Computational Biology and the Limits of Shared Vision

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Several studies have focused on the social sharing of visual practices as constitutive of evidence within a domain, while there has been relatively less attention paid to points where the social sharing of practices breaks down, or is resisted. This article argues that a study of both types of cases is necessary in order to gain a better perspective on social sharing of practices, and on what other factors this sharing is dependent upon. The article presents the case of currently emerging inter-disciplinary visual practices in the domain of computational biology, where the sharing of visual practices would be beneficial to the collaborations necessary for the research. Computational biology includes sub-domains where visual practices are coming to be shared across disciplines, and those where this is not occurring, and where the practices of others are resisted. A significant point of difference between these sub-domains is between visualizations that render the output of simulations and those which are images taken during observations using the techniques of microscopy. A crossing over, compromise or sharing of practices relating to these different sub-domains is difficult and often resisted. This resistance needs to be contextualised in a far richer account of the relations between the visual artifacts, the scientists who use them within disciplinary domains, the theoretical and instrumentational outlook of the disciplines in question, and that towards which the science is directed, its domain of study. Social practices alone are not sufficient to account for the shaping of evidence. The philosophy of Merleau-Ponty is introduced as providing an alternative framework for thinking of the complex inter-relations between all of these factors. This

philosophy enables us to think of the inter-constitutive relations between these
different factors, which ultimately define an epistemological and ontological
space in which the object of study itself has an active constitutive role, and in
which the scientist as person and perceiving body within a knowledge domain
is also constituted.

Since the 1980s, several studies of visual perception have persuasively ar-
gued that important aspects of human vision are best accounted for not by
recourse to inner mental representations but rather through socially ob-
servable actions and behaviors (e.g. Lynch 1985, Latour 1986, Lynch
1990, Goodwin 1994, Goodwin 1997, Sharrock & Coulter 1998). While
there are clearly physiological mechanisms required for vision, psychologi-
cal accounts of perception in terms of inner mental representations have
been dislodged from their position as the basic term in the interface be-
tween human beings and their environment and replaced with terms such
as "social practice," and "vernacular intelligibility." The focus for these
theorists in accounting for vision shifts to intersubjective rather than sub-
jective accounts of vision, with demonstrations regarding the ways in
which what is seen within an environment by any one person depending
on the sharing of that environment with others and on the practices for
sharing of the group. That is, members of groups and communities tend
to pick out and perceive the same kinds of entities and processes, and the
same kinds of properties and features. In these accounts, the inner world of
invisible and mystical mental representations is replaced by the outer
world of visible and observable social doings. Often the later philosophy of
Wittgenstein is claimed as a philosophical framework that informs this al-
ternative view, with the notion of language game being interpreted socio-
logically as showing that the meanings of terms, utterances, and "moves"
are determined by social agreement alone. Phenomenology is generally
claimed as another important philosophical framework, in particular the
social phenomenology of Alfred Schutz which focuses on the ways in
which a subjectively shared lifeworld conditions subjective experiences
(Schutz 1973).

The work of Maurice Merleau-Ponty on the phenomenology of percep-
tion is less often explicitly drawn upon in this tradition of social account-
ing for perception but shares many of the same concerns. Merleau-Ponty's
best known work is *The Phenomenology of Perception* (1962) and this work
shares the anti-representationalist and anti-subjectivist approach to vision
sketched out in the previous paragraph. Over the course of the develop-
ment of his thought, Merleau-Ponty was increasingly concerned to dis-
solve the dichotomies between subjective and objective, and between in-
ner and outer, that tend to be a feature of representationalist accounts of

perception by focusing instead on the perceiving body. This shift in focus allows for an understanding of perception in terms of the actions and interactions of subjects within an environment. The extent to which the boundaries between the two are porous, and the fact that these boundaries are by the very same token connectors, is especially clear in the notion of the body schema, which Merleau-Ponty had initially borrowed from neurology and then psychology. The body schema is that through which perceivers place and position themselves relative to other perceivers and to their environments as a whole; it is a type of scaffolding or architecture which is both "of the body" and "of the world" having been traced out through sedimented and ongoing transactions between the two. In works such as *Visible and Invisible* (1968), Merleau-Ponty goes on to further erode the subject-object dichotomy, although in order to do so his philosophy takes an ontological turn. The perceiving body is both subject and object, and, more important, reversibly so: we perceive because we interchangeably play both these roles. However, there is a perceptual blind-spot in that we cannot experience ourselves as both at the same time—as my left hand touches my right, I can experience my left hand as touching and thus as subject of the experience; and I can experience my right hand as touched and thus as object, but there is a lacuna, a gap in the experience of both at the same time.Due to the way in which attention operates in experience, we cannot but swivel from the one to the other without reaching the point of stasis where both are present to awareness. This is the limit beyond which it is impossible to go and still remain within traditional phenomenology. Moving beyond, Merleau-Ponty begins to unfold an ontology around the structures of being that make it possible for the reversibility and intertwinement of subjects and objects of perception, and in *Visible and Invisible* he does this through the notion of flesh. By the term "flesh" Merleau-Ponty means "sensibility in general." "Flesh" expresses the connection that there exists between subjects and objects of perception in the very fact of the capacity to perceive of subjects and the capacity to be perceived of objects. It is described as the element which defines them in relation to each other (Merleau-Ponty 1968, pp. 193–194), and also as the principle which allows the body and its environment to open onto one another—*la déhiscence*—, and for their crossing over or reversal (their crisscrossing)—*le chiasme.* Like the principle which allows two mirrors placed in front of one another to reflect one another without the reflections being "located" in either one of the mirrors, and which can be expressed as reflectibility in general, "flesh" is a feature of neither the subject nor of the object, but denotes the relation between them of *sensibility in general.* "Flesh" as such is not experienced; rather it frames the ways in which the environment "answers" to shared acts of perception. Rather than shared

practices, Merleau-Ponty often speaks of shared modes or "styles" of perception which are produced through people interacting with each other and with their environments (Merleau-Ponty 1968).[1]

Besides the unfinished manuscript of *Visible and Invisible* (1968), when he died Merleau-Ponty also left a large body of notes on the life sciences, where he continued to elaborate the detail of this ontology of flesh. The work that has been published in English under the title *Nature: Course Notes from the Collège de France* (English trans. 2003, French original 1995) as a collection of notes by Merleau-Ponty and of his students, giving a tantalising glimmer of the account which Merleau-Ponty himself might ultimately have put forward. It is particularly interesting for ongoing developments in the different strands of biological research that Merleau-Ponty turned, this time, to biology, just as previously he had turned to neurology, psychology, and psychoanalysis. For Merleau-Ponty in this late work, the ontology of vision and the ontology of nature are indissoluble. It is impossible within the scope of a single paper to show the full significance of Merleau-Ponty's sketched-out thoughts and ideas for vision in general and for vision in the domain of biological seeing in particular. In this paper, I undertake something on a much smaller scale, and that is to show how following Merleau-Ponty down this avenue of thought might be helpful as a way of deepening accounts of perception which focus on the social sharing of practices as constitutive of important aspects of vision, opening these accounts onto the ways in which vision is formed at the interstices between seeing/seen beings and things. It becomes clear that there is a need to push beyond the privileging of shared social practices in the understanding of vision where there is a failure to share visual practices, where sharing is resisted or collapses. At these junctures, it is possible to claim that this is because different groups and cultures have different practices which are not necessarily shared. However, it is also possible to take a different, ontological, route, which is the one I explore in this paper.

This paper focuses first, on accounts of vision in specialised environments, where expertise or specialised professional skills are in use, and second, on issues that emerge when we try to account for the failure to share visual practices that occurs at particular junctures. Computational biology is a domain where there are a variety of visual practices, which sometimes converge and overlap and sometimes compete. An ontological turn is useful as a way of making sense of the resistance to sharing visual practices

1. An even earlier philosophical account is that of Immanuel Kant who hinted at the central role of shared modes of seeing by invoking the *"sensus communis"* or common sense in judgement. Although the argument about the *sensus communis* is to be found in the *Third Critique,* Kant sees it as an argument relevant to knowledge in general, including the perceptual knowledge which he deals with in the *First Critique.* See Carusi 2008.

that can occur in the computational biology arena. The paper explores this claim and shows one way—a Merleau-Pontian way—of "doing" the ontological turn, and what it could offer to philosophical and sociological accounts of vision in science.

The paper begins with a consideration of a paradigmatic paper in the approach to vision in science as a matter of socially shared practices, Charles Goodwin's "Professional Vision" (1994), and raises questions regarding the extent to which an emphasis on socially sharing visual practices obscures the role of objects of vision in specifying the visual situation. Since instances of contested vision present themselves as interesting cases for examining the limits of shared vision, the case study of computational biology is introduced in the second section. Section 2a introduces computational biology as a sprawling area of interdisciplinary research in biology, comprising both early stage collaborations which are often still fraught with difficulties of sharing practices, and mature collaborations where there is a rapprochement between disciplines. The visualizations used are often indicative of the different stages of collaboration, with visualizations in mature collaborations serving significant epistemic functions for researchers across the collaborations, whereas visualizations of early stage collaborations are still embedded firmly in one or other of the different disciplines. Section 2b describes central features of visualizations in established computational biology, while section 2c delves deeper into a consideration of visualizations in early stage computational biology, which in section 2d are related to the context of observational experimental biology. Section 2e considers the types of exchanges across the contexts of the different visual practices in computational biology. The discussion in section 3 once again poses the question regarding why some exchanges seem more available than others, and brings to the fore the very different ontologies of that which is observed in a computational visualization as opposed to that which is observed through a session of microscopial observation, and the way in which this ontology specifies differently not only the ontological nature of the object seen but the act of seeing and the viewer. Merleau-Ponty's notion of the circuit is drawn upon as a way of rethinking the relation between viewers and that which is seen, which allows for an internal relation of co-constitution between them.

Section 1: Shared visions

Social accounts of vision focus on shared practices, modes or styles of vision that enable human perceivers not merely to agree on what they perceive, but to perceive the array of objects, events, and other features that are characteristic of human existence. Several writers have discussed the ways in which scientific seeing is socially mediated, constructed, or consti-

tuted through processes which essentially involve shared practices, styles, or modes of perception. The central message of these accounts is that scientific knowledge, evidence, observation, and perception are contingent on social or communal aspects of science.

Historical, sociological, and philosophical accounts of vision and visual practices in professional and scientific domains have yielded fascinating results detailing the variety of ways in which convergence on ways of seeing is obtained through techniques, activities, practices, routines, and cultures.[2] More rare are accounts which deal with a breakdown in sharing, a lack of agreement or downright disagreement, or an inability to come to see as others do, even when the potential for this would appear to be quite obvious.[3] Goodwin's "Professional Vision" (1994) is an interesting example. In this paper, Goodwin contrasts two contexts in which visual practices play a central role in enabling others to come to see important features of visual artifacts or fields. It is not simply *coming to see* that is at issue, but rather *coming to see as someone else sees,* in one case, the "someone else" being a professor, and in the other, a provider of court testimony. The first case is that of archaeology professors training students to "see" in an archaeological dig; the second is the case where lawyers defending the police officers on trial for the beating of Rodney King use video evidence to convince the jury that the officers' actions did not constitute an abuse. Goodwin sees close analogies between the two cases. He writes: "It would however be quite wrong to treat the selective vision that is so salient in the King trial as a special, deviant case, merely a set of lawyers' tricks designed to distort what would otherwise be a clear, neutral vision of objective events unambiguously visible on the tape. All vision is perspectival and lodged within endogenous communities of practice" (1994, p. 606). The juxtaposition of the King trial with the archaeology professor inducting a student into archaeological field excavation is meant to show the similarities across the two situations, that is that both involve discursive practices which "shape events" within a field (p. 606). Thus, coming to see as someone else sees is not only seeing in the same way (the same mode or

2. The literature on this point is too large to cite. Representative are Lynch & Woolgar 1990, Baigrie 1996, Jones & Galison 1998.

3. Peter Galison's (1997) account of the image and logic traditions of experimental and theoretical physicists is a notable exception, showing how these traditions struggled—and sometimes still struggle—to come to mutual comprehension. His notion of the trading zone as a way for scientists to come to sufficient understanding so that they can at least collaborate in a mutually beneficial way is useful; however, he sees scientists as interacting primarily through language, or gives a linguistic metaphor of their interactions, claiming that scientists in interdisciplinary domains develop pidgins and creoles which allow them to operate in the trading zone. On this theory, ways of seeing are contingent upon trading zone languages, an alternative which cannot be pursued in this paper.

style of seeing), but seeing *what* others in that same endogenous community see. Goodwin stresses this, writing of the use of video evidence in the King trials that "[o]pposing sides in the case used the murky pixels of the same television image to display to the jury incommensurate events: a brutal, savage beating of a man lying helpless on the ground versus careful police response to a dangerous 'PCP-crazed giant' who was argued to be in control of the situation," and that "[a]n archaeologist and a farmer see quite different phenomena in the same patch of dirt." Goodwin writes that the process of socially shaping vision "creates the objects of knowledge that become the insignia of a profession's craft: the theories, artifacts and bodies of expertise that are its special and distinctive domain of competence" (1994, p. 606). He provides a fascinating account of the details of the way in which this is achieved. However, it is an account which ultimately leaves one perplexed as to the nature of these events and objects so powerfully formed by socially shared visual practices. From where does the disagreement over what is seen in the video evidence—which gave rise to two different verdicts at the two trials held—emerge? Is it simply that in one case the jury shared the vision of one set of lawyers, and in the other, they shared that of another set of lawyers? What makes the difference between one or the other (besides the force of rhetoric alone, which is certainly a possibility)? Or did they never come to share the practices of either?

If we consider the kinds of differences between what is seen (a brutal beating or responsible police action; soil for crops or soil hiding archaeological treasures), it is clear that what is seen is not a matter of vision as an isolated act of perception alone, but vision embedded in contexts of actions, interactions, purposes, and motivations. Seeing is not only a matter of shared visual practices, but a range of other complexly related practices, actions, and behaviors. That is, what is seen is always a matter of the whole sphere of human actions. However, implicit in this *socially* contextualized understanding of vision is another important role-player, that is, the things with which humans interact, the objects whereby they seek to fulfill their goals, the physical and natural environments in which they interact as social beings. To use a grammatical metaphor: Actions are something like transitive verbs, in that they need objects, direct or indirect. To overly stress the role of actions is like considering the meaning of a sentence as a matter of the verb alone. This is a bad account of sentence meaning and a truncated account of vision. A shift of focus is necessary for the objects of vision to come back into view and to take their place in the whole visual situation, comprising viewers as well as what they see.

Of course, sociological theories of science have not been slow to per-

ceive this challenge and to try to meet it. There has been a clear shift from the "docile objects" which were the only form in which objects were admitted in accounts of scientific vision, for example, in an early paper of Michael Lynch (1985, p. 43). To name but two interesting developments that give more nuanced accounts of the ontological space in which complex social practices, such as the conduct of science, occur: (1) Actor Network Theory developed by Bruno Latour, John Law, and Michel Callon try to discard the assumption that in any field, some things are subjects and some things are objects, and attempt to adopt a position of neutrality. The extent to which this can be achieved is contentious, but this is a challenging move. And (2) Karen Barad adopts a position of agential realism for human and non-human bodies, with phenomena produced by ontologically inseparable agential intra-actions (Barad 2003).

Merleau-Ponty did not see himself contributing specifically to the understanding of vision in science or other specialised domains. In addition, he did not pause long on the role of technologies in making visible objects of science. Yet his theory is an unplumbed resource for a theory of *vision* in scientific contexts which pushes against an ontology skewed towards social actors.

Section 2: Failures of vision

In the previous section the question was asked, What might be the sources of resistance to socially sharing visual practices? To pursue this line of inquiry it is necessary to focus on cases where practices are not shared and where there is real resistance to their being shared, as much as where they are shared. Emerging shifts in disciplinary boundaries in scientific research are generally good places to look for such examples, since they present examples where visual practices are transferred between domains, or where instead they are not transferred but resisted. This is what is occurring in the shifting boundaries around computational biology, with some sub-domains within computational biology showing a high degree of disciplinary fusion, others showing a lesser degree of fusion but some cooperation, and others instead resisting entering the domain altogether. It is a good field of inquiry for this exploration, since it offers examples of shared visions and contested visions within the same broad field of study. At stake in the different visual practices that converge or diverge in and around computational biology is the reconstitution of the field of biology. The study of the visual practices of and around computational biology, against their broader epistemic backgrounds, allows us to test the limits of sharing visual practices. Accounting for these limits entails drawing on the ontology of vision in these sub-domains.

Section 2a: The research world of Computational Biology

The case described in this paper centres on Computational Biologists and their collaborators. Research in computational biology is progressing across many sub-domains within biology, for example biochemistry and physiology, though at various paces. Computational Biology can be defined as the application of computational methods to the study of biological processes, specifically the methods and techniques of modelling, simulation, and visualization. Computational Biology is closely related to Systems Biology, Mathematical Biology, and increasingly, Synthetic Biology. It is a multi-disciplinary area of study, drawing on the expertise and knowledge of a wide array of people from various disciplines: computer scientists, numerical analysts, engineers, mathematicians, biophysicists and a broad range of the life sciences, physiologists, developmental biologists and others. Computational biology crucially depends upon cross-disciplinary collaborations in order to be successful. This means that it depends on getting on board researchers who have fundamentally different visual and epistemic practices. Visual artifacts play a pivotal role in communicating the epistemic goals and outputs of computational biology across this range of disciplinary diversity. One of the marks of success in establishing collaborations is the use of the same visual artifacts across the different disciplinary groupings. Visualizations of computational simulations are common currency among the members of successful collaborations being used for analyses, in the context of workshops, and in publications. Where instead there is no collaboration, or a greater difficulty of establishing one, there is not a circulation of visual currency. There are several reasons for this of course, including social and institutional factors. However, the nature of visual artifacts used across disciplines, the style of observation with which they are closely connected, and the ontological nature of that which is observed also differ substantially.

Collaborations between experimental biologists and computational biologists, that is, broadly speaking, mathematicians, computer scientists, physicists, and engineers, are what keep the machinery of computational research going. Some collaborations, such as those found in cardiac modelling, are longstanding and have a long history. Others have a far shorter history and for various reasons—including greater biological and mathematical complexity—are more difficult. In this paper, a variety of domains are considered: cardiac modelling, modelling of multicellular systems such as the modelling of tumours, and domains which can be described as qualitative observational biology, such as embryology and cell biology. The case study emerges out of ongoing participative research with members of both groups and includes observations, interviews, participation at workshops and seminars, and my own teaching of interdisciplinary re-

search skills in the context of a doctoral course for computational biology students. A Forum on Scientific Method in Biology was held as part of this study: this was an event to which a small group of biologists "of different persuasions" were invited to discuss with each other what biological method is, with the aim of understanding which methods are considered to be likely candidates for arriving at specifically biological questions and answers (Carusi et al. 2009).[4]

Firstly, something of the general domain of computational biology needs to be understood. Computational biology attempts to model biological processes, usually in the form of ordinary or partial differential equations. The parameterisation of the models (through which they are said to "gain reality") is accomplished by using experimental data. This data can come from published literature; however, ideally it will come from collaborators in experimental biology. Such collaborators are very valuable to computational biologists because they run experiments which supply the kinds of data needed for the construction of the models, and ultimately for their validation. Models are a quantitatively expressed hypothesis regarding the phenomena observed in the experiment and are tested at two points. Firstly, through the simulation, which allows a testing of different solutions of the equations and a refinement of the initial hypothesis concerning the underlying mechanism. Secondly, in the comparison between the output of the simulation and experimental data. Precisely how this comparison is carried out is a crucial point in the validation of the models.

While there is not yet general consensus among the disciplines involved concerning the epistemic status of mathematical models, one view is that mathematical modelling is a hypothesis generating (and therefore predictive) device; a mathematical model, however, is not computational in itself, and is in fact generally worked out using pen and paper.[5] The mathematical model is in the form of differential equations; for it to be solved, numerical analysis and computational algorithms are used to establish the possible numerical values of the derivatives. This is carried out computationally in a simulation. The output of the simulation is visualized, and the observation and analysis of the visualization is a central part of the epistemic practice of the computational biologists.

However, collaborations are not always successful, since computational

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4. The Forum was convened by Annamaria Carusi (philosopher/STS scholar), Blanca Rodriguez (computational biologist) and James Wakefield (cell biologist); other participants were: Brian Goodwin, Denis Noble, Evelyn Fox Keller, Kevin Burrage, Philip Maini, Lynn Margulis, Derek Terrar, Eric Werner, Tim Horder.

5. And in fact, is more likely to be worked out in this very low tech fashion as was discovered in our investigation into the working practices of the Integrative Biology team members (Mascord, Jirotka & Carusi 2007).

biology is not universally accepted as a positive approach and methodology in biological research.[6] The difference in the acceptance and ease of collaboration can result from differences in the "stages of development" of the research program in the various pockets of biological research, although this assumes that all biological sciences will eventually be "mature" enough to develop into the use of full-fledged computational methods. Sometimes, however, non-acceptance can come from deeply held allegiances to particular observational practices. This is what will be discussed in the next few sections.

A brief and necessarily superficial history of heart and cancer modelling goes some way to explaining why collaborations in the former are relatively easier than collaborations in the latter.

Heart modelling has a longer history than cancer modelling, going back to the 1953 Hodgkin and Huxley model of electrical stimulation in excitable cells such as neurons and cardiac myocytes. Denis Noble modified the model so that it could also be used "to describe the long-lasting action and pace-maker potentials of the Purkinje fibres of the heart" (Noble 1962, p. 318). Since then there has been an extensive experimental program generating data to parameterise the models, and to validate them. For a variety of reasons, primarily having to do with (1) differences between whole-organ and cellular processes, (2) the tractability of these processes tractability to understanding through mathematical approaches, and (3) the only relatively recent development of mathematical and computational techniques capable of dealing with the levels of complexity of multicellular biological processes, cancer modelling is at an earlier stage of development.

In the heart modelling community, there is a long-established collaboration between experimental physiologists, mathematicians, and computer scientists. Heart modelling and *in silico* experiments started off with individuals who embodied inter-disciplinarity in that they were experimental physiologists, and therefore already geared towards the quantitative analysis of experimental data. There has been a long line of experimental physiologists who knew enough mathematics to develop their own models. As they conducted the experiments themselves, they were also able to test the models themselves: there was no gap between the development and testing of a model. As the models became more complex, professional mathematicians began to work in the area, and professional computer scientists also began to collaborate when there arose a need to run far more complex algorithms at a greater speed. However, among mathemati-

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6. See for example Horder 2008, and Carusi et al. 2009.

cians, computer scientists, and physiologists there was at least a shared understanding of the need for the collaboration, and a shared commitment to the use of quantitative methods, at least alongside observational methods. This creates a natural collaboration between experimental physiologists and mathematicians/computer scientists. Because of the increasing complexity of the problems and the need to speed up the algorithms, truly interdisciplinary individuals become scarcer, and there is a greater need for distribution of expertise. Cooperation and collaboration are pre-requisites for progress (Welsh et al. 2006), and have been in place in some instances at least since the 1980s.

Whereas computational and mathematical approaches to whole organ processes (such as electrical currents in the heart) require collaboration with physiologists, in the domain of tumour growth and other multicellular processes, collaboration with cell biologists is required. However, cancer modelling and cellular biology generally have a much shorter history of using quantitative techniques, with fewer people able to span the divide between biology and mathematics, in view of the greater complexity of the biological processes and thus also of the mathematics. Thus, it is more difficult to be a fully interdisciplinary practitioner. The corollary of the absence of an overlapping space has been the entrenchment of very different epistemic cultures, one of which is predominantly qualitative, whereas the other is predominantly quantitative. This makes co-operation and collaboration far more difficult to achieve, and the computational biology program in these domains becomes more difficult to establish.

A simplified computational biology research flow is illustrated in the diagram in figure 1. From this, we see that there is a flow between wetlab and drylab, or between laboratory experiments and computationally simulated experiments. Wetlab experiments, carried out by experimental biologists and physiologists, supply data in the form of parameter values. Mathematical models are produced by mathematical biologists in the form of ordinary or partial differential equations and parameterised (there are various approaches to this: bottom up, top down, and middle out; see Brenner et al. 2001; Krohs & Callebaut 2007). The differential equations of the models are solved in the simulations, in a process requiring the input of numerical analysts and computer scientists. The output of the simulation is visualized, and this output is then compared against experimental results.

The research strand devoted to visualizations is a central one in the overall computational biology program, with doctoral dissertations and other research being devoted to it. The process of getting from MRI images, or from histology to whole organ visualizations, is a research chal-
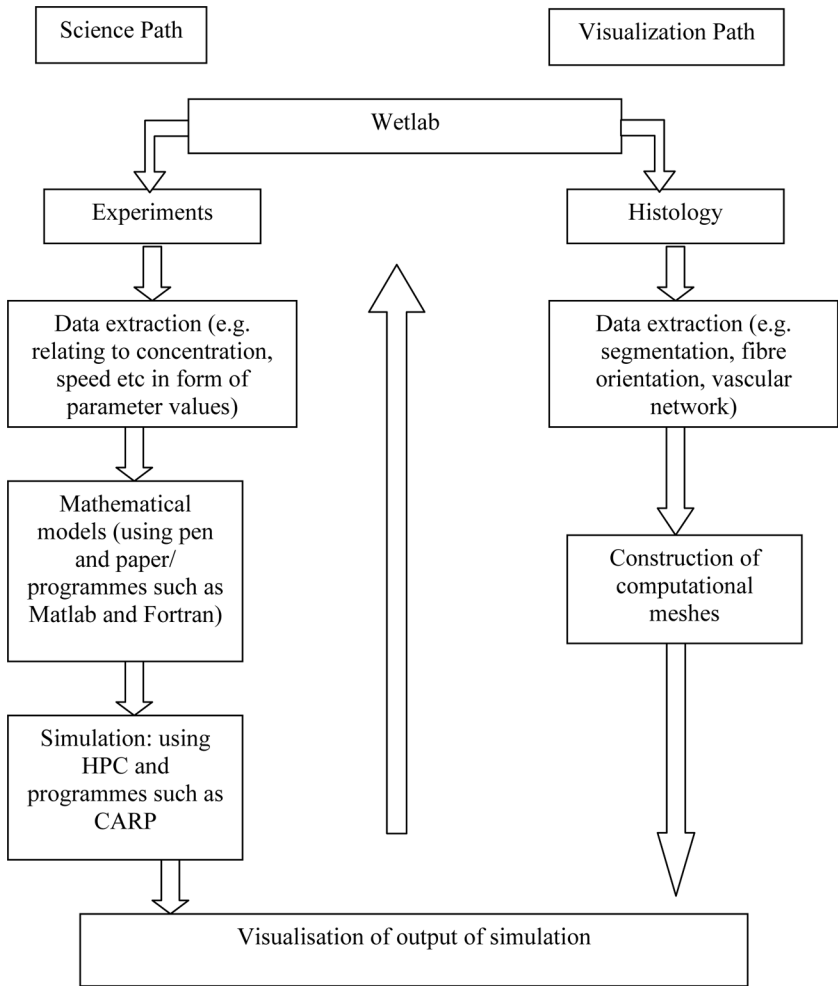
Figure 1: Core stages of a simplified 'bottom up' workflows from wetlab to visualisations and back to wetlab.

lenge in its own right (Burton et al. 2006; Plotkowiak et al. 2008), and is demonstrated in Figure 2.

Some of the problems that emerge occur precisely around the issue of parameter values. This is the output of the wetlab experimental process and a crucial component of the computational biology enterprise. For the mathematicians and computer scientists in the project, it is essential that
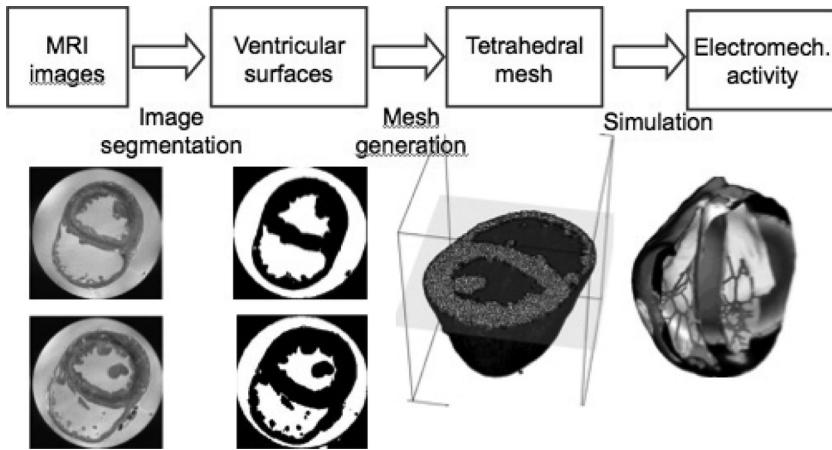
Figure 2: From images to visualisations via Tetrahedral Meshes

the models they develop be parameterised with data derived from wetlab experiments. Data provided by experiments for the modelling process are labelled with the term "parameter values"; however, the term can mean different things for a mathematical computational biologist than it does for a biologist or physiologist. A parameter value is a quantitative entity—it can relate to temporal or spatial properties, or concentrations of molecules, speeds, and so on. Data in the form of parameter values are the only way for models to "gain reality," by which is meant for them to engage with actual physical processes, and to have a chance of being tested and possibly validated.

This is not so for other areas of biology of interest to mathematical biology, such as cancer modelling. Several mathematicians in the study expressed frustration with biologists for not being willing to collaborate, and commented on the difficulty of extracting parameter values from biologists. The difficulties relate to agreeing on which parameters may be of interest, as well as the meaning of the values: for example, whether they are relative or absolute values. For a biologist, it may be sufficient to know that there is a relation between two (or more) properties, and they are interested in the quantity of these only relative to each other. Mathematical biologists, instead, must know "absolute" quantities, concentrations, etc. of these properties: that is, what they are independently of each other. Only with this knowledge, can they begin to pinpoint what they see as the causal mechanisms of the processes they are attempting to understand.

As a computational mathematician put it:

[B]iologists often just want to know that something happens, two proteins interact which regulate the expression of some other protein [. . .] now, that's it, they just want to know that that's the mechanism. We want to know exactly how much of that protein has to be present in order to cause exactly what degree of regulation of this other protein . . . so we can build a model which explains the concentration of the thing that caused the regulation of that other thing [. . .] [by] a series of graphs [and a] model to reproduce those graphs. Biologists don't and can't typically do that. They just know that these proteins are present in the system. They just tell you they're there. No concentrations, rate constants, absolute concentrations in any particular situation. Biologists either can't do that or they won't because it's not what they're interested in.

A very important aspect of this relationship is the fact that the research questions of computational biologists are often geared towards interventional strategies—for example, medical or pharmaceutical interventions. Computational biology for the understanding of cardiac disease is closely associated with pharmaceutical research. For example, the mathematician quoted above went on to say:

I want to be able to quantitatively explain what's going on in terms of molecules, forces, exactly what's going on. And the reason I want to explain that is I want to design new treatments and drugs and only way to do that safely is to be able to say exactly what's going on.

But this involves testing for parameters that may be of little interest to biologists—who are not medical, clinical, or pharmaceutical researchers. This is also found in contexts where collaborations with biologists are sought for engineering purposes, generating many questions about molecular kinematics for the engineers in the collaboration, but for which "it remains far from obvious in what sense they are *biological* questions" (Keller 2002, p. 232). This is a crucial point of difference in the epistemic goals of these disciplines. As a deeper exploration of the role of observation and visualization shows, it is also the point around which there are profoundly different ontological commitments.

The broad domain of computational biology can be divided into two categories: the first category is where there are successful and entrenched collaborations with buy-in from the various disciplines involved; the second is where there are tentative and early stage collaborations, with ongoing attempts to get collaborators on board. Within this second category

there are potential candidates for collaboration among biologists who have not bought into the computational biology program, and who may be sceptical and likely to resist it. For the sake of clarity, I shall label the first category "established CB," the second category "beginning CB," and the sub-category "CB sceptics."

The computational biology program of research is still relatively young, and is still a program in need of validation and acceptance in mainstream science. While it has been successful in garnering funding, with several large funding programs internationally devoted to it,[7] it has not met with universal approval, and sometimes struggles to find collaborators within specific sub-domains. However, because interdisciplinary collaboration is crucial for all aspects of the computational biology program, a great deal of energy is invested into it. Thus, the computational and mathematical researchers, from the doctoral to professorial level, involved in the cancer modelling programs (an example of beginning CB category) are well aware of the need to cultivate potential partners in biology, and spend a great deal of time in PR-type exercises. A number of strategies are used, including interpersonal meetings with potential partners, workshops, and demonstrations geared towards biologists. Biologists are the implicit audience of several journal articles by mathematical modellers describing what modelling can contribute to cancer research, and using several persuasive means to get biologists on board. For example, Byrne et al. (2006) put forward an argument for the use of mathematical models in studying tumour growth which appeals to the shared value of finding a cure for cancer: "In this paper, we review a number of mathematical models that have been developed to describe some of the above aspects of tumour growth. In so doing, we aim to show how mathematical modelling, computation and analysis can generate useful insight into the mechanisms that underpin this devastating disease" (p. 1564).[8]

While important in terms of the overall strategy of computational biology, these discursive strategies remain relatively external to the characteristic practices of the computational and mathematical modellers. Visual persuasive appeals are also used and to some extent the use of visualizations can be seen in a similar vein, as essentially rhetorical communica-

7. With projects such as the European funded Network of Excellence Virtual Physiological Human. http://www.vph-noe.eu/ (accessed 30/05/2011).

8. Other articles along the same lines for mathematical modeling in cancer are Komarova 2005, Van Leeuwen et al. 2007; others with greater emphasis on experimental physiology are Kohl et al. 2000, Hunter & Borg 2003; for the most recent development of the computational biology program towards simulating all organs of the human body: Fenner, Brook et al. 2008.

tions, trying to engage with the biologists' highly visual culture, and trying to share a common visual ground with the biologists.

In this section we have seen some of the context in which research in computational biology is carried out, the need for interdisciplinary collaboration, and some of the obstacles to it stemming from the different histories of the different domains, and their different purposes and goals. In the next section, we turn to visualizations as used within the modelling and simulation enterprise.

Section 2b: Visualizations in the context of established CB

Computational biology in the domain of whole organ modelling is a good example of the established CB category, with convergence sufficient for a collaborative interdisciplinary program of research. The collaboration between experimental physiologists and computational biologists in this category is greatly facilitated by the fact that they share a common vocabulary of parameter values: that is, not only what they mean, but what values it is useful and desirable to test in an experiment. This convergence around parameter values is made possible by the fact that they hold research questions in common, and thus their research is driven by the same interests and purposes—including, for example, seeking collaborations with industry partners such as pharmaceutical companies. The collaboration is kept going through common research projects, dedicated doctoral programs, co-supervision of doctoral students, and the increasing numbers of interdisciplinary researchers. The willingness of experimentalists to supply data for the simulations is a sign of the usefulness of the results of the simulations for their own research questions; indeed, both groups converge on collaborations with external industry partners, in particular pharmaceutical companies which are increasingly interested in using computational biology techniques for drug discovery and testing. There is coordination, but not complete overlap between the different disciplines, for example, since there are still distinct roles for the different disciplines. There is also much disagreement regarding, for example, what a model is, or even what computational biology as a field is, and what exactly it is trying to do. While there is some discussion, there is not disagreement to the extent that it prevents the collaboration from occurring at all.

The key role of visualizations in the computational biology program of research is evident in several ways: the visualizations are prominent in teaching, in workshops, and in publications. The visualizations are also cultural identifiers for computational biology groups, being used as logos, office artworks and the like. Visualizations are described—for example, by their developers—as a powerful communicative tool in conveying complex mathematical ideas in a visually compelling way. However, the visu-

alizations are couched in a dual rhetoric: one which emphasizes their use to communicate with experimentalists and get them on board; the other which emphasizes their epistemic role. They are used for data analysis, as tools of discovery and exploration of the processes under study; they are used for evidence and justification of claims. Visualizations are not an optional add-on to modelling and simulation and they are not an illustration of concepts and statements that are also couched in verbal terms. Rather, they are an integral part of the simulation process. As Winsberg puts it:

> Visualization is by far the most effective means of identifying characteristic features out of complex dynamical data sets, and so it is the most, if not the only, effective means of judging the degree of calibration a simulation enjoys with other data sets and with analytic results. Thus, visualization plays a crucial role in sanctioning as well as in analyzing simulation results. Not only does the epistemology of simulation call upon resources that are empirical, and that come from outside of the theory, it also calls upon the faculties of the observer. (1999, p. 290)

This is a crucial point. *The visualizations are qualitative renderings of a series of quantitative processes.* Their epistemic role, that is their use to validate the whole process of modelling and simulation, is largely based on observation of qualitative features. Not exclusively so, since evidently this is a mode of qualitative observation impregnated by quantitative analysis.[9] However, it is an intriguing fact that a process that is so deeply quantitative ultimately yields qualitative results. The question is what that visual rendering is ultimately of: that is, a question regarding what is seen to be represented, in terms of "content" and ontology.

Let us look at this in greater detail: Physiologists make use of various imaging techniques, including microscopial images, MRIs, and fMRIs, and make extensive use of images in their research. An image, however, is a very different representation from a visualization, since it has an entirely different causal history. Images are not without their algorithmic and mathematical aspect, but they are defined by the role in producing them of light, sound, or other signals emitted from the object imaged. What they are taken to represent is thus relative to the object with which they stand in the appropriate causal relationship. Although visualizations do start off from images, there are a number of intervening steps leading to the construction of a computational mesh and finite element mesh which then serves as the basic geometry for the visualized simulation, in

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9. The fact that qualitative comparison with experimental results is used for validation is also contested and held to be not optimal by some. See for example Parker 2008.

the form of an animated movie, which is at a very distant remove from any images. The movie which is the visualization of the simulation, is itself a rendering of the model in its predictive capacity of processes over time. In the first place, these visualizations play a role in bringing about a shared mode of perception and set of visual practices with epistemic import—as described in section 1, and argued for in the context of computational biology in a previous paper (Carusi 2008). Given that the mathematics is in the first place inaccessible and difficult to comprehend by non-mathematical researchers, the visualizations may be the only direct access the experimentalists have to the ideas and hypotheses being proposed for later testing. In order to facilitate communication with experimentalists, the style of the illustrations was initially derived from textbook illustrations with which physiologists would have been familiar. They play a crucial role in bridging the gap between those who speak the mathematical language, and those who do not—or at least not as proficiently as the dedicated mathematicians. For experimentalists, the context of the visualizations are their experiments, including instruments, laboratory techniques, processes, and so on.

The computational biologists (mathematicians, engineers, physicists) in the collaboration do, in principle, have an alternative point of access to the simulation (the mathematical model itself). However, as pointed out above, even they rely on the visualization for grasping the outcomes of the simulation, which is an initial testing of the model. That is, the simulation has more in it than the mathematical model, and not everything can be read off the model itself—else it would not need the simulation. For the computational biologists, the context of the visualizations is the model and modelling process, parameterisation, the computation required for the running of the simulation, and so on. Set against these very different contexts, the visualizations have different ranges of meaning for modellers and experimentalists. Despite this, the same visualizations are used by both groups in the collaboration, building up a core of shared meanings over the course of ongoing joint research projects. This is shown, for example, in the numerous occasions when the visualizations are used across disciplines. A sample of the resulting visualizations are shown in Figure 3.[10]

However, even though this paradigmatic example of established CB ap-

10. The limitations of black and white publication do not allow a full appreciation of these stills in their context, where for example they could be seen in a context of comparison with other visualizations and with the results of wetlab experiments. See for example the online version of Rodriguez et al, 2005 at <http://circres.ahajournals.org/cgi/content/full/97/2/168> [accessed 16/07/2010] for an excellent example of experiment/simulation comparisons.
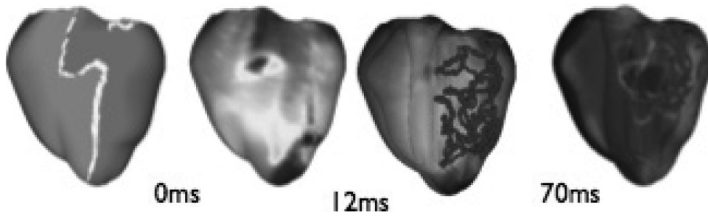
Figure 3: A set of stills from a simulation of electrical activity in the heart.

pears to be one of the sharing of practices around the visualizations, it is not at all clear that they do so because the practices are socially shared, or because of some other feature of the visualizations: such as what they are of or about, their ontology, on which the social sharing of practices is predicated. Let us look at the next category of computational biologists to see whether any clarity can be gained by a consideration of cases where there is a failure of shared vision.

Section 2c: Visualizations in the context of beginning CB

Images for the simulation of multicellular processes, such as tumour growth, are far more problematic, for a variety of reasons, including the fact that in these cases there is no certainty that the geometry of cells and other entities involved in the processes is significant (whereas it is clearly significant in the case of electrical currents across the heart). However, in beginning CB, there have also been attempts to engage with the observational preferences of biologists through visualizations, with mixed results. Visualizations such as that in Figure 4 are an example.[11]

However, unlike the visualizations used for the simulations of heart modelling, these visualizations were still relatively isolated from the rest of the mathematical modelling process for the projects studied. There are a number of reasons for this: the science is at a less developed stage than, for example, cardiac modelling, and this results in visualizations which are less complex, less intrinsic to the process of exploring evidence. This is clear from the fact that in when it comes to cancer modelling, it is still unclear whether the geometry of cells is a relevant factor, and this results in the fact that the spatial properties of the visualization (borrowing from the polygonal shape of epithelial cells) are not necessarily an essential aspect of the visualization for the purposes of rendering the processes at work in the growth of cancer cells. Even from the point of view of scientific workflows, the emphasis in these beginning CB projects is still on the

11. http://web.comlab.ox.ac.uk/chaste/cancer_index.html (accessed 30/05/2011).
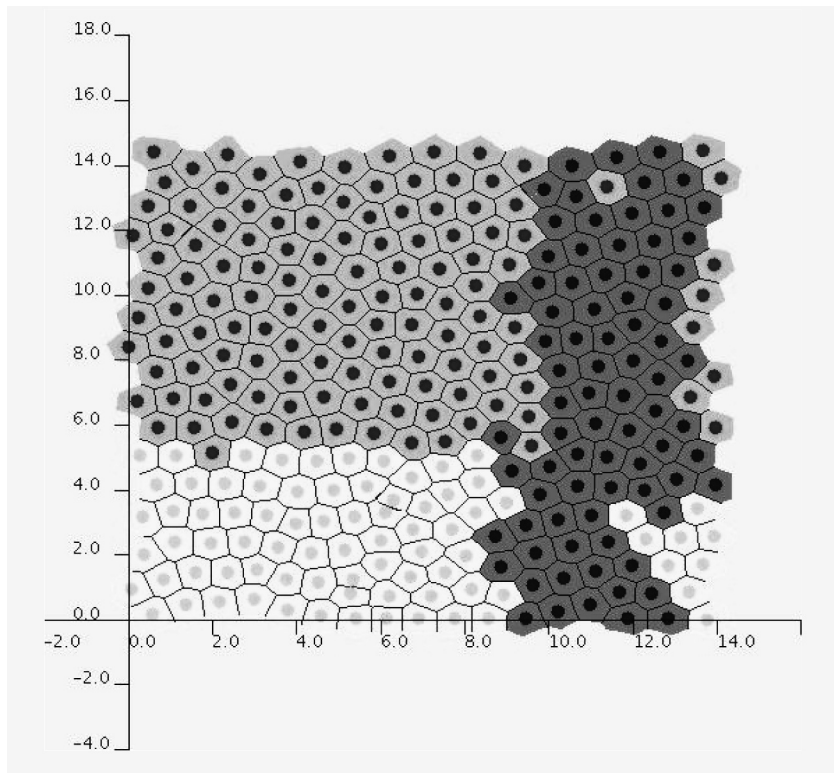
Figure 4: A still from a visualization of the growth of cancer cells 'Snapshot of an intestinal crypt simulation. Here the crypt is modelled on a cylindrical geometry, by performing the simulation on a plane and enforcing periodicity on the left and right edges. Transit cells are shown in yellow, differentiated cells in red, and the blue cells are the progeny of a single cell that was dyed at the beginning of the simulation.' [Notice the adoption of terminology borrowed from microscopy: the use of dyes for tracking cells].

mathematics, which, as an epistemic process, is not computational—indeed, only pen and paper may be necessary for working out the equations of the model. However, with the perceived need to communicate on the biologists' qualitative and observational terms in mind, the style for figure 4 was taken from the diagrams used in cell biology text books. However, in these textbooks, most often such diagrams are seen side by side with microscopial images,[12] and those images, in turn, seen in the context of micro-

12. S. F. Gilbert's *Developmental Biology* (2010) being an excellent example.

scopial practice. The communicative and rhetorical roles of these visualizations are emphasized over their epistemic role. Thus the visualizations are felt to be "for the biologists," since they help to communicate the program of research to the biologists who would otherwise not engage with the mathematics. However, visualizations of this sort are unlikely to find many appreciators in the biological community, and indeed this is clear by the lack of take-up this type of visualization had received in other contexts such as publications or presentations as compared with the cardiac visualizations. They failed to establish an "endogenous community" with socially shared visual practices.

In the previous two sections we have seen that visualizations play a key role in communicating the output of computational simulations. They are sometimes used in a way that cements the collaboration that makes the whole modelling and simulation enterprise possible. However, they sometimes fail to engage biologists and may well be resisted as representations of biological processes. The next section establishes why this is the case by consideration of the role of observation in cell biology.

Section 2d: Observations and images in the context of microscopy and cell biology

The practices around any particular artifact need to be seen in the context of other practices, instruments, and the broader conceptual context of the discipline. There are several important differences between the epistemic practices of biologists and mathematical biologists which have been extensively described by Evelyn Fox Keller in the book *Making Sense of Life.* These differences involve having very different orientations to theory— particularly theory couched in mathematical terms—, than to observation, and as has already been noted, to the goals of biology (Keller 2002). The attitude expressed in the biologist Lynn Margulis's ironic reference to mathematical models as "numerology" (Carusi et al. 2009, p. 32) is shared by many who would not express it in quite this way.

From the point of view of biologists, computational biologists often underestimate the complexity of biological processes—and indeed, of what would be required to conduct the experiments required to build a plausible model. For example, one biologist interviewed commented on a failed collaboration with a computational biologist:

> [The computational biologist said] we need to know the dissociation constant of this protein, reaction diffusion equations, concentrations of proteins in the cell. That sort of stuff I don't have a clue, it would take years to get. More generally, about systems biology as a whole—how do you model networks, pathways, particular pro-

cesses. Seems to me that you need to have these parameters. The concentration of your protein, how fast does it move in the cell, is it in the right location to reach with something downstream of it. And there are really only a very few biological pathways for which data like that are available.

One of the reasons why this type of data is unavailable is the difficulty of abstraction in biological processes. Mathematicians sometimes refer to the background complexity making abstraction difficult as "noise," whereas for some biologists, this is precisely what makes them *biological.* This brings into play the question of modularity and which categories or "modules" are still distinctively biological in character. For example, the self-styled historical biologist, Lynn Margulis, warns against the "Whiteheadian fallacy of misplaced concreteness" (Carusi et al. 2009, p. 27) and Brian Goodwin, an acclaimed mathematical biologist, in his more recent work turned towards a more holistic approach to biology, questioning the abstraction that necessarily goes hand in hand with mathematical models as a general approach for understanding biological processes. In his view, other non-mathematical terms play a significant role in describing biological phenomena:

> Brian: James [. . .] [y]ou used the terms "beauty," "harmony," "grace." Why not add "wholeness" and "health" and "coherence"? Because you can see when microtubule organisation is coherent and when it's disorganised, and when it's disorganised it's a clue to something; some of the components are "funny." Now, that can be quite subtle and I would say that human intuition is bloody good at this kind of detection. (p. 18)

These disagreements are also expressed with respect to the visualizations. Just because qualitative observation is such a key aspect of cell biologists' practice, it is specifically around attempts to engage them through observation that resistance is likely to be met. The failure to establish shared practices around these artifacts stems from that to which visualizations as opposed to microscopy are supposed to give visual access.

Microscopy is the key epistemic instrument for cell biologists' exploration and discovery of their specific research fields, and their observational practices are formed in relation to the microscope to a large extent. The microscope puts the biologist in what is *experienced* as direct contact with tokens of that field in the form of the samples being explored, even though the experience is in fact mediated by the microscope and the theories concerning the laws of physics which are built into it. This difference between what is known about how a microscope works and using a microscope

came out clearly in several interviews, as well as in the *Forum* (Carusi et al., 2009). If there is any instrument that biologists identify with, it is the microscope—in the sense that they could not be *biologists* without a microscope. For example, the quotation below is from a biologist who addressed the question of scientific method in biology first and foremost by describing the ritual of setting up a session with the microscope:

> The affinity of a cell biologist to his microscope shouldn't be underestimated. Although, I understand the physics of how a microscope works, it's not the physics that interests me. It's using the microscope as an extension of my eyes in order to perceive the innermost workings of a cell. In order to do that, I guess it becomes quite ritualistic. So, I take samples, cells, tissues, and I prepare them in a certain way. And then I'll go to my little microscope room, little dark room, pull the curtain, sit down on my seat, and then I'll go through a series of well-trod steps; I'll take the cover off the microscope, I'll put this little light on and that little light on, the x, y stage. And then when I'm comfortable, when I'm feeling "in the zone," I'll put the sample on there and I'll take a very cursory glance around what's on that microscope slide. And instinctively I know whether it's a good sample or a bad sample, whether the fixations worked properly, whether the stains worked properly, whether there are any differences between control and treated sample. (p. 9)

A closer examination reveals that it is not simply the instrument as such; rather, it is the observational situation comprising person-microscope-sample and the entire set of interactions around it that is so strongly emblematic of what it is to be a biologist. And the reason for its being emblematic is the direct access to biological processes that the situation is experienced as affording. Of course, this experience is not as direct, transparent, or unmediated as it appears. Vision is very closely connected with possibilities of action: this is especially strongly brought out in Merleau-Ponty's philosophy of perception. Microscopy is no exception to this. Throughout its history, the microscope has caught the imagination of natural history enthusiasts, promising a means for "exploring the inner labyrinths of nature" (Warner 1982) but it took some time for it to become an instrument for serious scientific pursuit. The turning point occurred when it became possible not only to look through a microscope passively, but to interact with it and with the sample. As Evelyn Fox Keller shows, the *professional* biological gaze through the microscope came into being when the object could be no longer merely be looked at but also manipulated. This is especially important in establishing that what is seen through the microscope is real and not an artifact of the microscope. Keller writes:

> once the microscope was joined with the manual manipulations of
> an experimental biology—marking, cutting and dissecting *under*
> the scope—and the interdependency of hand and eye previously re-
> served for the naked eye was extended into the microscopic realm,
> the microscope became a reliable tool for veridical knowledge.
> (1996, p. 112)

What is considered to be real in both everyday and scientific contexts is
very closely connected to perceptions of causality. The interactions that are
possible with observed samples—isolating one part from others, marking
with dye, separating, extracting, injecting, etc.—and in all instances
watching and following with the eye what happens following each of these
interventions—creates a context in which causality can be *experienced,* in
the first instance, in the very interactions themselves, including the mis-
takes. Ian Hacking's description of learning to observe by active doing, is
vivid:

> The conviction that a particular part of a cell is there as imaged is,
> to say the least, reinforced when, using straightforward physical
> means, you microinject a fluid into just that part of the cell. We see
> the tiny glass needle—a tool that we have ourselves hand crafted
> under the microscope—jerk through the cell wall. We see the lipid
> oozing out of the end of the needle as we gently turn the microme-
> ter screw on a large, thoroughly macroscopic plunger. Blast! Inept
> as I am, I have just burst the cell wall, and must try again on an-
> other specimen. (1983, pp. 189–190)

As Keller points out, this "making real by touch" is the entry way to the
scientific realm of causal efficacy, or "making things real by using them to
effect change in other things we *know* are real" (1996, p. 114). This was
clear in interviews conducted with biologists who use microscopy as their
main instrument, as they stressed the relationship between what is seen
and the techniques used as relevant to a particular experimental hypothe-
ses. For example, the use of fate-tracking (the use of dyes for particular
cells in order to track them through developmental processes) in embryol-
ogy, or the use of micro-scalpels and other instruments to transfer tissue,
and other similar techniques are all involved in the highly skilled hand-
eye coordination required for this form of microscopy.

What counts as real in these observations is a matter of the close inter-
relationship between observing through one kind of technology and ma-
nipulating what is observed, often by means of techniques using other
tools and technologies (micropipettes, microneedles, dyes, labelling,
tracking, etc). The process of vision is not static and inert, and that which

is observed is seen because it is manipulated, becomes visible through the interactions with it.

Compare it for a moment to the visualization that renders the outcome of a simulation. This too is a highly constructed viewing episode made possible by a particular configuration of viewers, instruments, and theories. The computational biologist is as interactive as the biologist with their visualizations: for example, they halt them, adjust parameters and see what happens, alternate views, etc. The visualizations used for computational biology also include social interactions, which is crucial for this highly cooperative and collaborative research area. The visualizations as material artifacts are fully incorporated (in the fullest sense of the word) into research events such as seminars and workshops, where they are displayed, animated by gestures, and used as media for social interaction. Physical interactions can also occur through the computational instrumentation that render the visualizations, or that analyse and process data: the software as well as (or through) the screen, the mouse, and keyboard. And of course, computational biologists experience their visualizations as compelling too.

In this section we have seen that the mode of observations of both those biologists who use microscopy as their main instrument, and those who use visualizations of simulations are highly interactive, involving physical interactions with that which is seen. In the next section we consider what kind of exchanges there are between these two modes of seeing.

Section 2e: Exchanging and sharing modes of seeing

Observation is a key epistemic practice across the groups considered, be it of visualizations of simulations, observations down a microscope, or observation of images: that is, either "stills" from a simulation, or microscopial images. What kinds of exchanges are most common between these? We have seen that in the practice of computational biologists, comparisons are carried out between the visualizations of simulations and experimental outcomes. However, in the case of whole organ modelling they are carried out using a range of different visual artifacts, ranging from 2D graphs to the optical imaging of organs.[13] The computational biologists on whom this research was based admitted to not being able to see anything much down a microscope, and seem to experience it as a passive mode of observation. For example, interviewees claimed not to be able "to do anything with it," such as stop it so that you can look again, or change anything while you're looking. This, of course, does not resonate with the microscopist's experience at all, since their experience is of a highly interactive pro-

13. See for example Rodriguez et al. 2005.

cess, one which seems to involve their whole being and identification as a *biologist.* For a biologist committed to microscopy as a mode of observation, seeing a visualization of a simulation lacks the allure of observed processes unfolding in the process of observation (the double-facedness of the process of observed/observation is essential, and will be returned to in the next section when we consider the ontology of what is seen). Thus when mathematical biologists share with biologists visualizations such as the one in figure 3 depicting the growth of cancer cells as mathematically modelled, in a bid to share with them the qualitative practice with which they are familiar, they are unlikely to find a responsive audience. The visualization simply looks uninteresting in the eyes of biologists who are used to seeing these diagrams alongside microscopial images or in contexts which relate to such images. Crucially, the micoscopial images in their turn, are only fully significant when seen relative to the complex and wholly embodied observations of processes often by means of powerful microscopes, and even more importantly, by means of a whole panoply of techniques and tools which allow them to interact with the sample (Keller 1996).

However, mathematicians too can exhibit a great deal of scepticism regarding the "visualizations" of biologists. For example, biologists sometimes produce animations of biological processes, such as those produced by Drew Berry,[14] or by those to be found on the website of Hans Clevers' Group at the Hubrecht Institute.[15] Typically, these visualizations are produced by means of a combination of observation, microscopy images, hand drawing, and in depth research of current knowledge of the processes depicted. These movies are used mostly for communication and education, and not for actual research. Importantly, their audience can include mathematical biologists (either in training or experienced researchers) in order to give them an idea of what the biological processes they are interested in may "look like." One view expressed was that it is very useful to be able to see the movies since mathematicians working in biology do not have easy access to this kind of process; the movies can help them to understand their models better by putting them in context. Other views (sometimes by the same people) were that the movies were also potentially misleading, in that they make it appear that all the problems have been solved, that there is a complete understanding of the process. Sometimes these animations are labelled as "just cartoons," although even this view often goes

14. Walter and Eliza Hall Institute of Medical Research TV: http://www.wehi.edu.au/education/wehi-tv/ (accessed 30/05/2011).

15. Hans Clevers Group, Hubrecht Institute, Movie Animation: http://www.hubrecht.eu/research/clevers/research.html (accessed 30/05/2011).

hand in hand with a distrust of what is compelling for the eye. A view that is often expressed is that the movies may be misleading in that they are seductive, and may persuade you to "buy into" a particular way of seeing—and therefore understanding—the phenomena. One interviewee likened this to seeing the movie adaptation of a book: the picture you had in your mind may not be the same as in the movie and that may be jarring; but equally, it may override the picture you have in your mind because it is so compelling. It is, in this regard, to be mistrusted.

The question arises as to how biologists themselves view these movies and animations of biological processes. As has been stated, this type of animation is not generally used for exploratory research; however, these animations can allow biologists to recognise, something which in principle, they may be able to observe. Watching the movies can resonate with the experience of observing the processes in question, and in this way, reinforce the attachment that biologists can experience to their observational practice. This relationship might draw upon the historical relationship between microscopial observation and hand-drawn illustrations.

In any particular area of computational biology and the disciplines affiliated to it either through active or potential collaboration, or simply sharing the broad biological domain with it, there will be visual artifacts around which exchange occurs and those instead around which exchange does not occur. No single case study can grasp all of the variations possible. However, in this article I have wanted to show one particular area around which exchange seems to be strongly resisted, and that is in the observational process of microscopy and its related visual artifacts on one hand, and the visualizations of simulations on the other. In this area, there are socially shared visual practices in the groupings around established CB, but this is very patchy and superficial in beginning CB, and does not occur among the CB sceptics, who resist seeing as computational biologists do. To extend the analogy from Goodwin's paper on professional vision, we might say that the established CB group display a similar sharing of practices evident in the case of archaeological instruction (though they are not quite the same since the different disciplines do keep their own practices as well); beginning CB might be likened to the groups constituted by lawyers and members of the jury who were convinced by their demonstrations of visual practices around video evidence (if there were any); and the CB sceptics can be likened to those members of the jury who remained unmoved by these demonstrations. The question is what makes the difference between these groupings? It is not a question that can be answered purely in the terms of socially shared practices, since the question is why do some visual practices come to be shared whereas others do not?

In the observational situations of biology and computational biology there are several structural features in common: a high degree of what we can call "fusion" between observers and instruments directed at processes to be made sense of. The instruments combine those which render a visual artifact to the gaze, and those which enable interaction and manipulation. None of these elements can be isolated from the others in the observation of the process in question: there is nothing to be seen without the intervention of viewers and instruments. The englobing context is that of the broader epistemic and institutional context in which the entire configuration of viewers, instruments, and processes occurs, and from which it gains meaning. There is no innocent eye, there is no neutral instrument, there is no simply given object. There is, however, a fundamental difference in the ontology of what is seen. This is explored in the last section of this paper.

Section 3 Discussion

Gaps, fissures, and breakdowns in sharing visual practices show the limits of what can be accomplished by the social sharing of these practices alone, and show that sharing, where it does occur, is predicated upon a much wider set of factors. Faltering or even failing inter-disciplinary collaborations where there are competing views on the same field are good places to examine these limits. The question "when can practices be shared?" is inter-related with other questions such as: who does the observing, with what activities, techniques, and instruments, in what contexts, and for what purposes? All of these converge on the question: *what* is seen or observed?" The observations of dry lab experiment and wet lab experiment are undoubtedly both constructions, but this still leaves plenty of scope for real differences regarding the epistemology and ontology of the broad field of biology. Whether progress is made in the more intractable sub-domains of computational biology depends on whether the translation can be made between models and simulations on one hand and the type of experimental data obtained through the specific techniques characteristic of that sub-domain on the other. Visual practices—including practices of observation—are a central aspect of these techniques, since they are often one of the primary means through which evidence is constituted within the sub-domain. Evidence interweaves interactions between people, instruments, and the entities observed, be they simulated or "real," creating a web that is at the same time social, instrumentational, and ontological. When broadened out in this way, it begins to become more apparent why visual practices cannot be shared across some domains without a concomitant change or even sacrifice of the very thing that is felt to define that domain of inquiry: the thing that is studied, about which researchers want to

know and understand more, towards which their inquiry is directed. There is a personal and subjective level in that what is studied and how it is studied defines not only the domain of inquiry, but also oneself as scientist in relation to it. It is this, perhaps, that partly accounts for the resistance to sharing visual practices: not simply because they are different ways of doing, but because they change the domain of inquiry, and in so doing, also potentially change the researcher, or make a demand for change at a very deep level of the way that researchers experience their own identity in a domain. However, researchers do change disciplines, so this must remain a partial answer.

Between the biological sciences considered in this paper, a central difference in the nature of the visualizations, images, and microscopial observations is the ontology of that towards which the scientists' visual practices are directed. No doubt in both cases, vision is not of a "pure" object—whatever that might be—but involves constructs of practices, instruments, and theories. In fact, the visual objects—that which is seen in and through the instrumentation of visualization and of microscopy—are both models, since the samples observed through microscopy are generally of animal models, such as drosophila, sea urchins, or worms. But the way in which they are constructed, the history that gave rise to them, and, importantly, the role of the biological object in them, is substantially different. In the process of modelling and simulation, the biological object is external, at least to the observation of the visualization that renders the process. What is observed in the visualization is the simulated model; it functions, like a concretisation of an otherwise abstract entity. It is not dissimilar to Kant's proposal that metaphors and other images "body forth" or make concrete an abstract idea, and are, for that very reason, an object of aesthetic pleasure—and hence, to continue in an un-Kantian way—of attachment and identification.[16] The biological object or process which is modelled acts as an external constraint; it is an external validator or data provider, not that which is seen or embodied in the visualization. What is compelling in the visualizations is the perceptual form of a mathematical idea.

What is observed through the microscope is instead a biological object—not an unadulterated pure biological object, but biological nonetheless, in the form of cells and components of cells, tissues, and organisms. The biological is not external to the process of observation. Rather, the

16. In the production of aesthetic ideas, genius "attempts with the aid of an imagination which emulates the display of reason in its attainment of a maximum, to body forth to sense [rational ideas] with a completeness of which nature affords no parallel" (Kant 1952, p. 176–7). Aesthetic ideas serve "as a substitute for logical presentation" for rational ideas (1952:177).

unfolding of the processes observed are closely related to the process of observation, through the interactions between observer, observed, and instrument that occurs throughout the manipulations through which the observed becomes visible. The blurring of the boundaries between viewer and viewed is captured very well by the exchange between Barbara McClintock and Marcus Rhoade reported in Keller's biography: To his remark that he marvels at what she is able to see when she looks at a cell down a microscope, she responded: "Well you know, when I look at a cell I get down in that cell and look around" (2003, p. 69).

In both cases, what is seen, what the visual experience is of, is that which emerges through an ongoing series of interactions in which the observer and the observed are both participants, in the sense that the shape of the interaction comes from both. So does the ontology of the interaction come from both: what it is of or about, biological process or mathematical idea, emanates from the whole interaction and not from one side constituting or constructing the other.

In the *Phenomenology of Perception* (1962) in a section dealing with "The Thing and the Natural World," Merleau-Ponty put forward a framework for thinking of the relation between perceivers and "the thing in the natural world" as one of *inter-constitution.* Merleau-Ponty wrote of the relation between perceiver and perceived as one of communication and dialogue, where each completes the other. In this relationship, the objects of science are far from being "docile objects" (Lynch 1985, p. 43). Natural objects become objects for us through a certain organisation and arrangement of their sensible aspects (they have, as Merleau-Ponty put it in *Visible and Invisible* (1968) an opening onto subjects, as subjects have to objects, in a reversible relation of intertwinement), but at the same time we cannot ever know them entirely—they can always surprise us. Even more, we might say that scientific vision offers an experience like Klee's, of objects that "look back" and with which the eye and hand are interwoven: "The eye [. . .] is *that which* has been moved by some impact of the world, which it then restores to the visible through the traces of a hand" (1993, p. 127). In this relationship with natural objects, the moves we make in the dialogue are completed by the object and vice versa.

> [E]very perception is a communication or communion, the taking up or completion by us of some extraneous intention or, on the other hand, the complete expression outside ourselves of our perceptual powers and a coition, so to speak, of body with things. (Merleau-Ponty1962, p. 320)

Perceiving things is in a sense to live them, to have them lodged within oneself in one's orientation towards the world:

> In order to perceive things, we need to live them . . . To "live" a
> thing is not to coincide with it . . . Our problem, therefore, be-
> comes clear. The perceiving subject must, without relinquishing his
> place and his point of view, and in the opacity of sensation, reach
> out towards things to which he has, in advance, no key, and for
> which he nevertheless carries within himself the project, and open
> himself to an absolute Other which he is making ready in the
> depths of his being. (p. 325–6)[17]

For Merleau-Ponty, the freedom of the subject (or even of socially grouped
subjects) to make their world is not infinite. Herein lies his disagreement
with Sartre for whom freedom is absolute. For Merleau-Ponty this
amounts to a blindness or to a seeing only of the self, which is the same
thing. He uses the metaphor of the circuit in his further eroding of the
subject-object distinction in the Notes on Nature. The two hands, touch-
ing and touched, are in a circuit: if one were to trace that circuit, one
would not be able to tell where one leaves behind the touching and enters
the touched. The relation between them is furthermore one of immanence:
the touching and the touched are touching and touched in relation to one
another. Similarly with vision: seers and seen are in a circuit. One would
want to say, in science, but also many other domains, that it is a circuit en-
crusted with technologies (a point that also needs much elaboration). The
point to emphasize though, is that these circuits of observer and observed
demarcate profoundly different ontologies. Visualized mathematical-com-
putational ideas are an entirely different ontological species from observed
biological entities in virtue of the scope of observers by whose interactions
(actions, proactions, reactions) they have been coaxed into visibility, and
by the constraints and possibilities of visibility and interaction for which
they allow. There can be no meaningful sharing of visual practices across
fundamentally different ontologies of vision. Rather, practices are shared
when ontologies of vision themselves become inter-related: when experi-

17. The details of the implications of this account—intended for basic experience and
not for science as such—cannot be broached here (See Rouse 2005). Of course it makes a
difference that for Merleau-Ponty, science is a "second-order expression" relative to a "basic
experience of the world," although "second-order" here does not mean that it is any less in-
tensely lived. On the contrary, if anything, we might suppose that scientists live the rela-
tion to their objects and artifacts more intensely than basic experience, and that the experi-
ence of being in a dialogue with these, of interrogating them with ones eyes and other
senses, and through one's instruments more is keenly felt. At any rate, there is no reason to
think that the ontology of these domains is a diminished version of the ontology of basic
experience. There is also a particular significance to the biological sciences, as is evident in
Merleau-Ponty's elaboration of his ontology of the flesh in his thoughts on the life sciences,
in *Nature* (1995). See also Hansen 2005.

mental practices yielding observational and image data and computational visualizations of simulations are oriented one towards the other. In these cases, the visibility of the experimental is interdependent with the computational, since experimental visibility answers to the computational mode of probing, and the computational visualization models itself upon the experimental mode of seeing. They come to be in a circuit of visibility themselves. But the forging of these circuits does not come without ontological cost, including a cost to observers in giving up a certain relationship to an object of vision, to a certain way of being for themselves as much as for that upon which they gaze. This must not be underestimated. It is not just that a particular perspective on an object is lost or gained, it is that the object—and with it, the entire mesh of inter-relationships through which it is manifested, including the being or identification of the observer—is lost or gained.

Conclusion

Within the bounds of this paper, it is not possible to delve further into the ontology of computational simulations and visualizations on one hand and of microscopial observations on the other. A fuller account is necessary. My aim has been to show that the sociality of visual practices—the fact of their being shared by communities—is not sufficient to account for what is seen through those practices, and that this is particularly evident when there is a resistance to come to see as others do. Merleau-Ponty offers, for vision, and potentially for the whole range of sensory openings onto our environments, an ontology of inter-constitution of parts, where each participates in the being of the other, or in the becoming phenomenon of the other. Drawing upon this theory, an alternative framework has been offered according to which scientific viewers and scientific objects inter-constitute each other and in this inter-constitution, define a mode of vision with a specific ontology, and a specific becoming of seer and seen. The ontology identifies at one stroke the scientist qua scientist and that which they observe (what it is to be a biologist of a certain kind is to observe in a certain way things of a certain kind). It is only through a framework, like Merleau-Ponty's, which allows for this more complex comprehension of the ontologies of vision that we can begin to grasp the complexities of current scientific visual practices. It is also a framework which places constraints on how much it is possible to tell about the so-called constitution of objects from manifested social practices among scientists or other communities. What is needed are new methodologies for probing the circuits in which ontologies become manifest.

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