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Martin Li	03/18/2024
Name	*Today's Date*

Dimensions of Disciplinarity

By

M. Shane Li

Mark Risjord

Advisor

Department of Philosophy

Mark Risjord

Advisor

Aminah Hasan-Birdwell

Committee Member

Astrid Prinz

Committee Member

2024

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By

M. Shane Li

Mark Risjord

Advisor

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Abstract

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By M. Shane Li

I situate a framework for a Darwinian understanding of scientific practice by drawing from perspectival realism, philosophy of interdisciplinarity, and philosophy of biology. I consider the role of institutional stakeholders in scientific practice, and I lay out a population-level view of scientific knowledge production that is compatible with realism towards science. I supplement this account with qualitative data from interviews with members of an interdisciplinary organization and draw out implications for science policy.

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Introduction

In this honors thesis, I bring together issues from perspectivism, interdisciplinarity, sociology of science, and evolutionary epistemology. Evolutionary epistemology of theories attempts to use selectionist or Darwinian explanations to make sense of the development of science and other epistemic communities (Bradie and Harms, 2023). I attempt to create a conceptual framework for the productive use of Darwinian explanations (or, the *Darwinian stance*) in scientific practice, and I draw from perspectivism in philosophy of science to do this. I also include empirical data from a qualitative case study of an interdisciplinary organization and use the Darwinian stance to understand what is epistemically unique about interdisciplinary collaboration compared to normal disciplinary science. Chapter One reviews perspectivism in science as well as introduces the issues in interdisciplinarity I hope to address. I argue that philosophers must contend with institutional stakeholders if they want to be scientific realists. In Chapter Two, I build on the perspectivist lens and introduce a framework of Darwinian perspectivism to understand the population-level role of institutional stakeholders in science. I utilize this framework to address some issues in neuroscience and scientific realism. In Chapter Three, I introduce the data from a pilot ethnography and in Chapter Four I use this data to continue to flesh out Darwinian perspectivism and discuss issues related to reproducers and individuals.

The project I am after is in making room for an epistemological realism about science. This means that this honors thesis is not interested in whether the objects science posits actually exist (metaphysical realism), but whether we are on solid epistemological grounds to judge the claims that science makes as true. This thesis is also not an outright defense of epistemological realism about science. I assume it and try to find the role that interdisciplinarity plays in the realist picture of science (Chapter One). I borrow from the conceptual tool set of philosophy of biology and evolutionary epistemology, and I layout a framework for a defense of scientific realism in a population-level view of scientific (Chapter Two and Four).

Selectionists in philosophy of science try to employ the Darwinian stance to explain issues in philosophy of science, but they have been met with a variety of criticisms. I point the reader to Wray (2010) and Renzi & Napolitano (2011) for a detailed review. I argue that previous selectionists either (1) relied on oversimplified notions of selection or (2) applied the stance too broadly. In this section, I will focus on the accounts developed by Bas Van Fraassen in *The Scientific Image* (1980) and David Hull in *Science and Selection* (2001).

Van Fraasen (1980) mentions the Darwinian stance to defend his account of anti-realism and empirical adequacy in science. Van Fraasen employs the Darwinian stance to explain why, without belief in realism, we can expect that scientists can latch on to regularities in the world and in the lab. He argues that a “Darwinist” and not an “intentional” explanation is what is needed to explain this feature of modern science (Van Fraasen, 1980, 39). Because of a selection process like Darwinian evolution, we can be sure that science will generally be able to isolate regularities in the world because empirically adequate theories will be selected for. However, he does nothing more than point in the direction of a Darwinian stance, saying no more than "any scientific theory is born into a life of fierce competition, a jungle red in tooth and claw. Only the successful theories survive" (Van Fraasen, 1980, 40). He believes selection has a role to play in the survival of scientific theories, but how this selection is to be carried out is not explained. Van Fraasen's use of selectionism is brief, and he has faced criticism for being too vague (Wray, 2010). What aspects of theories are being selected for and in what environment the selection plays out are left as open questions.

If we are going to satisfactorily apply the Darwinian stance, we must be more specific about how we are using it. David Hull (2001) uses the Darwinian stance to give an account of conceptual development in science. With his background in philosophy of biology, his invocation of Darwin is more developed than Van Fraasen's. He argues that the necessary elements of a selection process are entities that act as replicators and entities that act as interactors. This distinction is similar to (but not identical with) the genotype and phenotype in mammalian biology – replicators reproduce themselves and interactors engage with the environment. While evolutionary biology has very specific entities in mind when they label something a replicator or interactor, Hull casts the net broadly for scientific selection. For him, replicators are the sum of all scientific communication: papers, conversations, presentations and any other means scientists may communicate ideas. The interactors are the scientists themselves who test these concepts in the laboratory. Concepts get replicated through diffusion in the scientific community and are selected by experimentation. His account is difficult to defend, and I refer the reader to Bradie (1990), Rosenberg (1992), and Grantham (1994) for criticism and discussion.

For our purposes, I will point out two defects that I want to avoid. For one, Hull wants to use the stance to explain the conceptual and theoretical development of science, period. He believes selection processes can occur anywhere during the process of scientific investigation, and so an account of selection in science must be able to explain selection occurring anywhere in science. I believe this is much too ambitious a requirement and limit myself to fitting Darwinian explanations to a specified portion of scientific investigation (I spend time on this in Chapter 2). In addition, Hull believes that scientists are the ones doing the selecting. When scientific concepts enter the Darwinian environment, their fitness is determined by a community of scientists in “competition and cooperation” with each other (Hull, 2001, 101). Concepts are rewarded or rejected by epistemic communities based on internal epistemic standards. However, in actual scientific practice, selection often comes from outside the scientific community. From research grants, deans, and pharmaceutical companies – scientific success is awarded by a body consisting of more than just other scientists. A successful Darwinian stance will have to address non-epistemic selection in science, and Hull does not attempt to do so. I begin this thesis by arguing why non-epistemic factors in science are important in Chapter One.

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Chapter One: Perspectivism, Realism, and Interdisciplinarity

Abstract

Perspectival realists claim realism while admitting that all scientific representations depend on the perspectives of scientists. Massimi (2018) offers conditions that perspectival representations must meet to keep realism. Fagan (2020) points out that while perspectivism can contribute to understanding interdisciplinarity, Massimi's conditions are incompatible with many successful interdisciplinary projects. Fagan adjusts the conditions to bridge perspectival realism and interdisciplinarity. I argue this is a misstep. There is a deep tension between the two that neither Massimi nor Fagan addresses. In order to move past the tension, I argue that a population-level view of science is needed and introduce the concept of non-scientific stakeholders to allow perspectival realism and interdisciplinarity to blend.

Section I: Perspectivism and Realism

Scientific perspectives are "points of view" toward phenomena and have particular affordances (theories, methods, questions, acceptable explanations, and epistemic values) that describe the phenomena from that particular point of view. Perspectival realism is a type of realism in philosophy of science that directly grapples with the implications of scientific knowledge only ever coming from situated scientific perspectives (Giere, 2006). This contrasts with traditional metaphysical realist views about scientific knowledge that claim that our scientific knowledge gets at perspectival-independent facts about the external world. For the traditional realist, neuroscientists learn facts about the brain that are true whether or not "neuroscience" is an actual scientific discipline. A perspectival realism would contend that even though all our scientific knowledge is dependent on the scientific perspectives that generated that knowledge, we can still be confident that our scientific knowledge is *true* and not just the result of arbitrary social norms and practices. In this way, perspectival realism sets itself apart from relativism and occupies a social epistemological stake in realism (Longino, 2002). For perspectival realists, neuroscientific facts are true *because*, and not *in spite*, they come from the scientific perspective taken up by neuroscientists.

Perspectival realism has encountered criticism for either being indistinguishable from traditional realism (Chakravartty 2010; 2017) or for not being able to secure realism about science at all (Morrison, 2011). Massimi (2018) attempts to respond to these worries and claims that perspectival realism is a distinct type of realism about science. She holds that realism can be seriously maintained even if all our scientific knowledge is situated and perspectival in nature. This is a striking claim. If our scientific representations only ever get at a phenomenon from various situated perspectives, then why should we believe that they get at reality in a substantial way? Indeed, Massimi (2018) asks: “What is it like to be true within a scientific perspective? Is perspectival truth a coherent notion in the balancing act between realism and relativism in science?”

To answer these worries, she articulates what it means for a scientific representation to be perspective-dependent while still offering true enough representations of the world to satisfy the scientific realist. For this to be the case, Massimi requires that (1) the context of assessment of the representation be dependent on the scientific perspective that generated the representations and that (2) the representation also be “assessable from the point of view of other (subsequent or rival) scientific perspectives” (Massimi, 2018, p. 13).

Let us unpack these claims a little more. Massimi distinguishes between a context of use and a context of assessment. The context of *use* is when a scientist can rely on a representation as a starting point for inquiry without justifying or validating that representation. For example, when cognitive scientists wanted to study emotional memory, they can create representations premised on the representation of the hippocampus as necessary for long-term memory formation (hippocampus-as-memory) without having to design an experiment to justify that representation. The context of *assessment* is how a representation gets justified as accurately representing the world. Cognitive neuroscientists studying hippocampal-lesioned patients can validate the hippocampus-as-memory representation (Squire, 2009), as can psychologists analyzing data from fMRI studies (Maguire, 2001) and neurobiologists sticking probes into mice hippocampi (O’Keefe and Dostrovsky, 1971).

Massimi states that the context of assessment of a representation is dependent on the scientific perspective that generated that representation. The *human*-hippocampus-as-memory representation is a representation that only cognitive neuroscientists can validate. This representation used the hippocampus-as-memory representation to generate the new claim that specifically in *humans* the hippocampus is also necessary for long term memory. Neuroscientists studying memory in macaques or rats do not have the conceptual tools to make sense of human lesion or imaging data, and instead have to trust the cognitive neuroscientists who claim that the hippocampus-as-memory is true for humans. They are not expected to be able to *assess* the human-hippocampus-as-memory representation.

But how can we trust the truth of scientific knowledge if scientists can justify their own representation by "their very own lights and standards" (Massimi, 2018, p. 16)? That scientists from different perspectives are able to self-justify their representations is not enough for us to be realists about that representation. Instead, cross-perspectival assessment, where scientists from different perspectives are able to assess each other's representation, is taken to be the second necessary condition of a perspectival realism.

Cross-perspectival assessment poses an interesting puzzle for perspectivists that alludes to the problem of incommensurability pointed out by Kuhn (1962). If assessment is perspective-relative, and those in a different perspective do not have the tools of the original perspective, how is cross-perspectival assessment even possible? Here, Massimi introduces the idea of *standards of performance adequacy*. The standards are the "contextual, perspectival, and pragmatic" features of an representation that must be retained across scientific perspectives (Massimi, 2018, p.14).

The standards can demand either *weak* or *strong* perspectival assessment. A weak cross-perspectival assessment demands that different perspectives using the same representation are using them correctly. A neuroscientist working in monkeys or mice can use evidence from hippocampal lesions in humans without needing to be able to justify those studies – but only if they use the evidence correctly. These standards are made from within the perspective the representation was created and must be met by scientists in any perspective hoping to correctly use that representation.

A stronger form of cross-perspectival assessment demands that the standards themselves are consistent across perspectives. Weak cross-perspectival assessment adjudicates *use* of representation based on the standards of adequacy, while strong assessment adjudicates the standards themselves. If a group of cognitive neuroscientists working with humans find that it is the amygdala that is doing everything that we thought the hippocampus did (human-amygdala-as-memory), then they can be subject to strong assessment by other neuroscientists. The weak standards of performance adequacy of the amygdala-as-memory representation adjudicate when other perspectives could successfully use that representation, but the standards themselves can be challenged with strong standards of performance adequacy. Other neuroscientists loyal to the hippocampus-as-memory representation might disagree by pointing out inconsistencies with their results or by criticizing the methodology of the amygdala-as-memory perspective. This is *strong* perspectival assessment since it adjudicates not the use of representation, but the representation itself and the standards of adequacy that produced that representation.

For Massimi, strong cross-perspectival assessment is a necessary ingredient for perspectivists to call themselves realists:

> "[the standards of performance adequacy] allow us to evaluate the ongoing performance of our scientific knowledge claims across time and perspectival shifts, because we simply do not possess a God's eye view to do that otherwise.
>
> Perspectival truth may well be our best bet of getting things right from a human vantage point—a vantage point we equally share with our historical

> predecessors and contemporary rivals. This is the only vantage point we can legitimately reclaim as our own." (Massimi, 2018, p. 17)

Realism comes not so much from the specific knowledge claims put forward, but instead from the fact that scientific perspectives are in a constant process of using and assessing different representations from a variety of different perspectives. With both perspective-dependent context of assessments and strong cross-perspective assessment, Massimi believes that scientific realism can be defended, and we can say that the claims of scientists from situated perspectives can be true.

Section II: Perspectival realism towards interdisciplinarity

Fagan (2020; 2022) argues that perspectivism in philosophy of science can provide useful conceptual tools to understand the challenges of interdisciplinarity. Philosophy of interdisciplinarity is interested in how interdisciplinary explanations are possible (Mäki, 2016), and insights from philosophers can aid science policy makers in encouraging interdisciplinary collaboration (MacLeod, 2023; Fagan, 2022; Reijula et al., 2023). Furthermore, perspectivism seems likely to be especially useful since there is something intuitively connected between thinking of science in terms of disciplines and in terms of perspectives:

> The notion of perspective rests on a visual metaphor, which maps onto key aspects of scientific modeling. Construction of explanations is one kind of modeling, which differs in its particulars across specializations/perspectives. So, the question becomes: how to integrate, or unify, explanatory models from different perspectives? (Fagan, 2020, p. 42).

Perspectivism starts philosophy of science from the metaphor of situated perspectives offering knowledge claims from their own points of view. Massimi's perspectival realism ties these perspectives together as painting a cohesive and true picture of reality. Massimi's move is the same one that interdisciplinary policy makers are after: bringing a smorgasbord of disciplines together to get at reality in a way that the individual disciplines could not by themselves.

Moreover, a good account of interdisciplinary explanation is necessary for a good account of realism in scientific practice. Interdisciplinarity is where the most important episodes in science often play out: whether it is where the disciplinary sciences come together to affect policy (climate change, nuclear bombs), or where much of the most exciting scientific discoveries are being made (quantum computing, CRISPR Cas-9). Our faith in science is funded by examples like these where previously disconnected disciplines and perspectives in science can unite to cash out real and impactful epistemic innovations.

Yet, at the same time, interdisciplinarity is famously difficult. There might be many more failed interdisciplinary collaborations than there are success stories (Roy et al., 2013; Yegros-Yegros et al., 2015), and scientists often perceive interdisciplinary work as less rigorous and not worth the effort of pursuing (Benson, 1982).

Perspectival realism, by taking seriously the situated nature of scientific knowledge production while still trying to find a way toward a cohesive scientific realism, is in a prime position to answer how interdisciplinary explanations are possible and why they often fail. Past work in interdisciplinary studies, science and technology studies, and philosophy of interdisciplinarity have focused on the institutional (Sá, 2008) or cognitive aspects (MacLeod, 2018) of interdisciplinary work. However, Fagan (2020) sets out a project of blending perspectivism and interdisciplinarity to make sense of the conceptual and social epistemic hurdles preventing successful interdisciplinary collaboration.

How are Massimi's perspectives related to disciplines? Disciplines are usually housed in university departments, discussed in academic journals, and brought together by conferences. There is usually a curriculum for training new researchers, a set of accepted methods and problems, and some exemplary experiments or explanations. Scientific perspectives, in Massimi's terms, include models and other scientific representations, knowledge claims, methods, and methodological-epistemic values. Importantly, like the artistic metaphor that perspectivism shares its name with, scientific perspectives point the practitioner in a certain direction. What distinguishes disciplines from perspectives is that disciplines are institutional and historical in character while perspectives are epistemic. Neuroscience as a *perspective* takes the knowledge, methods, and values from psychologists, biologists, physicists, and more, and guides them toward answering questions about the central and peripheral nervous system. Neuroscience as a *discipline* provides the institutional space for the representation created by the various sub-perspectives about the nervous system to be shared and used by each other. From there, a cohesive understanding of the nervous system can be created, maintained, and developed. Without the (inter)disciplinary backdrop of neuroscience, our knowledge of the brain would be disunited and less usable by other academic disciplines (psychology, biology) or non-academic entities (NIH), medical companies). Without neuroscience conferences, journals, and departments, it would be harder for psychologists, biologists, and medical practitioners to be able to learn to use the representations of—and to integrate their own work into—neuroscience.

Does Massimi's perspectival realism account for the epistemic value of interdisciplinary collaborations? Fagan (2020) argues that it is not a perfect fit. Since interdisciplinary explanations often pull from independent and highly specialized disciplines, it makes it unlikely that collaborating perspectives share the same epistemic and conceptual goals. When interdisciplinary explanation is successfully produced, there is no neat conceptual form that these explanations take. Fagan takes a survey of the different ways perspectival models can successfully contribute to the same representations and argues that Massimi's cross-perspectival assessment does not account for all possible cases. Massimi would require that the collaborating disciplines be able to assess each other for us to be realists about their interdisciplinarity, yet there are many examples of successful interdisciplinary collaboration that occur without mutual assessment (MacLeod and Nersessian, 2018; Grüne-Yanoff, 2016). Strong cross-perspectival assessment requires "enough overlap across perspectives" for the standards to be assessable, while interdisciplinary collaboration can occur between disciplines that do not "share an intellectual lineage" (Fagan, 2020, p. 43). Unless we are prepared to rescind our realism from many successful interdisciplinary collaborations, something about Massimi's articulation ought to be changed.

Fagan proposes conceptual bridge building as an alternative requirement to cross-perspectival assessment (2020; 2022). Successful collaboration requires that practitioners in one perspective understand (1) what the representation from another perspective would contribute to their own research, (2) vaguely how that representation fits into that other perspective, and (3) the details of a bridging concept where the representation from the other perspectives gets attached to their own perspective. In other words, interdisciplinary collaboration requires practitioners to know how they are going to *use* the foreign representation but does not require them to be able to *assess* those foreign representation. Fagan lightens Massimi's requirement of strong cross-perspectival to weak cross-perspectival assessment to be able to account for the varieties of interdisciplinary collaboration.

Fagan believes that Massimi's perspectival realism makes too strong a requirement for interdisciplinary collaborators. Strong cross-perspectival assessment is a rare feature of even successful interdisciplinary explanations, and if we want to be perspectival realists towards these collaborations, this requirement must be dropped or amended. Fagan offers replacing strong cross-perspectival assessment with interdisciplinary bridge building. Perspectival realism towards interdisciplinary collaboration requires: (1) the context of assessment of the interdisciplinary representation be dependent on the scientific perspectives that generated the representation, and that (2) the representation be understandable by each perspective through a process of interdisciplinary bridge building (weak perspectival assessment).

Section III: Problems!

I argue that Fagan sacrifices the realism Massimi is interested in by trying to accommodate interdisciplinarity with conceptual bridge building. Conceptual bridge building is a form of weak cross-perspectival assessment. Weak cross-perspectival assessment by itself is not enough to fund realism towards interdisciplinarity. To give an example, phrenologists were able to make conceptual bridges towards neuroscience, physiology, and psychology. There was plenty of weak cross-perspectival assessment in which the claims of phrenology drew from and were drawn into claims in other disciplines (Jones et al., 2018). This indicates nothing about how successful of a science it was, and the conceptual bridges did not save phrenology from the ungrounded and rotten foundations it was built on. Weak cross-perspectival assessment in the form of interdisciplinary bridge building is not enough for a perspectival realist.

However, there is a deeper divergence between the perspectival realism Massimi articulates and realism in interdisciplinarity explanations. Massimi has in mind existing perspectives being judged by later or rival perspectives, but philosophy of interdisciplinarity is worried about new and developing perspectives being created out of existing perspectives. These different temporalities cause problems for understanding interdisciplinary collaboration through a perspectivist lens.

Interdisciplinary collaboration is using institutional elements like funding, departments, and organizations to wrangle separated perspectives towards an overlapping goal. This can be thought of as creating a *hybrid perspective*. For cognitive science, the methods, values, and representation from philosophy, psychology, linguistics, neuroscience, anthropology, and computer science are integrated towards the goal of understanding cognition (Sloan Foundation, 1978). From this hybrid perspective, novel claims and representation that could not be made by the parent perspective independent of the interdisciplinary context can be assessed and used.

Should cross-perspectival assessment work from parent perspectives towards hybrid perspectives or from hybrid perspectives towards parent perspectives? It could not be the case that the strong cross-perspectival assessors are the parent perspectives. The point of a field like cognitive neuroscience is to create and assess representation that neuroscience cannot. It also cannot be that the hybrid-perspective is expected to assess claims of the parent perspective. An interdisciplinary field is already able to assess many of the claims of its parent's perspective, and it is unclear what retrograde assessment tells us about the new knowledge claims made by the hybrid perspective that we are interested in. The assessor must be someone besides the parent perspectives.

One could argue that the cross-perspectival assessment should only occur after several perspectives can corroborate the claims made by a hybrid perspective. Realism towards an interdisciplinary venture will have to wait until the other disciplines react to the representation that the hybrid perspective puts forth. But this throws the baby out with the bath water. We want to understand how interdisciplinary collaboration is possible. Making this move prevents perspectival realism from contributing to discussions about what type of interdisciplinary ventures *right now* are most likely to be successful and contribute to our scientific truths.

Perspectival realism is in a pickle when brought to interdisciplinary collaboration. The requirements of Massimi's perspectival realism are unmet by many successful collaborations, and Fagan's attempt to refit perspectivism to the task loses track of realism along the way. Interdisciplinary bridge building is no replacement for strong cross-perspectival assessment, and something else must be adjusted for interdisciplinarity to be brought into the perspectival fold.

Section IV: Interdisciplinary Stakeholders

I believe that both accounts are missing an important aspect of interdisciplinary work and that this hampers their attempts to sew interdisciplinarity and perspectivism together. Disciplines are the institutionalized forms of scientific perspectives, and therefore that institutionalization must be accounted for in any attempt to bring realism to interdisciplinarity. Realism must be accounted for in the *institutional stakeholders* of successful interdisciplinary collaboration for there to be realism for the interdisciplinary collaboration. I argue that in interdisciplinary collaboration, the cross-perspectival assessor to satisfy in order to preserve realism is the institutional stakeholder behind that interdisciplinary collaboration.

For realism in interdisciplinary collaborations, the institutional stakeholders behind those collaborating disciplines must share enough epistemic values with the perspectives for realism to be met. For example, interdisciplinary collaborations funded by tobacco companies providing a new claim that smoking is actually “mostly fine” for you are different from CDC and NIH backed interdisciplinary collaborations to produce vaccines. Because the tobacco companies are only interested in *using* the study results rather than *assessing* them, they don’t need to share the perspectival norms and values. The NIH and other government agencies, because they institute strict tests and series of clinical trials, attempt to *assess* the representation from their vaccination production collaborations. They are able to assess the representation because they implement some form of the methodological-epistemic values that the biomedical sciences implement within themselves. We can be realists towards collaborations coming out of the NIH but withhold that realism from collaborations with blatant financial incentives. The difference lies in the assessors – the stakeholders – and if we can trust their strong assessments.

Is this still strong cross-perspectival assessment? One could argue that because the stakeholders are not scientific perspectives themselves, they are not in a suitable epistemic position to perform strong perspectival assessment towards the interdisciplinary representations. Instead, stakeholders are only interested in being able to *use* the interdisciplinary RS. How various bio-chemical perspectives are able to synthesize a new vaccine is not of interest to the NIH, the only thing that matters is whether the vaccine works. As such, the stakeholders are at best assessing the successful use of these representations, and this would only count as weak cross-perspectival assessment – not enough for the perspectival realist.

To the contrary, I argue that this assessment can be strong assessment. It is certainly the case that not every stakeholder behind an interdisciplinary venture performs strong assessment.

Consider tobacco companies that might fund studies about the health effects of smoking. The stakeholder is only interested in using those results to advertise their product and are not seriously interested in the details of the scientific perspectives creating that representation. This is similar to when cognitive neuroscientists use representation from mice neuroscientists to guide their own investigations. They try to correctly use the representation but are not able to able to judge the standards of the mice neuroscientists that created that R_S. Both the tobacco companies and cognitive neuroscience in this case are at most performing weak cross-perspectival assessment.

But stakeholders are also able to perform strong cross-perspectival assessment. When a cross-disciplinary effort is made to produce a vaccine, like the COVID-19 vaccine, the institutional stakeholders behind that effort institutes various tests and clinical trials along the way (National Institutes of Health, 2020). These trials directly assess the safety and efficacy of the vaccines produced from the various bio-chemical perspectives, and therefore they assess the standards of adequacy provided by those perspectives for creating vaccines. While the governments might only want to be able to use those vaccines, the specific agencies and institutions acting as stakeholders can perform strong assessment. This can be compared to rival groups of cognitive neuroscientists comparing the human-amygdala-as-memory representation and the human-hippocampus-as-memory representation. The standards of the perspectives that generate the competing representations come under scrutiny and face strong assessment. The strong assessment can come from both institutional stakeholders and contemporary or rival perspectives.

This is all well and good, but what explanatory resources are being provided by thinking of stakeholders as strong assessors that are able to guarantee realism? I believe that this way of thinking about opens us up to questions about the selection of the institutional stakeholders behind interdisciplinary collaborations. The selection of perspectives is how groups of scientists and the perspectives they create get selected by stakeholders (university departments, grants, and private companies). This stakeholder selection process is a lot more variable and less likely to keep in line with the methodological-epistemic values that science prides itself on. To keep perspectival realism for interdisciplinarity, we need to account for the stakeholders and the non-scientific aspects of scientific inquiry (Figure 1).

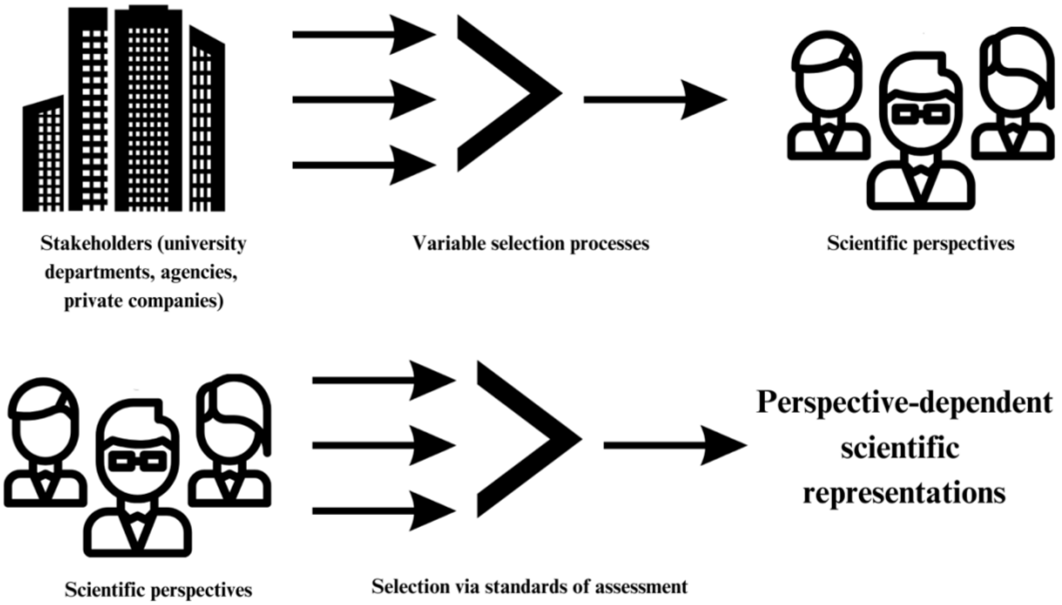

Figure 1. The two levels of epistemic selection in institutional science.

Section V: Concluding Remarks

Just because we are stepping one foot outside of science does not leave philosophers of science bereft of conceptual resources to make sense of what is going on. In the rest of this honors thesis, I introduce and articulate a way of integrating both perspectival realism and the social world surrounding interdisciplinary collaboration into a productive explanatory lens. This lens understands representations and perspectives as two different levels where epistemic selection processes occur. The selection processes of representations are the cross-perspectival assessments, both strong and weak, and reflect how the representation is reproduced within various scientific perspectives. This level has been well attended to by philosophers of science and scientists alike and has been shown to be able to produce spectacular results (Strevens, 2020). The selection of perspectives is much more variable and occurs in the context of university departments, government agencies, and private corporations. How perspectives get chosen by these stakeholders is important for scientific realists, and overlooking this level leaves philosophers of science worse off. I have shown the importance of attending to the selection of perspectives by stakeholders by considering perspectival realism and interdisciplinary collaborations in this paper. In the next chapter, I argue that the Darwinian evolutionary framework developed by Godfrey-Smith (2009) is a useful explanatory strategy for understanding the relation of the population of scientific perspectives and their selection by stakeholders.

In the rest of this honors thesis, I show that a philosophy of interdisciplinarity armed with Darwinian explanations is a robust explanatory framework for understanding realism about the institutional stakeholders behind scientific perspectives. I then focus this framework on issues in philosophy of interdisciplinarity and science policy. I draw from a pilot ethnography performed on the interdisciplinary organization of the Center for the Advancement and Training in Anthropogeny (CARTA) and extract some lessons for funding interdisciplinary collaborations.

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Chapter Two: Darwinian Perspectivism

Abstract

In this article I articulate a form of perspectivism I call Darwinian perspectivism. Perspectivism is the framework within philosophy of science that understands the knowledge generated by science as essentially situated and historically located. Perspectival realists argue that a realism about science can still be kept even if all our scientific knowledge is situated (Massimi, 2022). However, past perspectival realists have focused on how scientific beliefs and justifications created within scientific perspectives can meet the threshold of being scientific truths. This I take as an individual-level explanatory focus. Darwinian perspectivism takes a population-level viewpoint and tries to understand how scientific perspectives can be selected for and reproduced by non-epistemic stakeholders. I believe that this viewpoint is critical for a realist to account for. If we cannot trust how scientific perspectives are funded and rewarded, then our trust in the knowledge produced by those perspectives is threatened. I draw from Godfrey-Smith (2009) to introduce a Darwinian stance where perspectives are understood as populations of disciplines undergoing various selection pressures. These disciplines have different avenues of heredity, variation, and selection, and the Darwinian stance relates these different avenues to a picture of scientific practice that allows for realism. I argue that taking this stance is explanatorily rich for both science policy makers and perspectival realists.

Section I: Realism and Darwinism

Realism in philosophy of science cannot ignore the non-scientific stakeholders behind science. For example, why do we trust studies funded by the NIH and not the studies featured in advertisements by the Tobacco industry? Is this a mere contingent difference? A standard realist approach would say that the pursuit of profits of the tobacco companies cause them to fail to be impartial. With that line, the question about why we trust the backing of the NIH rather than Big Tobacco is answered by appeal to the work of individual scientists. If the scientists don't let non-epistemic values from the stakeholders influence their work, realism is preserved.

However, the NIH shapes whole research programs, and their values are baked into the perspectives that receive funding. When the NIH gives out grants, they are primarily seeking outcomes that would have medical applications. For researchers entering the various bio-chemical or neural sciences, the impact of the NIH is seen in the labs that get funded, the projects that get favored, and the results that are sought.

The form of realism this account contributes to is interested in understanding why science is so epistemically successful. For this goal, we must understand not only the epistemic virtues, methods, questions, and body of knowledge for a group of scientists (henceforth, a scientific *perspective*), but also how the *stakeholders* behind those scientists contribute to that scientific perspective.

Institutional stakeholders do not usually fund individual experiments, but instead populations of scientists. Agencies like the NIH give out millions of dollars of funding for an entire suite of projects within a field. To account for realism in the stakeholders of scientific perspectives, we must look at the level of populations and not individuals. This distinguishes much of the past work on the methodological values of scientific perspectives. The epistemic values of science are social epistemic norms that aid scientists trying to reliably arrive at the truth. Stakeholders are interested in distinguishing fruitful avenues of scientific investigation from unfruitful avenues. We need to understand the difference between individual-level scientific activities and group-level organization of disciplines to show that stakeholders don't undermine realism.

To make sense of stakeholders within a picture of scientific realism, I argue that philosophers of science can make use of the explanatory resources of Darwinian selection. We need to be able to think at both the level of individual scientists and the level of populations of scientific communities that are variously funded by government grants, private donors, and universities. These two levels require different conceptual toolkits.

Is it possible to think of scientific perspectives as exhibiting Darwinian features? There is no reason that the principles of understanding the behavior of organisms in the natural world should not extent extend to scientific practice. As Rouse (2023) writes: "people are organisms whose bodily capacities, practical orientations, and continuing existence are constitutively entangled with the biological environments with which the human lineage coevolved and with which individual humans codevelop in diverse ways" (Rouse, 2023, 14). Rouse gives a naturalistic account of social practice as a uniquely human form of biological niche construction. In the philosophy and sociology of science, some take the naturalistic analogy further and argue for a skeptical view of scientific knowledge (Barnes et al., 1996). However, using the conceptual tools of evolutionary biology does not necessarily entail rejecting realism, and I employ selectionism to bolster a realist picture of science.

Others have used selectionism to address issues in the philosophy of science. However, past selectionists have focused on evolutionary explanations at the level of individual scientists and ignored selection pressure from non-scientific stakeholders (discussed more in Introduction). I point the reader to Wray (2010) and Renzi & Napolitano (2011) for a detailed review. In my approach, I focus on the selection of populations of scientific perspectives instead of individual scientific theories. I also focus on selection from non-epistemic stakeholders instead of selection coming from other scientists.

Understanding scientific practice in the explanatory terms of evolutionary biology is not necessarily inconsistent with scientific realism. Especially since the type of perspectival realism we are after is one that embraces the situated and historical viewpoints from which scientific perspectives operate. In the next section I introduce an account of natural selection that I believe would be conceptually beneficial for philosophers of science.

Section II: Darwin in evolutionary biology

On a general level, Darwinian populations are populations in which something complex and adaptive can arise out of something which is not. The classic examples are populations of biological organisms that acquire intricate phenotypes in response to environmental pressures. A *minimal* Darwinian population is the necessary and sufficient conditions to be subject to selectionist explanation. There must be:

> a collection of causally connected individual things in which there is variation in character, which leads to differences in reproductive output (differences in how much or how quickly individuals reproduce), and which is inherited to some extent. (Godfrey-Smith, 2009, 39)

In a minimal population we might expect some incremental changes, while a *paradigm* population is a population where novelty may arise. Populations of bacteria and organisms have certain features that allow for the creation of novel adaptive traits, while other populations of organisms will fail to develop novel adaptive traits. Paradigm populations typically require stable mechanisms of inheritance, variation, and differential selection. But there is no clear-cut rule for what is and isn't paradigmatic and populations can be more or less Darwinian.

Godfrey-Smith gives a flexible account because Darwinian populations arise in a wide range of biological organisms with inheritance and selection mechanisms that act on several levels and elude clean classification. Importantly, this flexible formulation allows cultural and social phenomena to potentially be paradigmatically Darwinian.

A crucial explanatory feature of this framework is that it allows for selectionist explanations of traits in a population by pointing at characteristics in the structure of the population. These are termed *distribution explanations* by Godfrey-Smith. Some examples include: "since enough generations of finches lived on this island, their beaks became adapted to hunt in this environment," and "if we allow these bacteria to continue to reproduce in an antibiotic environment, eventually they will all gain antibiotic resistance." Going forward, when we take a *Darwinian stance*, we try to explain a phenomenon in selectionist terms. Not everything can be explained with selectionist explanations, just like how not everything can be explained in intentional explanations (Dennett, 1981)., and the explanandum must be a population resembling the paradigm So far, I have only used examples of classic Darwinian concepts like variation and inheritance, but the stance is not restricted from use of extended evolutionary synthesis concepts like speciation and niche construction (Pigliucci and Müller, 2010).

To give discussion of Darwinian populations more structure, Godfrey-Smith uses a n-dimensional space as a metaphor where each dimension is a continuous value of an element of a population that take part in the selection process. For example, H (fidelity of heredity) and V (abundance of variation) are central to biological natural selection. We can imagine the values of H and V for a certain population as continuous between 0(low) and 1(high), and that these values can make up the dimensions of a multidimensional representation (Figure 1). Often, the higher value of a H and V, the more Darwinian the population A population with high heredity and low variation would occupy one space on this graph, and a population with both high heredity and high variation would occupy another. In different selection pressures, the second population might be favored over the first due to producing more varied descendants that might include adaptations to selection pressures. Different characteristics can be added into this spatial representation as further dimensions alongside H and V, and therefore complex and meaningful discussion can be possible about different populations in different selection processes.

Another important dimension in Darwinian populations is the reflection of the intrinsic traits on its overall fitness(S). For example, liver cells have very low S because they will be die after a few generations due to the body's immune system (unless they become cancerous). Are the individuals of the population being selected because they are the fittest and best adapted to the environment, or is it luck? A population with high H, V, and low S will not be as Darwinian as a population with high values in all three. The selection of individuals in the first population will not be dependent on the adaptation of those individuals to the environment, but instead on other factors (such as drift). The population with high S will undergo selection processes that better reflect how well adapted the individuals are to their environment, and that population is therefore more likely to result in the creation of novel adaptive traits.

The final dimension we will consider is C, or the continuity of fitness. This tracks if small changes in the traits of the individuals lead to small changes in the fitness of the individuals. C is low when a small change in phenotype means large changes in fitness. If a population has low C, it is often the case that most variation within a generation will die off. But if it has high C, small changes in variation will lead to small changes in fitness. In the metaphor of a fitness landscape (Wright, 1932), high C allows for exploration of the fitness landscape without catastrophic drop offs. A high C fitness landscape is “smooth”, while a low C fitness landscape is “rugged”.

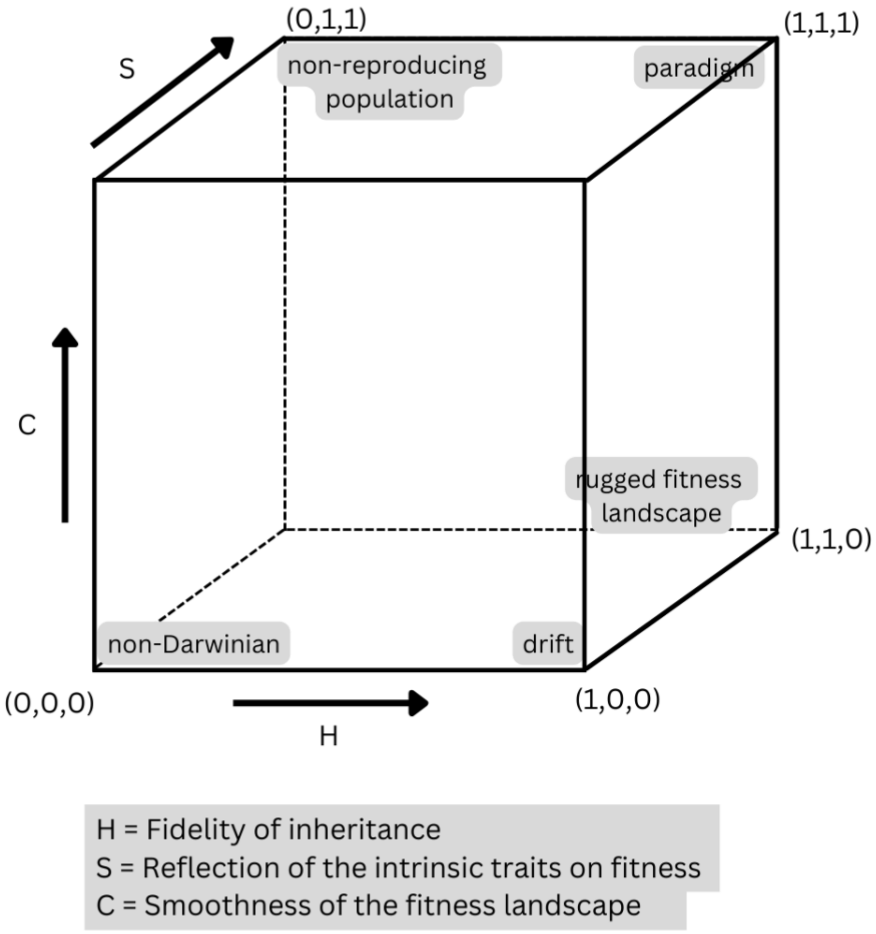

Figure 1 (adapted from Godfrey-Smith, 2009)

Section III: Can we extend selectionist explanations to disciplinary science?

Godfrey-Smith endorses understanding some cultural processes to be Darwinian but cautions paying attention to the level of the cultural variants making up Darwinian. He contrasts individual-level habits, like linguistic utterances, with group-level habits, like institutional organization. Issues arise when cultural variants are chosen in a "complex way," and influences come from different levels to disrupt reproductive lineages (Godfrey-Smith, 2009, 162). Understanding individual-level habits as Darwinian becomes difficult since individual-level habits are often affected by group-level changes. For a cultural practice to be understand as Darwinian, past instances of a practice must have strong "causal responsibility" on future instances (Godfrey-Smith, 2009, 163). If science can be conceptualized in a way that preserves a causal reproductive lineage across generations of individuals, then it can potentially resemble a paradigm Darwinian population.

As mentioned in Section I, my approach focuses on population accounts of selection in science. Selection primarily comes from non-scientific actors hoping to exploit the *epistemic resources* generated by science (although scientists do play a role by reviewing grants, sitting on boards, and advising officials). Realism is challenged by the influence of non-epistemic shareholders since they are not beholden to the internal standards of science. Realists needs to show how stakeholders fit within a selectionist explanation in a way so that they are compatible with (or even supportive of) realism. The Darwinian perspectivism I articulate attempts to do so.

What are epistemic resources and how are they exploited? Scientific perspectives, as Massimi (2022) develops, are effective at exploring the modal space of natural phenomenon. The game of giving and asking for reasons across taking place across generations of scientists allows us to generate stable inferences about varied phenomena such as atomic mass, psychological development, and climate change. Some of these inferences can be exploitable by non-scientists: governments benefit from being able to exploit nuclear power, doctors and parents benefit from understanding human development, and everyone on earth benefits from being able to anticipate the changes climate change will cause. Epistemic resources are the benefits that non-scientists extract from the social epistemic knowledge production of science.

The set of stakeholders is smaller than the set of exploiters of the epistemic products of scientific knowledge. Stakeholders are the ones that materially support the investigations of specific scientific perspectives. The atomic bomb affected the lives of everyone on earth, but it was only the U.S. government that funded (and monitored) the collaboration of scientists that discovered how to build it. While historically, there was a variety of different stakeholders from ancient China to the Renaissance, the most common stakeholder today is the university system with private industry, government agencies, and private donors playing a major role. Stakeholders often have specific goals and requirements behind their support. For example, the NIH funding a COVID-19 vaccine will be more selective of which research groups get funding than a tobacco company just needing a quote for their advertisements (National Institute of Health, 2020).

With these considerations, the shape of the population that I analyze in Darwinian terms is as follows: The population is made up of various scientific perspectives distributed throughout the international university system. Going forward, I take the institutionalized form of scientific perspectives as *disciplines*, which get selected by various stakeholder. Stakeholders select based on the actual or potential epistemic products disciplines can offer. The primary stakeholder for disciplines is the modern university system. Other stakeholders, such as companies and governments, may have their own research interests, but the knowledge they trade in is derived from the knowledge developed at universities. All stakeholders are interested in exploiting epistemic products and fund science based on perceived benefit to them. Universities rake in prestige and tuition paying students, and non-university stakeholders often get useful knowledge (technical or otherwise).

Section IV: How the university of system creates the conditions for Darwinian science

In the previous section I talked about how Godfrey-Smith’s conceptualization must meet strict standards to be applicable to cultural practices. I argue that the organization of scientific perspectives into institutionally embedded disciplines allows scientific perspectives to meet these standards. The university system allows populations of disciplines to resemble paradigm Darwinian populations.

Necessary for a minimal Darwinian population is a stable mechanism for reproduction (H). The research university model derived 19th century Germany is arranged to support high H. Labs train generations of scientists who can then be recruited by private industries and government bodies (Shepard, 2015). This recruitment allows the knowledge generated within the university to be exploited by non-epistemic stakeholders, while the stakeholders provide funding and jobs for practitioners of scientific disciplines. The reproductive lineages of disciplines are therefore stabilized within universities. This stabilization effect of the university on disciplines can be compared to a ratchet effect in evolutionary biology (Tomasello et al., 1993), or biological niche construction (Sanches de Oliveira et al., 2023). The complicated and technical nature of some disciplinary knowledge forces causal responsibility from past scientific disciplines to the population of subsequent scientific disciplines. This “responsibility” is vital for our Darwinian account to hold, as, without it, reproductive lineages cannot be tracked over time.

A more detailed discussion of reproduction in science can be found in Chapter Four. In any case, the university structure contributes to scientific perspectives' ability to achieve high H and approach paradigmatic Darwinian status.

A requirement of high H is being able to materially sustain itself. A psychology department is cheaper than a particle physics laboratory. The continued existence of scientific perspectives relies on having access to natural phenomena. This tie to the environment connects to the realism since we expect our scientific disciplines to reliably latch onto real phenomena in nature. Access to phenomena varies between disciplines and some will find acquiring the material support more difficult. It is easier to pay for office space and mice than a particle accelerator. The university, by creating the institutional space for disciplines to seek funding from governments and private companies in exchange for providing epistemic resources, helps disciplines that require high material requirements maintain high H.

The social epistemic environment of the university also contributes to the high V value needed for paradigmatic scientific disciplines. Hormio and Reijula (2023) argue that universities are "anarchic" in that their institutional organization optimizes the generation of highly specialized knowledge. No single actor at a university could hope to even conceive of the range of knowledge held by the various departments housed there. Instead, the sum of knowledge of a scientific perspective (a *knowledge field*, in Hormio and Reijula's terms) is dispersed across several universities that communicate via conferences, papers, and more. Hormio and Reijula argue that this institutional structure is optimized when the disciplines within a university are encouraged to be creative. I argue that when scientific perspectives are embedded in an anarchic university system, the disciplines can be said to have high V value. This V is aided by the existence of avenues of communication via conferences, publications, and collaboration. There is ample opportunity for revision to the received scientific perspectives, and conformity across departments is impossible to enforce.

One thing that is important to note is that high H and V are not guaranteed. While the university system creates the opportunity for disciplines to become Darwinian, it does not secure it. A field can fail to attract new graduate students and have a lower H value. A field can also, due to internal dynamics, become insular to new ideas and have a lower V (Zollman 2007; Wu 2023). The Darwinian stance allows us to distinguish populations of disciplines in Darwinian terms. Not every scientific venture is equal, and taking a Darwinian stance allows us an explanatory avenue to make sense of it.

This brings us to the third dimension: S, or how intrinsic the selection is. When the success of a discipline is not based its own internal characteristics, then that discipline has low S. The internal characteristics can be understood as the perspectival standards of assessment of Massimi (2022), or deference to the empirical demands of the iron rule of explanation (Strevens, 2020). S is the traditional aspect of scientific perspectives attended to by philosophers, and it has an important role in this population level picture. Without high S, we cannot be sure that the scientific knowledge being produced is true. The Enlightenment ethos of disinterested knowledge seeking is mythologized at most universities, and systems like tenure help to maintain it. Although certainly not without exceptions, the university setting introduces a culture of knowledge-seeking that contributes to the high S of its disciplines. In addition, systems like peer review and empirical verification standards also support S.

The final Darwinian dimension I will consider is C, or continuity. In evolutionary biology, high C allows for exploration of the fitness landscape. In Darwinian perspectivism, it captures the ability of disciplines to explore the epistemic landscape of a natural phenomenon.

For example, mental disorders are a phenomenon that can be seen at the molecular, genetic, psychological, clinical, and sociological level. During the middle of the 20th century, epistemic resources to treat mental disorders were primarily sought from psychiatrists, since neuroscience did not have the technology and techniques to contribute, and the discipline of psychology was largely working within a behaviorist framework. As the 20th century developed, more and more fields were bridged and neuroscience research in mouse models was able to influence pharmaceutical interventions which aided psychiatric counseling. The disciplines latching onto mental disorders increased their C, and the stakeholders (NIH, pharmaceutical companies) were able to gain epistemic resources as well. However, since one of the primary stakeholders were pharmaceutical companies, disciplines guided their research toward developing pharmacological solutions, while sociological interventions to address the socio-economic causes of mental disorders never had the same importance for stakeholders (Horitz, 2009; Timmermans & Hans, 2008; van Dijk et al., 2016). The ability to explore the epistemic landscape was increased for certain disciplines (neuroscientific, pharmaceutical), while other disciplines were ignored (sociology, economics). This made for an unbalanced increase in C, and therefore a less-than-ideal disciplinary science. An increase in C should bolster our confidence in the knowledge producing capacities of the sciences studying mental disorders. However, the interests of the stakeholders behind this population of disciplines lowers the C and gives pause in being realists about this scientific venture.

There are a couple things I want to point out about the C dimension. As the mental disorder example shows, interdisciplinarity is vital for a high C. When the various disciplines studying one natural phenomenon are conceptually bridge (Fagan, 2022), then our faith in the knowledge produced by those disciplines grows. Keeping a high C is important and difficult since it requires that the often highly specialized and insular scientific perspectives be able to cross disciplinary boundaries and engage with new ideas. The spirit of transdisciplinary that Mittelstrass (2018) elaborates emphasizes this exact point.

A high C also directly aids in discovering epistemic resources. If we have only one perspectival understanding of mental disorders, then the epistemic resources available are also limited. If the only way we know of treating anxiety comes from psychoanalysis, then our solutions will be limited to psychoanalytic solutions. Part of the benefits of high C populations of is exploring the epistemic landscape for epistemic resources. Understanding how the bio-chemical aspects of neural disorders relate to the behavioral manifestations of those disorders in a systematic way will likely reveal additional interventions that pharmaceutical companies and government health agencies will be interested in. High C increases the epistemic resource extractability by exploring the epistemic landscape.

I have shown that the contemporary university system can be understood as supporting populations of scientific disciplines resembling paradigm Darwinian populations (Figure 2). The various dimensions of paradigm Darwinian populations (H, V, C, and S) are often found in successful scientific fields. Furthermore, when disciplines are lacking in some of these dimensions, then they are less likely to be able produce epistemic resources. Therefore, a form of selectionist explanation is possible where we explain the creation of epistemic resources of scientific disciplines by reference to their resemblance to paradigmatic Darwinian populations.

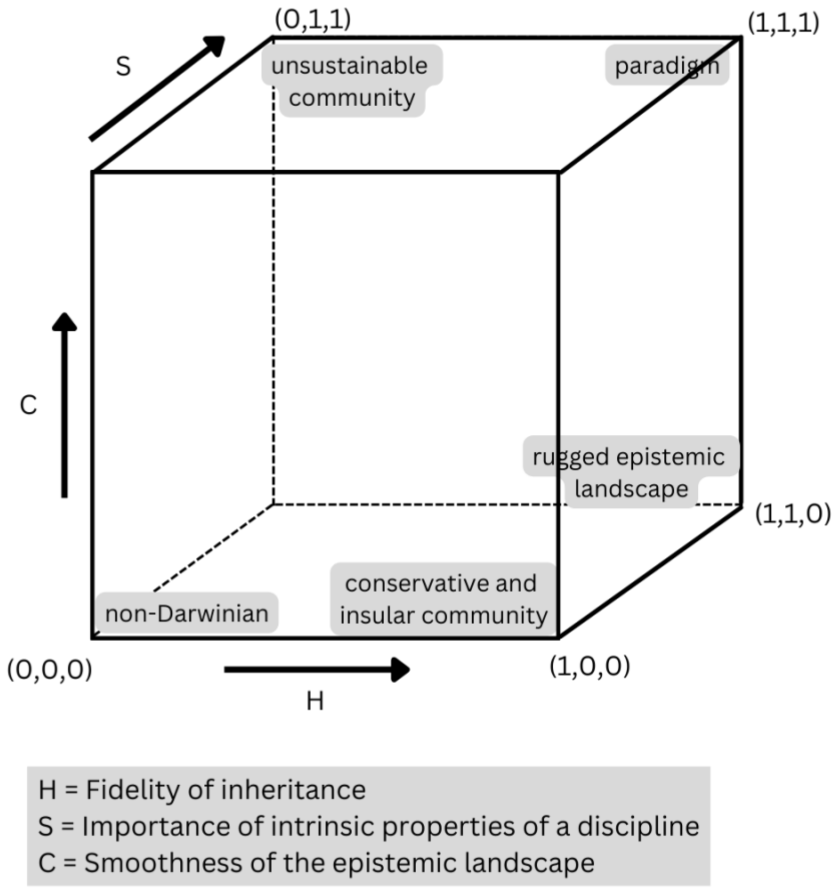

Figure 2: Darwinian space of scientific communities

Section V: What the Darwinian stance does for the scientific realist

Wilfrid Sellars believes one of the principle philosophical questions of contemporary philosophy of science is how institutional science has become the most prolific generator of knowledge out of all other social epistemologies (Sellars, 1963). Realism in philosophy of science is the project of understanding how and why science is so epistemically successful. Perspectival realism emphasizes how the situated nature of scientific knowledge generated by various historical scientific perspectives can create a truthful and robust picture of the natural world. But, so far, philosophers have mainly focused on the individual-level features of science that contribute to its epistemic richness. This account takes a step back and tries to grapple with the group-level features of science.

Massimi (2022) argues that we come to know phenomena in the world because the data scientists gather show stable events that are modally robust and are reliably reidentified across different historically situated scientific perspectives. This opens a “window on reality” that is not possible outside of science. By treating scientific perspectives as a Darwinian population in an environment that includes stakeholders, we can understand how such scientific knowledge can be generated and under what social epistemological conditions it can continue. Being able to identify when the institutional elements are optimal for the creation of epistemic resources is a powerful tool in the hands of philosophers of science and science policy makers.

There is more work to do to flesh out the details of how realism can emerge from scientific practice are. Darwinian perspectivism focuses on the population level aspects of science that are necessary for a realist to account for while glossing over the details of how that realism accounts. The explanation of “since enough generations of finches lived on this island, their beaks became adapted to hunt in this environment” is a Darwinian one, even though it makes no reference of the mechanics of heredity or the specific mutations that occurred causing the phenotypic and development changes that resulted in optimized beaks for hunting different types of prey. Darwinian explanations deal in the level of population and can ignore the causal mechanisms happening at lower levels. My aim in this paper has been to show that a Darwinian framework can make sense of the population-level relationships between scientific perspectives and stakeholders while leaving room for realism.

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Chapter Three: Hybrid Perspectives in Anthropogeny

Abstract

In the Chapter One, I showed that perspectival realists need a way of accounting for stakeholders in disciplinary science. In Chapter Two, I introduced the Darwinian stance and argued that a population level view of scientific disciplines as Darwinian is a useful conceptual tool for philosophers of science. What remains is to account for how reproduction occurs in this picture. The final two chapters tries to understand reproduction in Darwinian science using qualitative data. This chapter introduces the pilot data from an ethnography of the Center for Academic Research and Training in Anthropogeny (CARTA), which is an interdisciplinary organization attached to the field of anthropogeny. Anthropogeny is the study of human origins and leverages methods from across the human sciences to understanding what makes humans unique. This chapter introduces what the perspective of anthropogeny is, and how CARTA operates within that broader field. In the final chapter I show what this ethnography tells us about reproduction in science.

Section I: Introduction

Section Ia. What is Anthropogeny?

Anthropogeny is the scientific field dedicated to the study of human origins and is primarily interested in what differentiates humans from non-humans. However, almost no one uses the term anthropogeny “except for CARTA who found the word and revived it” (Cognitive Scientist1). Related to anthropogeny are the fields of evolutionary biology, biological anthropology, paleontology, and behavioral ecology. For the purposes of this paper, I will use anthropogeny to refer to the discipline of researchers associated with CARTA, but many of these researchers might consider themselves as members of another field (often anthropology, genetics, neuroscience, or cognitive science). What drives anthropogeny, according to CARTA, is “exploring and explaining the origins of the human phenomenon” (CARTA, 2023). To do so, anthropogeny employs traditional paleontological methods like fossil and archaeological evidence, but also a range of other tools from genomics, primatology, linguistics, or whatever else might shine a light on explaining what makes humans unique.

The question of what makes humans distinct has traditionally been answered by paleontologists, archaeologists, and anthropologists who use archaeological and fossil evidence to try to learn about early hominins. However, advancements in genetic and molecular biological techniques have been able to contribute precise empirical evidence for many of the questions asked by traditional anthropogeny fields. Svante Pääbo, winner of the 2022 Nobel Prize and an early affiliate of CARTA, employed genomic evidence to contribute to paleontology as early as 1985 (Nobel Assembly, 2022; Pääbo, 1985). In discussing the likelihood that Neanderthals contributed to modern human DNA, he writes that genetic tools offer much more precision and certainty compared to “discussing the forms, shapes, holes, and ridges of Neanderthal bones” (Pääbo, 2014). Although CARTA has been criticized for just trying to “reinvent the field of behavioral ecology” (Organizer1), it sees an important role for itself in connecting researchers employing a broad range of scientific methodologies to answer the question of what makes humans unique.

CARTA is an interdisciplinary organization which officially began in 2008 but has its origins in informal meetings starting in the 1990s (CARTA, 2023). It is based in the Salk Institute of Biological Studies and the University of California, San Diego (UCSD), and brings together researchers from across disciplines interested in questions about human origins. CARTA’s main activity is hosting bi-annual symposiums, where a panel of researchers are brought in to discuss a topic related to anthropogeny. These range from more traditional academic topics like artificial intelligence and comparative anthropology, to broader topics like the history of body modification and the role of myth in human origins. The symposium last two days: on the first day there is a public event where speakers give a talk about their research to a public audience, and on the second day there is a private event for discussion and mingling between the invited speakers and CARTA graduate students.

The second primary activity of CARTA is a graduate certificate program at UCSD. Graduate students enrolled in PhD programs in the departments of Anthropology, Biomedical Sciences, Biological Sciences, Cognitive Science, Linguistics, Neurosciences, Psychology, and Visual Arts can enroll. The certificate program includes two required classes and allows students to join reading groups and attend a field course in Tanzania.

CARTA features a few other small projects, like a catalogue of human differences (Varki and Gagneux, 2017), but the main function CARTA sees itself as performing is bringing together researchers interested in a shared topic of human differences (CARTA, 2023). In this way, CARTA is more akin to small institutes like Santa Fe Institute that primarily facilitate interaction rather than larger institutes like the Max Planck Institute (MPI) that carry out and fund basic research.

Section Ib. Methods

In order to understand CARTA and its relation to the discipline of anthropogeny, I employed qualitative empirical methods. The use of empirical methods is an important tool in the philosopher's toolkit and provides evidence that can be used to raise questions and provide insights about philosophical issues (Chang, 2012; Machery, 2016). Ethnographies and other qualitative methods can be particularly useful (Latour and Woolgar, 1986; Mansnerus and Wagenknecht, 2016), but philosophers must follow the same empirical standards of rigor attended to by social scientists (Nersessian and MacLeod, 2021). The data I acquired is less than would be demanded for a full ethnography and instead functions as a pilot ethnography. The conclusions generated with them must be qualified, and follow-up work on the issue is recommended. Nevertheless, I believe that the data collected can still guide our understanding of reproduction and individuals within a Darwinian picture of scientific practice.

The ethnography consisted of seven semi-structured interviews and attending one public symposium in person. The interviewees were selected because they were affiliated with CARTA and included current researchers, emeritus professors, current or former graduate students, and one organizer. The symposium I attended was titled "Comparative Anthropogeny" and took place in November 2023. In addition to sitting in the audience of the public presentation on Friday, I also toured the CARTA offices and spoke to organizers and CARTA affiliates. I was unfortunately not allowed to attend the private meeting the following day. The interviews and symposium attendance gave insight to the relation of CARTA with various disciplines, the social environment of CARTA, and the funding structure of CARTA.

However, this was only a pilot, and a full ethnography would be needed to validate these results. Ideally, a full ethnography would include more interviews, and attendance of the private symposia meetings in addition to the public presentations. This pilot only interviewed CARTA affiliates and all the interviewees emphasized the positive experiences with CARTA. A full ethnography would include perspectives from outside CARTA, as well as cynical perspectives from within the organization. In addition, the interviews only sampled from the fields of anthropology, neuroscience, cognitive science and linguistics, while CARTA boasts membership from 148 areas of expertise (CARTA, 2023). A full ethnography should include interviews from other sciences like genetics, molecular biology, and medicine, as well as interviews from other traditional anthropogeny fields like archaeology, paleontology, and primatology. As for the symposium, I only saw CARTA in person during one symposium and was not able to observe the day-to-day activities of the CARTA organizers. A full ethnography would document the year-round logistics and management of CARTA as well as the activities of the graduate certification.

Nevertheless, the data I was able to collect gave me sufficient insight into some of the philosophically relevant aspects of the organization and the field of anthropogeny. In the next four sections, I discuss the interdisciplinary perspective of CARTA (Section II), as well as the social environment (Section III), the training and impact on the field (Section IV), and the funding and stakeholder issues (Section V).

Section II: Perspectival Focus

Anthropogeny is an example of an interdisciplinary perspective. It combines methods, techniques, and questions from a variety of unrelated fields. In this way it is a hybrid perspective as described in Chapter One. CARTA as an organization does not seek to contribute to research in anthropogeny directly, but instead brings researchers from various parent perspectives to consider how they could contribute to the anthropogeny hybrid-perspective.

The main way that this perspectival focus is achieved is through symposiums. For two days, researchers present their research related to some topic in anthropogeny and then discuss with other researchers, graduate students, and members of the public audience. By bringing together people from so many disciplinary backgrounds, the CARTA organizers believe that they are facilitating collaborations that genuinely improve the scientific study of human differences. As one CARTA organizer puts it, the "outside perspective is the magical juice" and the questions asked by geneticists or molecular biologists about human origins are "extremely precious" for the field of anthropogeny (Organizer1). One of the motivators behind CARTA is the belief that the question of what makes humans unique is "too big a question" for the field of anthropology to answer by itself, and it needs interdisciplinary input for progress to be made (Anthropologist2).

In addition, CARTA gatherings can be very fruitful for the researchers that are invited to participate. Many researchers in the social and behavioral sciences are already interested in the questions central to anthropogeny, but being in an environment where those questions are highlighted allows researchers to frame their research alongside other disciplines. One linguist highlighted the benefit of learning from primatologists who have been studying gestural and vocal communications in chimpanzees as it informs their understanding of "what is language and what properties does language have or doesn’t and what is in fact universal.” In this researcher’s view: “the more approaches you know that different sciences take toward the same problem the better you can situate yourself, your theory, your research, and your results” (Linguist1).

Part of what contributes to this is that CARTA symposiums are unlike other disciplinary gatherings, and great efforts are taken to foster an intimate and collaborative epistemic environment. In other disciplinary conferences, most people attending come from the same scientific backgrounds and already understand the possible approaches to the various problems in the field. At CARTA symposia, the topics often are outside the purview of traditional scientific disciplines. In addition, the plurality of disciplines represented at any given symposium means that participants encounter and engage with ideas that they normally wouldn’t. One biological anthropologist describes how “a lot of human life history people did not come to the biological anthropology meetings…I just hadn’t met them before even though we're talking about the same things often from slightly or very different perspectives” (Anthropologist1).

Even if the questions anthropogeny asks are traditionally housed in anthropology, CARTA prioritizes being able to move around various disciplines. CARTA is not housed by a department and maintains an independent office on the UCSD campus. This is intentional, and a CARTA organizer said that they “take pride in the fact that this is a free-floating virtual thing that doesn’t want to be tied down either on the molecular, field, fossil, philosophy or history of science side” (Organizer1). One anthropologist describes how their discipline “had challenges bringing in ‘hard science’ people to anthropology, whereas CARTA is welcoming with open arms. There’s a greater acceptance of a broad range than within a department” (Anthropologist2). This allows CARTA to take a unique role in the field of anthropogeny and act as a big tent to bring disciplines together that usually would not find themselves together.

Section III: "Leave your ego at the door"

Scientists are (in)famous for often having big egos. In fact, scientific progress is perhaps equally driven by a love of truth and a desire for credit (Zollman, 2017). Interdisciplinary collaborations where different scientific perspectives are bridged presuppose some epistemic humilities from the practitioners. Fagan argues that epistemic humility is required for successful collaboration, and each perspective must understand that they do not have the whole picture and needs input and contributions from other perspectives (Fagan, 2020). How does CARTA create a collaborative epistemic community and what do the organizers do to ensure successful interaction?

Getting lots of people from a variety of backgrounds to spend time together is not an easy task, and this goes double for scientists. Not only are researchers coming from highly technical fields, they also often come with egos and biases that might affect how well they work together with each other. Even at the level of jargon, many scientists "use the same words to mean something completely different" (Organizer1). This difficulty is exacerbated by the "open arms" approach of CARTA to interdisciplinary collaboration. By being in southern California in between several major research universities, many eminent scientists and even Nobel Laureates would pass through. Whenever CARTA organizers "knew someone interesting was coming through they would invite them" (Organizer1). This could quickly create friction as the invitees would often be big names in their own disciplines, but "had no idea who was way more famous than them but not in their field." (Organizer1).

To make these situations work, CARTA organizers see themselves as engaging in something like social engineering. This is a common theme in interdisciplinary collaborations: such as how systems biologists described their discipline as constantly engaged in getting the “sociology” right so they could “generate the appropriate social environment where scientists with different expertise could come together to work productively as a team” (Calvert and Fujimura, 2011, 156). A similar task is taken up by the CARTA organizers in making their symposiums work. Ajit Varki, founder of CARTA, apparently took inspiration from stories of Quincy Jones organizing pop stars for a benefit concert. In order to “juggle all these huge egoes…[Jones] made a huge sign that said ‘please leave ego at the door,’” (Organizer1). Ajit Varki would “vote people off the island if they didn’t interact well…He controlled the circle of people involved in a way that was very productive” (Neuroscientist2). Balancing the egos and personalities of scientists so that CARTA meetings could be productive was a constant challenge for the organizers.

From the interviews, this effort seems to have paid off. Many CARTA affiliated researchers credit CARTA as being a very academically fulfilling organization — with the private meetings at the symposiums being one of the highlights. The meetings include only the invited speakers, graduate students, and CARTA staff, and tend to have more “intimate discussion” than the public presentations the previous day (Neuroscientist1). The discussions occur in a “very open and easy environment” where participants can “ask questions without feeling dumb” (CognitiveScientist1). As one linguist puts it, “where else would you ask a geneticist the most basic question about genetics?” (Linguist1). For graduate students in the CARTA specialization track, these events give an opportunity to meet both “people more senior in the field, but also people that are my peers in different departments that I wouldn’t come together with outside CARTA” (CognitiveScientist1). An emeritus professor credits CARTA as being an incredibly “valuable and intellectually satisfying” professional group with “more genuine intellectual interaction and less professional posturing than any other group.”

Section IV: Disciplinary Influence

As discussed, CARTA does not seek to produce new research but instead facilitate interaction and train new researchers. In this way, CARTA is a unique Darwinian individual compared to traditional institutional science like laboratories, departments, and institutes. In this section, I elaborate how CARTA sees itself as contributing to the field of anthropogeny.

Through programs like the graduate certification course and the symposiums, CARTA hopes to have an impact on the future of the field of anthropogeny. The goal of the specialization track is to bring graduate students from various departments at UCSD together to think about anthropogeny. It tries to "show people who are not biological anthropologists how what they are doing can be brought to bear on questions that are central to biological anthropology" (Anthropologist2). As the graduate students continue into academia, they may reproduce this anthropogenic perspective in their own students. This is made easier by the public symposiums being recorded and available online. One researcher explains that they use the talks in classes because they allow students to "hear from these world class researchers" (Neuroscientist1). The same researcher claims inspiration from how CARTA "[fosters] interdisciplinary communities" to how they will lead their own lab (Neuroscientist1).

But CARTA's effect on their graduate students goes beyond just education. By exposing graduate students to the professional networks of CARTA, they hope to aid their job prospects and future employment. As part of one of the required classes for the anthropogeny specialization, graduate students participate in symposiums and get paired with one of the speakers and are tasked with writing up a report on the talk and discussion. During the discussions, students are always sat "between speakers" to open up conversations from senior scientists to junior scientists (CognitiveScientist1). One former graduate student credits CARTA with helping them meet “the important linguists and psychologists” in their field and “establish those connections and learn about people and their research” (Linguist1). Even outside of the symposiums, graduate students can go to CARTA organizers and ask to “facilitate contact” with senior people in their field (CognitiveScientist1). CARTA is an opportunity to early career scientists to begin networking with eminent names in their field, and this can lead to personal connections, collaborations, and job offers.

Beyond the graduate training, CARTA has been associated with developments within the various fields of anthropogeny. One neuroscientist describes how in the 1990s, the differences between human brains and the brains of model animals (like mice and macaques) were overlooked. They recalled presenting on differences between macaques and humans and receiving a “a violent table pounding response”. This sentiment has changed since then, and this researcher claims that “at least on this side of the Atlantic, everyone who’s been involved in publishing work that shows the differences has been a CARTA member” (Neuroscientist2). It is unclear how causally responsible CARTA was on this changing attitude, but this example highlights how important providing a venue for cross-disciplinary discussion of human differences could be. CARTA has also tried to take other routes in influencing the scientific landscape. The Matrix of Comparative Anthropogeny (MOCA) is a reference work showcasing human differences across molecular, genetic, behavioral, and cognitive domains. Each article is written by a CARTA member and the compiled work is currently available on the CARTA website (Varki and Gagneux, 2017). The original plan was the “publish it through…the National Library of Medicine” however, that never panned out (Anthropologist2).

CARTA attempts to influence the scientific landscape to investigate and pay attention to questions of human differences. Instead of publishing or funding research about human differences, CARTA hopes to indirectly aid the field by instilling anthropogenic perspectives in graduate students and providing a venue for discussion of anthropogeny.

Section V: Funding and Stakeholders

Most institutionalized science is funded by universities, government research grants or private money. However, the overall scientific funding structure is designed to accommodate basic or applied research and not collaborative institutions like CARTA (Trapani, 2021). In this section, I show how CARTA is kept afloat financially, and how their unique institutional structure contributes to their successes and difficulties in finding funding sources.

This institutional set up of CARTA is different from traditionally funded science. A laboratory has a very specific desired output, and grant distributors are able to weigh specific possible outcomes. In CARTA, the impact is not through publishing research but by facilitating interactions that could lead to new research. This makes it so that the "output is hard to measure" and one CARTA member compares to "[throwing] people together in a bag and [shaking] it. Maybe something comes out, and sometimes it doesn't" (Anthropologist2). The epistemic merits of this approach aside, from a funders point of view, it's a "high risk and high gain venture" (Anthropologist2).

To this point, CARTA has had a very untraditional funding history. Sponsors have been philanthropic and driven by individual interest in funding an organization like CARTA. The longest funder has been the Mathers Foundation, and this benefactor began when a director at Mathers attended some meetings and "was impressed" by the discussions and the way anthropologists “seemed to be able to talk to geneticists and primatologists and to you know paleontologists and linguists” (Organizer1). The director then arranged for the Mathers Foundation to help fund both the graduate certification program and the costs of transportation and lodging for presenters and attendees of the symposia. But a few years ago, that director passed away and “the board of directors decided that they wanted to return to their main source of funding which was bench laboratory science, not conferences and CARTA” (Anthropologist1). Since then, CARTA has been kept afloat with various philanthropic donations and the financial “cliff” is “2 years away” (Organizer1). Many interviewees point to the fact that since the output of CARTA is so hard to predict, securing funding is incredibly difficult. One long-time member reflects on the serendipitous “confluence of someone like Ajit [Varki and] Rusty [Gage], who were asking these questions at this new institution, UCSD, that had an interest overall in interdisciplinary work… and to get linked up with someone who could fund the project. Having those three pieces fall together was really fortuitous” (Anthropologist2).

This difficulty in funding is not due to the study of anthropogeny being a scientific dead-end. There are plenty of labs or institutes that are engaged in anthropogenic research and are able to achieve success. For example, the Gilad Lab at the University of Chicago is led by a CARTA affiliate and studies the “genetic basis of human-specific traits” (“Gilad Lab Research Overview”, 2023). The CARTA affiliate led Muotri Lab at UCSD that tries to answer “what makes us uniquely human” by “studying the brain from an evolutionary and developmental perspective” (“Muotri Lab Research”, 2024). Even Ajit Varki’s own research, which studies human-specific immune cells, has produced important and potentially profitable work (Honigsbaum, 2023; Varki et al., 2011). The largest single institute studying anthropogeny as CARTA conceives it might be the MPI of Biological Anthropology in Leipzig, Germany.

Founded in 1998 by Svante Pääbo, Christoph Boesch, Mike Tomasello, and Bernard Comrie, the institute was imagined as a place where “palentologists, linguistics, primatologists, psychologists, and geneticists” would work together to answer: “what makes humans unique?” (Pääbo, 2014). All four of the founders were CARTA affiliates and the institute has since produced Nobel prize winning work (Nobel Assembly, 2022). It is clear that when anthropogeny is done in an institutional setting that is within the boundaries of traditional scientific laboratories and research institutes, it can be very successful. However, CARTA, with its distinctive arrangement and output, is currently treading water financially.

The contemporary scientific funding structure is not designed to accommodate institutions like CARTA (Trapani, 2021), and instead it exists in a sort of liminal space between institutional grant funding and research grant funding. Research funding assumes the existence of infrastructure and looks for recipients that are likely to deliver interesting or useful results. CARTA doesn’t produce its own research and cannot do that. Institutional funding is meant to help build infrastructure for a scientific institution and covers the costs of buildings, laboratory equipment, and program logistics. The funders don’t have a specific idea of what the output is going to be but are willing to provide the seed money for a permanent institution. After setting up, the recipients are expected to be able to compete for research funding and the institutional funders stop providing money after a few years. CARTA is institutional but can’t support itself and doesn’t produce research. The only funding option left is philanthropic, and this is riskier than institutional or research funding.

Section VI: Conclusion

CARTA is an example of an institutionalized scientific perspective but diverges from traditional institutional structures like laboratories and research institutes. Because of this, it is not well rewarded by the institutional stakeholders of science, and it's financial future is uncertian. In this chapter, I show what CARTA sees itself as successful in doing and why the job it plays in anthropogeny is important. I also show how the funding structure of contemporary science is unable to make room for CARTA, even if the scientific perspective of anthropogeny is rewarded when it is embedded in traditional structures. In the next Chapter, I bring this data to bear on questions of reproduction and individuals within a Darwinian picture of science. I also frame CARTA's role in the context of Darwinian populations of science, and show how the interdisciplinary support that CARTA provides is crucial for scientific success.

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Chapter Four: Reproduction, Individuals, and Institutions

Abstract

In the previous chapter I introduced the data from a pilot ethnography of the Center for Academic Research and Training in Anthropogeny (CARTA). This chapter combines that data with the conceptual framework of population-level Darwinian science introduced in 2 to create an account of individuals and reproduction within disciplinary science. This chapter expands the evolutionary epistemological picture and shows how departments, laboratories and organizations fit together to create a Darwinian population. It explicates how the interdisciplinary collaboration fostered by CARTA is so vital for generating scientific progress.

Section I: Introduction

So far, I have focused on what are the Darwinian populations in science. The populations are broadly the different scientific perspectives institutionalized into disciplines across departments, laboratories, and organizations. These disciplines have different sets of questions, methods, goals, and knowledge, and are engaged in an inferentialist game of giving and asking for reasons. As a result of this epistemic process, different phenomenon has been discovered in the world and these phenomena can be fashioned into various epistemic products. The stakeholders behind the different disciplines are interested in funding science as it relates to discovering new epistemic products to be used. These epistemic products could be new synthetic materials, or curriculum for university classrooms. I have argued that the Darwinian stance is a productive way of understanding the relationship between the various stakeholders and disciplines. In addition, the Darwinian picture can contribute to realism about science.

This chapter tries to understand how reproduction fits into this framework. In Chapter Two, I argued that disciplinary science tends to have high heredity (H), but I did not go into the specifics of how reproduction occurs. Reproduction is an important issue to get right, as without a strong mechanism for reproduction then it would not be possible to apply the Darwinian stance to scientific disciplines. Godfrey-Smith, in discussing cultural evolution, emphasizes the need of strong “causal responsibility” of past instances of a population to future instances (Godfrey-Smith, 2009, 163). In order to map the responsibility of past instances of scientific disciplines to future instances, we also need to have a good idea of what an *individual* would be in this picture, and how those individuals are related to each other.

The ideas or perspectives of science cannot be the reproducers or individuals in this picture, contra some other accounts of evolutionary epistemology in science (Hull, 2001). Perspectives themselves cannot reproduce themselves in any meaningful way on their own. That a graduate student in neuroscience holds the same ideas or perspectives as their professor is not because of any reason internal to the ideas and perspectives of neuroscience. There is nothing about the perspective of neuroscience and thinking about the central nervous system in terms of neurons with transmitters and action potentials that causes the ideas to hop from scientist to scientist like a virus. Instead, the reproduction of the perspective requires years of training, practice, and study the graduate students undergo as part of their curriculum. This reproduction takes place in university departments, journals, and conferences. To understand reproduction of science, it is necessary to understand the epistemic aspects of science (the perspectives) as well as the institutional arrangements scientists are in (the disciplines). Both the perspective and the discipline that houses it “share a common fate”, so an evolutionary account of science must consider both the ideas and the institutions as bound together (Wilson, 2007, 154).

Section II: Disciplines and Collective Reproducers

How can disciplines reproduce themselves? Godfrey-Smith (2009, 87-91) highlights three different broad types of reproducers: simple, collective, and scaffolded. Collective reproducers are entities made of simpler parts that can reproduce on their own without needing other parts of that entity. For example, buffalo herds are entities that can be reduced to the reproduction of the individual buffalo in that herd. The reproduction of buffalo herds can be reduced to “just” the reproduction of individual buffalo in that herd. This is not to say that the fitness of buffalo herds is reducible to the fitness of the individual buffalo, since being part of the herd helps the buffalo survive, but that the reproduction of the herds can be reduced to the reproduction of the individual buffalo. Other examples of collective reproducers are bee hives and humans. Simple reproducers are entities that’s reproduction cannot be reduced to any lower level, such as bacterial cells that split with their own internal resources and mechanisms. The final category is scaffolded reproducers: reproducers that’s reproduction is tied to the reproduction of others. They cannot reproduce on their own, and their reproduction is tied to something external to them. For example, viruses need a host cell to be able to reproduce. Another example is a liver cell: livers are, in a sense, reproduced from human parent to human child, but that reproduction is not done by the parent liver cell to child liver cell. The reproduction of liver cells is scaffolded onto the reproduction of the whole human, which is itself a collective reproducer.

Scientific disciplines seem to be a type of collective reproducer. For example, the discipline of neuroscience can be understood as an entity made up of various departments, labs, journals, and conferences. The reproduction of neuroscience occurs through the reproduction of labs and departments that make up the discipline of neuroscience — where graduate students become postdocs become tenured professors or principal investigators (PIs) of new labs. The crucial aspect is that some elements of the scientific perspective are taken up in a new institutional arrangement. If a new discipline is started at one department and quickly spreads to other departments, that is reproduction. However, if graduate students from a department end up getting hired at the same department, there is no new institutional arrangement so that discipline has not reproduced. Labs represent an easy to track individual reproducer in disciplinary science. Graduate students in the sciences are often highly associated with one lab or mentor, while in humanities departments graduate students are more likely the result of the sum of input of many professors. When graduate students become PIs of new labs, it is a clear case of reproduction since the perspective that graduate student gained at the old lab is reproduced in a new environment that can continue reproducing and creating more graduate students.

How do we understand what is distinct about the scientific reproduction versus non-scientific university reproduction? Compared to fields like history or philosophy, which don't have labs, does the way the laboratory sciences reproduce provide any social epistemic or evolutionary benefits? What about CARTA? What role does it play in the reproduction of discipline of anthropogeny? To make progress on these questions, we have to understand the different ways that collective reproducers can reproduce.

Godfrey-Smith provides examples of a variety of types of collective reproducers. Buffalo herds are collective reproducers, but so are bee hives and humans. To give structure to accounting for the variety of collective reproducers, Godfrey-Smith introduces three more dimensions: bottleneck (B), reproductive specialization (G), and integration (I) (Godfrey-Smith, 2009, 91-100). Just like with H, variation (V), and continuity (C) from Chapter Two, the higher the levels of these three dimensions, the more paradigmatic the Darwinian population is. B represents how much the reproduced individual starts anew. Buffalo herds have low B since, if a herd splits into two and reproduces, the new herd is just a continuation of a lot of the parts of the parent herd. In humans, on the contrary, there is a unicellular beginning to the new human. Godfrey-Smith argues that this "bottleneck" allows for crucial variation to occur. Split buffalo herds are not very different from parent herd, but new humans can gain novel traits (or shed malicious parental traits like cancer or blindness).

G, also called referred to as the germ line dimension, represents the division of labor in the reproducers. In humans, there are some cells that can reproduce (germ cells), but most cells cannot (somatic cells). Liver cells, no matter how "fit", are reproductive dead ends and will not be able to reproduce. On the contrary, most buffalos in a buffalo herd are able to reproduce. Humans therefore have high G while buffalo herds have low G. High G can be useful since it divides the labor for the reproducers. Not every part of the collective has to expend the resources for reproduction and can instead focus on other matters to ensure survival.

The final dimension is I. This is the hardest to define of the three dimensions and represents the "'integration' of the collective in an overall sense" (Godfrey-Smith, 2009, 93). To differentiate with the germ/soma distinction, Godfrey-Smith gives the example of eusocial insect hives. For example, both honeybees and bumblebees live in hives with tens of thousands of individuals, caste divisions, and intricate mechanisms of organizing colony defense and foraging. Both would have high I. However, bumblebees are more likely to have worker bees be able to lay eggs while honeybees are much more likely to only have the queen be able to lay eggs. Because of this, bumblebees have lower G than honeybees, but both have the same I.

Section III: Varieties of Reproduction in Science

The reproducers in science can exist in a variety of ways, some of which can be seen as fulfilling the dimensions of B, G, and I. The typical case of scientific reproduction is a graduate student trained at one lab moving to a different university and starting their own lab there. The scientific perspective of the parent lab is reproduced into a new lab. This seems to have high B, as the new lab can be set up in ways that introduce perspectival variation like new questions or new methods. It can also avoid some issues with the old lab like if there was a toxic culture or antiquated methods still used by the old PI. However, we can also think of cases where B would not be as high. In a humanities field like history or philosophy, there are no labs but only departments. If two departments share the same perspectives and are closely tied, a graduate student from one department being hired into the other department won't be an opportunity for change like when a former graduate student starts a new lab. The perspective and culture of where that graduate student was trained is largely carried over to the department that hired them, and reproduction does not go through a bottleneck.

What about G and I? To this, I will draw from the ethnography of the Center for Academic Research and Training in Anthropogeny (CARTA) discussed in the previous chapter. CARTA is an institutionalized version of the scientific perspective of anthropogeny, which is the study of human origins. Anthropogeny can occur in labs within the disciplines of genetics, medicine, neuroscience, or psychology, but CARTA is a unique institutional structure for anthropogeny. Instead of creating new research and performing the puzzle-work of science, its primary goals are hosting interdisciplinary symposiums and contributing to the training of graduate students. In the graduate training, graduate students in a variety of departments at the University of California, San Diego (UCSD) take a few courses on anthropogeny and get access to various networking opportunities that CARTA provide. The goal is that the graduate students, when they go off and start a lab in whatever field they are in, brings over some of the perspectival features of anthropogeny. These could be in the form of guiding the questions asked by the lab or helping the researcher situate their own work in relation to other disciplines. The symposia on the other hand bring together various researchers across the disciplines and try to cultivate a productive epistemic atmosphere. Here the hope is that new ideas can be tried out and discussed, and maybe new projects can be inspired.

The graduate training highlights how CARTA is akin to somatic cells while graduate students are germ cells. CARTA is not trying to cause the creation of other organizations like CARTA in their graduate training. When a department or lab trains graduate students, the hope is that those graduate students go on and create their own labs. Among the graduate students in a laboratory, all of them can potentially become a new reproducer. This means there is low G.

With CARTA, however, it is not trying to create new CARTA-like organizations, but instead affect the reproduction of the graduate students already in various labs and departments at UCSD. This showcases a higher G. The symposium, by bringing together researchers from across the field of anthropogeny, encourages interaction and collaboration between both senior scientists and graduate students. Compared to an alternative where these interactions never occur, there is higher I among CARTA affiliates because of the symposia.

CARTA's arrangement means less individual reproduction and its own H value is low — CARTA isn't trying to spawn more interdisciplinary organizations like CARTA in other universities. Instead, CARTA supports the Darwinian dimensions of other anthropogeny labs: researchers are able to explore the epistemic landscape of anthropogeny through symposiums and graduate students from a variety of departments at UCSD are connected to researchers and often go on to do anthropogeny research during graduate school or after they graduate. CARTA increases the continuity (C) of the field of anthropogeny instead of trying to reproduce itself.

How do we understand CARTA? It is an institutionalized form of anthropogeny, but it isn't a reproducer in the same way that labs and departments are. Does this show where the Darwinian framework cannot apply to science? On the contrary, Godfrey-Smith's framework is in a position to explain the function and existence of CARTA and its utility to the rest of the field of anthropogeny.

Section IV: De-Darwinization

An important aspect of Darwinian processes is that they take place on several different ontological levels. In the case of buffalo herds, there is the level of the herds themselves, the individual buffalo, and the cells making up individual buffalo. All three levels are important for the Darwinian processes, as each level makes unique contributions to the fitness of the buffalo. The herd helps the buffalo avoid predation, the individual buffalo allows centralized and coordinated interaction with the environment, and the cellular level maintains the biochemical processes that are needed for survival and reproduction. For something at one level to be paradigmatically Darwinian, often the lower levels must not be Darwinian. With liver cells, if each liver cell's fitness is reflected their intrinsic traits (S), then cancers would constantly develop in buffalo and the higher levels would not be Darwinian. The buffalo as an individual developed mechanisms to "de-Darwinize" the lower levels to support the higher levels (Godfrey-Smith, 2009, 101). Part of the way this has been done has been increasing B and G values so that only some of the cells have the potential to reproduce.

I argue that CARTA shows a similar process occurring in scientific Darwinian selection. Anthropogeny represents the higher-level entity that is reproduced through the reproduction of lower-level laboratories and departments. These institutions have not been de-Darwinized and are more like the individual buffalo of a buffalo herd. CARTA, on the other hand, has been de-Darwinized. It is not meant to reproduce, but instead help the rest of the population by increasing their I and C. Then is CARTA to the discipline of anthropogeny like liver cells to the buffalo?

Not quite. Liver cells are present in every individual buffalo, but CARTA is independent from the reproducing labs of anthropogeny. The levels that are clearly distinguished in buffalo (herds to individuals to cells) are not easily distinguishable in disciplinary science. It is clear what liver cells are for buffalo and to tell when the liver cell ends, and the other parts of the buffalo begin. But that is not so easy to do with scientific perspectives and disciplines: is CARTA only present during symposia or is it performing its role when people watch the recorded videos or have conversations inspired by CARTA?

What is clear and distinct is the institutional structures that make up CARTA and the various anthropogeny labs. With funding and stakeholders, we can demarcate where the individuals begin and end by minding the discrete institutional arrangements. Some of the individuals are laboratories who act as reproducers, while CARTA is an individual that is niche constructive. Instead of a lower-level de-Darwinization like in biological evolution, CARTA is an example of between-individual de-Darwinization. A similar arrangement can be seen with conferences, journals, and other scientific institutions that are not reproducing. They are individuals in the sense that they are an instance of a scientific perspective merged to a discrete institutional arrangement, but their role in the discipline is myriad and *niche constructive*.

Having variation in the different individuals within a discipline is vital. The things that CARTA does cannot be done in laboratories or even traditional conferences and journals. There is an open-mindedness and emphasis on collaboration fostered by CARTA that is rare among scientific institutions. This can have important social epistemic effects on the wider field. For example, Reijula et al. (2023) modeled how different groups of epistemic agents might most effectively divide work in a shared cognitive task. That shared task can be compared to an interdisciplinary problem. The task can be broken down into smaller problems, but the smaller problems are interrelated and solving the smaller problems depends on having answers to other smaller problems. Each individual agent can only do part of the task, so the group of agents must find a way to effectively split the task into manageable parts. But to do this, there must be some understanding of what should be split. In traditional research grants, most of the money is provided up front. In the model, this is represented by the task being divided up quickly without the agents knowing much about the task. This proves to be a very ineffective strategy. Much more effective was having the agents work a little bit on the task together and then splitting up.

This way, the agents know how their solutions fit into solutions that other agents are working on. This can be compared to a small exploratory phase that is followed by the rest of funding for research. This latter funding structure is rare in science but is something that CARTA tries to do. At symposiums, researchers are given the opportunity to explore possible interdisciplinary collaborations without having to complete them. The most promising ones can be followed up afterwards in their own labs, while CARTA focuses on providing the space for that initial collaboration and exploration.

This framework makes the benefit of CARTA to the wider field of anthropogeny clear. Without CARTA and other individuals with non-reproducing roles, Darwinian populations in science are less likely to produce novelty. These non-reproducing individuals increase the Darwinian dimensions of the reproducers by creating niches that improve and maintain fitness.

Section V: Concluding Remarks

In this honors thesis, I have explored various issues in scientific realism, perspectivism, philosophy of interdisciplinarity, evolutionary biology, cultural evolution, sociology of science, and science policy. I will now briefly summarize the main takeaways and concepts of this thesis, and sketch where to go from here. I started with wondering about scientific realism and how to reconcile our faith in the results and possibilities of science with the knowledge that scientists can only ever individually access partial viewpoints of phenomenon. This realism was especially tested in the case of interdisciplinary collaborations in science. In Chapter One, I argued that there was a way to hold both scientific realism and a forward-facing view of scientific collaboration, but to do so the stakeholders behind science must be considered. When judging new and developing scientific collaborations, we cannot rely on cross-perspectival validation like we can with historical scientific discoveries (Massimi, 2018). Instead, we must look at the stakeholders behind science and find our realism there if we are to find it at all. In Chapter Two, I continued this line and explored what taking stakeholders seriously in science could mean for philosophy of science. One of the important insights of Massimi's perspectival realism is that knowledge about the world only occurs when populations of scientists are able to come together and intersect their knowledge (Massimi, 2022). I explored what a population-level view of science would mean and how it might differ from an individual-level view of science traditionally taken up by philosophers. I showed how fruitful this conceptual framework could be with the right tools and the possibility of a distinctly Darwinian approach to understanding science. In Chapter Three, I introduced an empirical case study of a very unique academic institution: CARTA. I highlighted some of the issues that came up again and again in the interviews, such as perspectival focus, social cohesion, interaction with disciplines, and getting funding from stakeholders. I reflected on what philosophy of science could contribute to understanding these issues. In Chapter Four, I returned to the Darwinian picture I began in Chapter Two but focusing in on the issue of reproductions and individuals in Darwinian science. The qualitative data proved very useful, and I believe I have provided a convincing understanding of what an individual is and how reproduction can occur.

The defense I have made for realism is a circuitous one. Fagan (2020) opened up a threat to realism in the case of interdisciplinary collaborations between perspectives. I argue that to properly address this threat, the tools of philosophy of biology and evolutionary epistemology can be used. H and V are critical for perspectives in institutional science to keep high. Without high H, then there are no scientific perspectives that persist over time. Without high V, scientific perspectives will epistemically stagnate and bankrupt the realist. But what about C? I argue that

CARTA, and other interdisciplinary environments, increase C by allowing scientific perspectives to intersect and develop. CARTA is a certain kind of stakeholder in anthropogeny that aims at enhancing interdisciplinarity. This, in turn, enhances the C value. There are more explorations of the epistemic landscape of human origins among linguists, neuroscientists, and anthropologists thanks to CARTA than if CARTA was unable to keep funding itself. A population of scientific perspectives with higher C will be able to discover more epistemic products than a population with lower C. Without the stakeholder of CARTA, our realism in the disciplinary scientific community to discover and hold true scientific representations about the origins of humanity is threatened. Our realism about any population of scientific perspectives, whether in the epistemic landscape of medicine or physics or neuroscience, is dependent on the stakeholders that create the Darwinian dimensions for those perspectives to undergo selection processes.

This honors thesis has covered a lot of ground, but I think more can be done with this Darwinian framework. The Darwinian stance has only been applied in this honors thesis to different populations of scientific perspectives. I believe that it might prove useful in comparing scientific communities to non-scientific epistemic communities. For example, is there something in the institutional arrangement of medicine that accounts for its epistemic differences from science? What about journalism? Is there something Darwinian about science that is not the case in other epistemic communities? There is also an interesting line of inquiry in historical scientific communities. How do the scientific institutions of renaissance Italy compare to Ming China or Victorian England? How did the Darwinian features of contemporary science get created, and are there historical examples that we can take inspiration from? Finally, there is much more to do to empirically validate this framework. Bibliometrics and online resources like NeuroTree could be leveraged to produce models of perspectival and disciplinary growth and development over decades. Will the Darwinian framework have any explanatory power to understanding how science changes and develops?

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