final customer empowerment by allowing customers "to receive accurate and near real-time feedback on their energy consumption or generation, and to manage their consumption better […] and to lower their electricity bills" (European Union, 2019: 132). Interestingly, not only the same technological components but to some extent also the same patterns of use are claimed to provide the means for both goals. For instance, the same script of dynamic prizing may govern an action that aims at saving energy or at saving money (or at both). Thus, the role analysis has to take into account that a particular role behaviour may satisfy different role expectations simultaneously.

Finally, I want to emphasise that the role concept does not imply a harmonistic view. There are mechanisms of dealing with role conflicts without solving them such as the mechanism, which Merton has described as "[i]nsulation of role-activities from observability by members of the role-set" (Merton, 1957: 114-115). To the extent that the nature of the relationship between particular positions is unknown to the holders of positions with competing role expectations the role conflicts implied remain latent. The layered structure of many complex sociotechnical constellations (Silvast et al., 2018: 5) provides many opportunities for rendering invisible conflicting activities. Just consider how many of all the features of artefacts, which are designed to make manufacturing more efficient, escape the attention of the average customer even when they interfere with some of their expectations of the respective artefact. However, latent conflicts may turn into manifest conflicts at some point in time, which may destabilise a sociotechnical constellation if no other way of dealing with them is found.

Conclusion

A basic understanding in science and technology studies is that technology and society evolve in processes of mutual shaping. Scholars in this field, thus, are in need of relational concepts that help them analyse the co-constitution of technological artefacts and social practices, orientations, and contexts. For some time, ANT (including related approaches) has been the most prolific source of relational concepts of this kind. In recent years, however, the affordance concept has become increasingly popular. In this article, I have shown that the conceptual roots of these relational concepts are different. While the concept of affordance is rooted in the organism-environment relation, the concepts of script and role are respectively rooted in the relation of distributed actions and the relation of distributed actor positions.

These different conceptual roots make the main focus of the three relational concepts different: The affordance concept focuses mainly on the relation between features of artefacts and common properties of users. The main focus of the script concept is on how the contributions of artefacts and users to particular courses of distributed action are negotiated and ensured. The concept of role widens that focus to settings of distributed activities that include more than two actor positions and, consequently, several interrelated scripts and role relations. To sharpen our conceptual tools for describing the interaction between human and material agency, we should make use of these different perspectives; we should employ the concepts of script, role, and affordance to analyse artefact-user relations at three different levels of stability, abstraction, and interrelatedness.

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Notes

1 This extension of the affordance concept is one of the reasons for its frequently lamented lack of conceptual clarity and analytical integrity as well as for its inconsistent use in research (Evans et al., 2017: 36-37; Parchoma, 2014: 360-363).

2 Though many of Norman's examples focus on visual information, he uses the term 'visible' in the broader sense of "being directly perceivable", thus taking into account that "affordances may be perceived using other senses as well" (Gaver, 1991: 82).

3 In addition, there is the practical problem that this distinction severely limits the scope of the affordance concept. As Norman (1999: 42) concedes, it renders the concept inapplicable to most of today's technological artifacts as far as they include digital components that are symbolic and thus knowledge-based (Jucker et al., 2018: 93-95). For obvious reasons, most scholars and practitioners using the affordance concept have ignored this consequence.

4 Cf. https://de.statista.com/statistik/daten/studie/298727/umfrage/verteilung-der-zaehlertechnik-in-deutschen-haushalten/ (accessed on 26 October 2020).

Ialenti Vincent (2020) Deep Time Reckoning: How Future Thinking Can Help Earth Now. Cambridge, MA: MIT Press. 208 pages. ISBN 9780262539265

Antti Silvast

antti.silvast@ntnu.no

Some energy policy choices have implications for decades into the future. Some choices have impacts centuries, tens of thousands, or hundreds of thousands of years from now. How can current planners know what these impacts will be? Vincent Ialenti's book *Deep Time Reckoning: How Future Thinking Can Help Earth Now* examines professionals that forecast far-future geological, hydrological, and ecological events in nuclear waste storage. His fieldsite is in Finland: a country famous for its nuclear power programme and as a host for the world's first anticipated deep geological nuclear waste repository, called Onkalo. This is a disposal option where the spent nuclear fuel is stored deep underground inside the Finnish bedrock. Onkalo is to open in 2023-2024 and contain the nuclear waste during the hundreds of thousands of years to come.

Deep Time Reckoning studies deep time: timescales that concern geological events at much greater than human timescales. Ialenti writes not primarily for an academic treatise but for the educated expert and lay publics. He presents nuclear waste disposal to facilitate learning - i.e. "deep time reckonings". Ialenti deems these reckonings crucial at a moment when societies face a dual crisis: an ecological crisis and a putative intellectual crisis, a "deflation of expertise", which indicates a generalised mistrust of expert authority and knowledge. The Finnish nuclear management expertise and its long perspectives - "the world's most long-sighted experts" (p. xiv) - offers fresh insights in this situation.

The book is empirically vast, including fieldwork that lasted 32 months (2012-2014) and covered 121 informants from nuclear waste management and its public regulation to research, companies, NGOs, and politicians. As an anthropologist, Ialenti adopts the famous maxim of "following the actors" and treats his informants as "humans with dreams, hobbies, anxieties, hopes, frustrations, quirks, passions, gossip, regrets, kindnesses, and opinions" (p. 20). His observations range from offices and seminars to even free time activities (including a family summer cottage). The educational contents include exercises that form a practical toolkit in deep time thinking. The sheer amount of material is and would be impressive for any academic or popular science work.

The book's introduction focuses on the key actors: the Finnish nuclear waste management company Posiva and the radiation and nuclear safety authority STUK. Between them is the Safety Case, a repository safety assessment report that is a precondition for the government-approved construction license for Onkalo. The Safety Case becomes a main topic for the ethnographic analysis, offering a window into the far-future Finland that is produced in the myriad of technical reports that constitute it.

The first empirical chapter examines a key element of the Safety Case: analogy studies, where analogies of various sorts from Finnish prehistory to modern-day glaciers in Greenland are drawn upon to anticipate future Finland. The second chapter moves into computer modelling and explains how multiple computer simulations are integrated to a framework to foresee far-future geological and ecological conditions. Embracing several kinds of uncertainties, these models also have fixed properties – lifestyles and human needs are assumed to stay unaltered far into the future that comments on the anthropological assumptions of these models..

The third chapter examines the topics of "zooming in" and "zooming out": how the Safety Case professionals have to be constantly zooming back and forth between near and far human, ecological, and geological histories in their work. The fourth chapter opens up how the Safety Case changed when its key developer passed away unexpectedly and how his legacy continued to shape the working practices on an everyday basis.

The Conclusion recommends how to embrace deep time based on the findings and the lessons learned. It is followed by a lexicon of key technical and academic terms and notes.

Mixing popularizing and academic arguments, the book contributes to knowledge from various perspectives. As science journalism, the book is an impressive achievement. It explains complex issues of nuclear technology and studies it in an accessible way through the lives of people involved. This presentation teaches much: including the history of Finland, its particular kind of energy sector, the expertise involved in risk management, and nuclear waste issues everywhere. The deep time reckoning lexicon is particularly impressive and has potential to be published on its own.

But Ialenti's findings also align with many main thematic areas in STS and anthropological scholarship, and strengthen them. One is on interdisciplinarity: nuclear waste management constitutes highly interdisciplinary expertise, integrating disciplines and professionals from geologists to biologists, engineers, and metallurgists. Indeed, the far-future anticipation requires a huge amount of teamwork, with the Safety Case experts "working in complex collaborations that, as a whole, exceed any single person's comprehension, yet still somehow work" (p. 19). To examine this knowledge in the making, Ialenti makes a great methodological addition to STS scholarship in "following the actors" holistically as humans. He does not stop his fieldwork in offices and computer modelling, even if these are also of paramount importance for the analysis.

In doing this, however, the work could have taken a few steps further into current expertise scholarship. The deflation of expertise is a powerful critique and Ialenti develops it especially drawing from the United States, where such issues were prominent in the past years and have remained pertinent. The idea produces further insights all over the world, such as in research: like Ialenti's informants, the success of researchers is increasingly measured by meeting productivity goals, rather than their expertise per se (pp. 34-35). This is another example of deflating the expertise of the professional studied.

But some STS scholars could still conceptualize experts and expertise in a slightly different manner. Ialenti seems to liken expertise to authorized knowledge and its production. This is a valid definition but may pay less attention to recent STS themes: such as counter-expertise, the multifocality of expertise, and its dispersed and relational nature (Åkerman et al. 2020). While the book is nuanced within the nuclear sector and its own knowledge disputes, it indicates this gap when it comes to describing publics and their knowledge about experts.

In several points, Finns are claimed to show relatively strong trust or even admiration of expertise, engineers, and natural scientists. In others, this argument is inverted: Finns also oppose expertise e.g. in antinuclear demonstrations or during economic crises that experts could not foresee. But this conclusion feels too binary: either Finns trust in the experts or oppose them, "the embrace (in experts) had both promises and perils" (p.30). This binary probably sidesteps a more complex situation: such as the polarized mix of trust and distrust, moral responsibilities, and perceived risk and benefits that local publics in Finland have associated with Onkalo (Litmanen et al., 2010). In terms of experts, multifocal expertise and different epistemic claims are involved in these arguments that do not quite conform to the accept/reject dichotomy.

At one point, Ialenti observes that "most Finns I met saw the Olkiluoto repository as a pragmatic solution to an unfortunate problem" (p. 35). This contains further ground than dichotomies and opens an important issue: how different publics are capable of solving such pragmatic problems in the far future and with what consequences. Studying these public issues could provide an opportunity to continue this work in STS both academically and as concerns interacting with the public.

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De Saille Stevienna, Medvecky Fabien, van Oudheusden Michiel, Albertson Kevin, Amanatidou Effie, Birabi Timothy and Pansera Mario (2020) Responsibility Beyond Growth: A Case for Responsible Stagnation. Bristol: Bristol University Press. 184 Pages. ISBN: 978-1529208177

Ruth Falkenberg
ruth.falkenberg@univie.ac.at

Is it possible to innovate in a socially and environmentally responsible manner within an innovation-for-economic-growth paradigm? This question – which, probably unsurprisingly, the authors at least partially answer with 'no' – sits at the heart of the book by Stevienna de Saille and her colleagues from the *Fourth Quadrant Research Network on Responsible Stagnation*. At a time when has it seemed long impossible to ignore our pressing environmental crises, and when the Covid-19 pandemic has exacerbated global socio-economic inequalities even further, the book represents a highly topical intervention in discussions surrounding Responsible (Research and) Innovation. Questioning the growth paradigm in which these notions are embedded, the book calls for a more fundamental engagement with what it means to innovate responsibly, beyond only enhancing processes of public participation.

The central notion around which the book evolves is that of Responsible Stagnation (RS) – representing the Fourth Quadrant of the Matrix of Responsible Innovation (Guston, 2015), which respectively pairs 'responsibility' and 'irresponsibility' with 'innovation' and 'stagnation'. As the authors highlight, RS should however not be understood as an antithesis to innovation, novelty, or creativity. Instead, the book encourages to re-think limited notions of for-market-innovation and to adopt a broader concept of innovation as circulation-of-novelty.

As outlined in the introduction by Stevienna de Saille, the authors see RS as characterised by five main principles: (1) being a "pool of great ideas" and (2) a "particular configuration for change" in which (3) "ethics matters". Moreover, they describe RS as advocating an approach of (4) "restraint" and of (5) "living gently" – with the earth as well as those that inhabit it. As is already apparent from this characterisation, and something the authors themselves note, the concept of RS is at times not entirely clearly circumscribed and it represents less of a policy framework or a distinct set of propositions but should be understood as an "intellectual space" that initiates conversation in a certain direction.

This intellectual space is laid out in the book as an amalgam of perspectives, reflecting the authors' various disciplinary backgrounds. The second part of the book asks "What is wrong with innovation and growth?". In Chapter 2, Michiel van Oudheusden describes the background to the introduction of the RRI concept in EU policy discourse. He outlines how RRI has emerged in continuation of attempts to include publics in processes of decision making about research and innovation trajectories. As he notes, such an approach is certainly welcome since it does to some extent challenge simplistic linear innovation models; yet, being embedded in an innovation-for-growth paradigm, he sees it as unlikely that RRI will "re-orient STI towards meeting pressing societal challenges and towards engaging with the real needs of broader segments of society" (p.33).

In Chapter 3 Kevin Albertson describes why the authors see for-market-innovation within a growth paradigm as unfit to address social and ecological challenges. Albertson first refutes the assumption that in a free market economy the fruits of innovation will equally benefit everybody, and he illustrates how markets tend to re-inforce socio-economic inequalities and create negative externalities, e.g. in environmental terms. Second, he provides a critique of GDP as the major indicator of economic well-being, highlighting how it is in many ways an inadequate measure for capturing actual prosperity. Albertson however also notes that RS does not aim to do away with markets, for-market-innovation and even economic growth altogether. Rather, RS advocates an a-growth approach that is agnostic towards growth but aims to overcome the sole focus on for-market-innovation and economic growth as measured by GDP.

The book continues by laying out how their alternative, RS, could play out "in the real world" (p.55). In chapter 4 Fabien Medvecky sketches what it would mean to put responsibility instead of growth centre stage in thinking about innovation. Adopting an ethics of care perspective, Medvecky highlights that RS would entail slowing down processes of deliberation to allow all voices to be heard, but also to adopt a more relational perspective focusing on interdependency, embeddedness, and plurality.

In Chapter 5, Effie Amanatidou and George Gritzas turn to the question of how innovation for social needs, instead of innovation primarily aimed at growth, could take shape in practice. After critically reviewing several alternative innovation models such as "frugal, reverse, Jugaad and so on" (p.77), the authors suggest a "Society in Control" notion of innovation and sketch how ethically responsible social innovation could look like both within as well as outside of markets.

Subsequently, Mario Pansera, Keren Naa Abeka Arthus, Andrea Jiminez and Poonam Pandey explore what RS would imply in a global perspective, for different communities in the Global South and the Global North. The authors sketch how innovation has historically been entangled with post-colonial development discourses in highly problematic ways. As they emphasise, also R(R)I and RS are concepts that stem from the Global North and carry distinct notions of participation and responsibility that might not necessarily be feasible in different local contexts. The authors thus caution against a universalistic framing of RS, arguing that it must necessarily mean different things in different contexts and provide a space in which a plurality of ways of knowing and innovating can thrive.

Timothy Birabi, in chapter 7, deals with "challenges facing willing firms" (p.111) – laying out the mechanisms that currently often uphold a dichotomy between the interests of share- and stakeholders and thus make it structurally difficult for firms to innovate in ways that are actually responsible towards society and environment, if this does not maximise investment returns. He then sketches alternatives to this model and presents examples of firms that the authors see as instances of responsible for-market-innovation. Finally, Stevienna de Saille, Fabien Medvecky, and Michiel van Oudheusden conclude the book by recapitulating the contribution of RS to discussions about R(R)I.

Through this amalgam of perspectives, the book offers a multifaceted encouragement to fundamentally reconsider what it means to innovate responsibly. The overall program of the book is thus very valuable. However, it seems that the notion of RS might at times not be an ideal descriptor for the authors' suggestions. The authors see RS as attaching the "fourth wheel" (p.19) back to the cart of innovation – yet, one may ask whether the program is not actually better described as widening and re-thinking the scope of responsible innovation altogether, also since the notion of RS is throughout the book used in a multitude of ways that often depart quite a lot from what one may associate with the term stagnation.

Furthermore, in providing such a broad program, the book leaves several questions to the reader, such as how to rethink the role of the state in a novel innovation paradigm, how to determine a fruitful balance and interaction between innovation for and outside of markets, and, most importantly, what sort of macroeconomic paradigm would be required to sustain all this. While it is explicitly not the authors' aim to provide a comprehensive discussion of such questions, it is nevertheless surprising that an engagement with adjacent currents of thought in which such questions are debated, such as post-or degrowth, is mostly absent from the book. This leads to the volume being more interesting for an audience not yet too familiar with growth-critical discussions. The book, though, will be highly relevant to scholars of STS and critical innovation studies, especially those concerned with R(R)I and interested in fresh perspectives on the subject of socially and environmentally just innovation. It may also be appealing to a wider audience interested in such questions since it introduces concepts and ideas from various fields in an accessible manner.

Lastly, I am left wondering whether the a-growth perspective the authors adopt, though being potentially attractive because it is more acceptable to a wider audience, is not partially inconsistent. As the authors themselves note, living and innovating responsibly within planetary boundaries will require a "necessary reduction in material consumption" (p.134). As highlighted elsewhere (see e.g. Hickel and Kallis, 2020; van den Bergh and Kallis, 2012), if one does not believe that a decoupling of economic growth from environmental pressures is possible, it follows that some sort of economic downscaling will be required, thus necessitating more than simply an agnostic approach to economic growth.

While this is a discussion that, I believe, will stay with us in the future, I do see the book a very useful and relevant starting point for making growth-critical debates more prominent within STS and adjacent fields, where such voices seem to have ben until now rather silent.

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