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Introduction

Introduction: Interdisciplinary model exchanges

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1. Introduction

The five studies of this special section investigate the role of models and similar representational tools in interdisciplinarity. These studies were all written by philosophers of science, who focused on interdisciplinary episodes between disciplines and sub-disciplines ranging from physics, chemistry and biology to the computational sciences, sociology and economics. The reasons we present these divergent studies in a collective form are three.

First, we want to establish model-exchange as a *kind* of interdisciplinary event. The five case studies, which are summarized in Section 2 below, show the relevance of this kind. Arguing for the relative unity of these cases will, we hope, re-orient the current debate over interdisciplinarity so as to reflect more appropriately the importance of this kind. We discuss our view of the current state of the debate in Section 3. The evidence from these cases also helps us to develop a taxonomy of interdisciplinary model exchanges in Section 4—a taxonomy, we would like to add, that might be useful for the discussion of interdisciplinary exchanges beyond the context of models and their transfer.

The second reason for presenting these studies together is that they provide an important *source of evidence* for the philosophy of science. Over the last three decades, philosophy of science has increasingly differentiated into philosophies of various disciplines. This differentiation in our view has greatly increased our understanding of the scientific practices of the respective disciplines, including the epistemological and methodological standards and conventions on which these practices are based and by which they are evaluated. But it has also made it harder to compare these practices and standards across disciplines. The studies we present here are case studies of interdisciplinary exchange: they focus on the transfer, collaborative construction or parallel use of models and similar representational tools. They therefore provide a unique opportunity to investigate various disciplinary treatments of the same or at least similar representational tools. This allows the identification and comparison of different disciplinary practices and their underlying conventions as well as of the respective normative standards of their evaluation. By tracing the paths along which models travel between disciplines and research fields we can observe to which extent discipline-external practices associated with an adopted tool are retained or replaced by discipline-internal practices. This generates invaluable information about both disciplinarity and interdisciplinarity. We develop this argument further in Section 5.

The third reason for presenting these studies in collective form is that their philosophical analysis also has important *normative implications* for the notion of interdisciplinarity itself. Too often in the current (non-philosophical) discourse is interdisciplinarity cast as an exclusively integrative project: interdisciplinary exchange is often claimed to be successful only if the involved disciplines become mutually more integrated as a consequence of this process. In contrast to this, many of the cases presented here show that interdisciplinary exchange can be scientifically highly successful, even if at the end of the exchange disciplinary borders remain fully intact. Indeed, borders can be fruitfully crossed without any integration across these borders. Such considerations of divergence in scientific practices and tools offer arguments against a naïve plea for unitarian or non-pluralist versions of interdisciplinarity. Disciplinary divergences may have their justifications, and attempting exchanges that require the reduction of these divergences may consequently not be justified. We pursue this argument further in Section 6.

2. The five studies

In his paper *Maxwell's Color Statistics: From Reduction Of Visible Errors To Reduction To Invisible Molecules*, Jordi Cat describes James Clerk Maxwell's introduction of statistical models into physics. Cat takes issue with the argument that the conceptual and methodological roots of Maxwell's physics lie in the social sciences (to wit, Quetelet's representation of social traits by the normal distribution). Instead, Cat argues that Maxwell substantially relied on existing methods in physics—specifically, error detection in data models—when adopting Quetelet's tools: "what I have called the genidentity in the cognitive series…gradually becomes restricted to the thinnest and most abstract versions of the original instance" (Cat, 2014). Thus, although Maxwell's physics clearly is the product of interdisciplinary transfer of representational tools from the then emerging social sciences, it is the result of mixing an adopted tool with scientific practices that had been in use in physics before.

In their paper *Varieties of Noise: Analogical Reasoning in Synthetic Biology*, Tarja Knuuttila and Andrea Loettgers describe the interdisciplinary research practices of synthetic biologists, and

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study how, through analogical reasoning, synthetic biologists utilize the theoretical results, tools, methods, templates, and concepts of other fields and disciplines. They argue that besides the positive analogies they draw to physics and engineering, synthetic biologists also distinguish their newly emerging domain from either of these origins by drawing negative analogies to engineered systems concerning, for example, how processes are controlled. Specifically, Knuuttila and Loettgers focus on the notion of noise, which has different meanings in each of these disciplines. Their analysis of this exchange yields the conclusion that "analogical reasoning is more transient and preparatory in nature, a tool used to conceptualize and grasp novel and less known phenomena" (Knuuttila & Loettgers, 2014), rather than a tool that fixes a mapping between (isomorphic) source and target domains.

In his paper *Disciplines, Models, and Computers: The Path To Computational Quantum Chemistry*, Johannes Lenhard argues that computational quantum chemistry did not arise just from the increase in computational power, but through the use of computational models as autonomous agents. These computational models introduced "a new conception of modeling" (Lenhard, 2014) into quantum chemistry, as shown through an investigation of density functional theory (DFT). DFT commences from theory (the Schrödinger equations), but then simplifies it considerably, by including an unknown functional E(p) whose "physical meaning" is justified "for reasons of performance" through "semi-empirical adaptation" (Lenhard, 2014). This stands in marked contrast to the traditional conception of *ab initio* methods in quantum chemistry.

In their paper *Unification And Mechanistic Detail As Drivers Of Model Construction: Models Of Networks In Economics And Sociology*, Jaakko Kuorikoski and Caterina Marchionni describe how techniques of modeling networks are employed in different disciplines. By comparing the respective uses of this model in economics and sociology, they identify differences in modeling methodology between these disciplines. They argue that these differences are not fully accounted for by different explanatory purposes, but rather involve different conceptions of the virtue of *generality*. In economics, generality is understood as the virtue of unifying all explanatory projects under one analytic tool; while in sociology, generality is understood as the virtue of mosaic unity of levels of mechanisms,[1] where the representational tool should relate evidence from different levels.

In her paper *The Birth Of Classical Genetics As The Junction Of Two Disciplines: Conceptual Change As Representational Change*, Marion Vorms describes the formation of classical genetics as the junction of Mendelism and cytology. This junction, she argues, did not simply come about through the introduction of a cytological hypothesis into Mendelism, in conjunction with an accompanying mechanistic explanation. Rather, the junction occurred as an integration of the two representational forms of the respective sub-disciplines. Vorms distinguishes the diagrammatic representational form of Mendelism from the schematic form of cytology, and shows that the introduction of "linkage maps" by Sturtevant created a new, integrated form that was reducible to neither, but which embodies a genuine conceptual change and the emergence of classical genetics.

3. Theoretical background—interdisciplinarity and philosophy of science

The academic community, including the institutions that manage and determine research policy, has paid considerable and growing attention to interdisciplinarity over the last decades, both as an objective of scientific practice, as well as a subject of academic study itself.

The vast majority of these studies derive from perspectives such as those of research management, science education, measurement of scientific performance and the like. They deal with issues of describing and designing the broad institutional settings of disciplinary divisions of the sciences, hopes and difficulties of communication and collaboration between them, challenges and achievements in constructing interdisciplinary projects and research cultures, issues in connecting academic research with the interests of and contributions from extra-academic parties, and so on (cf. e.g. Barry & Born, 2011; Frodeman, Klein, & Mitcham, 2010; Klein, 2001; Moran, 2010; Østreng, 2010; Stehr & Weingart, 2000).

Philosophers of science have largely neglected the topic until recently (see Mäki, 2014 for this claim). Of course, there is the extensive 'unity of science' discussion, which addresses the question how scientific disciplines relate to each other. Yet the question there is cast predominantly as a matter of theory unification or unity of method, and the answers are often based on ontological presuppositions: if the objects of inquiry are appropriately connected (for example, one being reducible to another), then arguably theories about them can be unified and/or the methods used in investigating them can converge, and hence disciplines putting forward these theories and applying those methods are related (see e.g. Cat, 2010).

It is quite clear that typical contributions to the unity of science debate do not answer the currently most pressing questions about interdisciplinarity. Most of the criteria designed within these debates for the unity of science are far too strong to be useful. Disciplinarity is a matter of fact for contemporary science, even though the ontological foundations of the disciplines are rather unclear and controversial to scientists and their observers alike (as witnessed by the various reductionist and anti-reductionist arguments still entertained). Attempts at forging interdisciplinary collaborations or exchanges, or at founding new disciplines, cannot wait for these ontological questions to be sorted out. Thus much of the unity of science debate, although it is the only widely known philosophical discussion that addresses scientific interdisciplinarity directly, has relatively little to offer for the explanation or the critical assessment of actual interdisciplinary exchanges.[2] This is not to deny the importance of ideals of unity in *motivating* various interdisciplinary aspirations, appealing to principles such as, "since the world is one, science should be one as well".

Note that none of the studies presented here chooses this route—none of them discusses ontological presuppositions. For example, take the case of Kuorikoski and Marchionni (2014), who—although discussing unification extensively—explicitly do so as a justifier of discipline-specific practices. From their perspective, economists aim at unification in the sense of the most parsimonious set of behavioral assumptions from which the maximum number of

[1] In the "mosaic unity of science", a term coined by Carl Craver and applied originally to neuroscience, each mechanistic level of explanation gradually constrains the space of possible mechanisms for a phenomenon. Each level thus contributes pieces that eventually lead to the completion of the whole mosaic, which is the mechanism that accounts for the explanandum phenomenon.

[2] On top of ideas about ontological unity and disunity of disciplines, there are ambitions and arguments concerning their derivational (dis-)unity (Mäki, 2001), but these cannot be expected to do any better in setting plausible and tractable standards other than for limited local unifications. The most promising line might be provided by "Neurathian" ideas of unity by integration whereby disciplines are connected to one another at the edges, guided by ideals of coherence and without requiring any sort of hierarchical subordination of other disciplines by one super discipline, for example.

types of consequent phenomena can be deduced. This methodological maxim puts economists in direct contrast to sociologists, who propose multi-level mechanism schemas as a common but disjointed toolbox for the social sciences. This is a conflict between discipline-specific methods, where ontological considerations play no explicit or instrumental role.

Instead, the studies in this special section pursue other trajectories and trends in the philosophy of science useful for the analysis and assessment of interdisciplinarity in science. These include arguments on various forms of incommensurability between scientific disciplines. In fact, Kuhnian paradigms are constituted pretty much as scientific disciplines are, so similar arguments could be expected to apply when considering issues of compatibility and communication across disciplinary boundaries. Vorms, who follows Kuhn in characterizing disciplines by the representations they produce and reason with, stresses the epistemological content of the paradigm concept:

> “Paying attention to disciplines and scientific communities rather than (exclusively) to theories does not constitute a shift from philosophy to sociology, but rather a novel approach to the epistemological issues that philosophy of science traditionally tackles.” (Vorms, 2014)

Most contemporary philosophers of science have moved away from a foundationalist presumption that philosophers can describe and fix on *a priori* grounds the standards that constitute genuine scientific knowledge. Instead, they now pursue the study of scientific practices with the goal of assessing them in the light of the objectives the respective sciences proclaim. The resulting accounts—e.g. of theoretical and statistical modeling, of experimentation, of measurement, each in different disciplines—are not historical accounts of mere contingencies, but seek to understand and perhaps justify these practices in the light of the respective discipline’s contexts and objectives. They thus offer a rich source of information about the variety and extent of scientific rationality.

This attitude is present in all the studies collected here. It is particularly obvious in Cat’s paper, who describes the historically contingent development of Maxwell’s statistical physics, but locates it in “an epistemic tradition sustained and policed by astronomers and expanded to a broader experimental culture of precision” (Cat, 2014) to which Maxwell felt he had to answer, and which affected the practices in which the newly imported social science models were to be used in physics. The attitude is also obvious in Lenhard’s paper, which describes the rise of computational quantum chemistry as driven not only by the availability of new computational tools, but also by the development of autonomous computational models with associated novel epistemological standards.

This focus on scientific tools and practices facilitates a re-orientation of the investigation of interdisciplinarity. Disciplines are importantly characterized by these tools and practices, shaped and guided by institutionally established disciplinary norms and conventions. Exchanges, impositions, mergings or simultaneous uses of these tools and practices then constitute episodes of interdisciplinarity. Besides an ontological or derivational unity and disunity of disciplines, we can now investigate the institutional aspects of scientific practices that pull towards greater or lesser degrees of intensity in the contacts between disciplines.

This proposed re-orientation has important forerunners. We briefly discuss three examples that bear resemblances to our approach here. Galison (1996) suggested a contextualized, practice-based analysis of interdisciplinary exchanges, in which the focus is not on the ontological objects of inquiry, but on

> “a cluster of skills in common, a new mode of producing scientific knowledge…rich enough to coordinate highly diverse subject matter (Galison, 1996, 119).

Galison analyses the case of computer simulations in post-war science. A wide range of theoretical and applied scientists, including mathematicians, physicists, bomb builders, industrial chemists and meteorologists made use of similar simulation techniques and representations. These constituted a “trading zone”, in which members of different disciplines could coordinate their activities and even alternate between problem domains with relative ease. At the same time, these trading zones left the respective scientists’ disciplinary identities intact: they provided a neutral place of exchange outside of which scientists keep pursuing their ordinary disciplinary styles and practices.

Our approach also has affinities with the proposal by Humphreys (2004) to use *templates* as a new unit for the philosophical analysis of science. Templates are purely syntactically characterized, context-independent and general—and are thus distinct from theories, models or laws. Examples of templates include functions, systems of equations, but also non-mathematical, well-specified inference rules. Crucial for our purposes is that templates by themselves are non-interpreted syntactic entities, which become models only when they are employed in different scientific contexts for different purposes. In this vein, Knuuttila and Loettgers argue that synthetic biologists build their models by drawing on a repository of formal systems studied in complex systems theory. These formal templates are applied in a variety of disciplines to investigate a wide range of entirely different kinds of phenomena. This highlights

> “an interesting link between analogical reasoning and the widespread use of cross-disciplinary formal templates in science. Examples of such formal and computational templates that can be applied to different problems in various domains are, for instance, the Poisson distribution, the Lotka-Volterra equations and different agent-based models.” (Knuuttila & Loettgers, 2014)

Another related study is that of Mattila (2005, 538), proposing an “object-oriented interdisciplinarity” as a collective combination of skill and know-how in the construction and use of research objects such as models. According to her, studying interdisciplinarity in the making should focus on the core of scientific research involved in interdisciplinary exchange, and the objects and tools that are involved in this core research. She argues that elements of models—including modeling methods, substantial knowledge of the target, and data—are the carriers of interdisciplinarity. Such elements embody complementary areas of disciplinary expertise out of which emerges interdisciplinary expertise as “playful, close, even family-like collaboration” in modeling. For her case of simulation-based modeling of epidemics, she therefore proposes a “micro-level study of interdisciplinary modeling”.

The studies collected in the present volume are of interest in their own right, identifying for specific disciplines the origin of their tools and the genesis of their associated practices, thus allowing one to draw important conclusions about the respective methodological and epistemic standards of these disciplines. By collecting these studies, we propose to make these investigations *comparative*: by investigating a number of interdisciplinary episodes next to each other, we hope to learn both about interdisciplinarity and about the tools and practices involved in these episodes. This editorial project serves as a follow-up on earlier projects on the philosophical and historical study modeling in interdisciplinary comparison and interaction (Grüne-Yanoff & Morgan, 2013; Weisberg, Okasha, & Mäki, 2011).

4. A taxonomy of model exchanges

Interdisciplinarity is always a matter of disciplines being related to one another in one way or another. Given that disciplines are institutional entities, interdisciplinarity has an institutional facet of norms, conventions, traditions, and collective practices somehow related to one another. This is so regardless of whether we just compare disciplines with each other, or whether we consider issues of communication, transfer of ideas, integration, intrusion, collaboration, criticism, or conflict between disciplines. Here, and in the papers of this special section, this institutional side of interdisciplinarity is not put on center stage. Nevertheless, the importance of the institutional framework within which ideas are transferred should not be forgotten—even if they are largely bracketed for the sake of focused analysis of the role that the properties of the transferred items play. Another limitation is that we do not consider versions of interdisciplinarity in which the participation of extra-academic partners or stakeholders play a significant role. The focus here is intra-academic.

Once we focus the investigation of interdisciplinarity onto scientific tools, a simple model of interdisciplinary exchanges offers greater analytic clarity. This model only has a handful of components: disciplines, disciplinary agents, scientific objects or tools (e.g. models and methods), and scientific problems. A first characterization goes as follows:

> An *interdisciplinary exchange* occurs if objects employed or developed in one discipline are used to solve problems of another discipline.

All the cases discussed in this special section are instances of such interdisciplinary exchange: Cat describes the employment of statistical models, developed in the social sciences, in the physical investigation of gasses. Knuuttila and Loettgers describe the employment of the concept of noise, developed in engineering, in the newly developed field of synthetic biology. Lenhard describes the introduction of new computational methods and, crucially, of the concept of a computational model, to quantum chemistry. Kuorikoski and Marchionni describe the application of network modeling techniques, developed in graph theory, in economics and sociology. Vorms, finally, describes the junction of Mendelism and cytology through the development of a new representational form.

Although these cases all share the general characteristic of an interdisciplinary exchange, they differ with respect to (i) who performs the exchange, (ii) what is exchanged, and (iii) for what purpose this exchange is performed. To systematize these differences, we propose the following simple model. Let there be two disciplines, A and B, with the respective (i) A-agents and B-agents, (ii) A-objects and B-objects, and (iii) A-problems and B-problems.[3] Different combinations yield 13 distinct cases (allowing for an outside object X in case 13). Of these cases, listed in Table 1, some are degenerate: (1) and (8) remain within one discipline, and (4), (5) and (9) involve only an agent changing disciplines.[4]

Table 1:
Possible interdisciplinary exchanges in a two-discipline environment.

		who uses…	what object…	to address what problem….
1.	within-discipline	A	A	A
2.	exportation	A	A	B
3.	importation	A	B	A
4.	move	A	B	B
5.	move	B	A	A
6.	importation	B	A	B
7.	exportation	B	B	A
8.	within-discipline	B	B	B
9.	personal collaboration	A&B	A	A
10.	transfer collaboration	A&B	A	B
11.	genuine collaboration	A&B	A&B	A
12.	new field generation	A&B	A&B	A&B
13.	parallel development	A/B	X	A/B

The others can be categorized into three types of interdisciplinary exchange: transfer, collaboration and parallel development.

In cases (2), (3), (6) and (7), an object from one discipline is employed to address a problem from another discipline. This of course is a paradigmatic case of interdisciplinary exchange. But in these cases, the agents who pursue this exchange belong to only one of the disciplines involved. We call this type of exchange *interdisciplinary transfer*, and contrast it with cases of collaboration where agents from both disciplines jointly pursue the exchange. More specifically, in a *transfer by exportation*, an A-agent uses A-objects to address B-problems of another discipline (case 2, and its inverse, case 7). For example, Gary Becker developed a model of reproductive choice based on economic models of optimization under constraints, calibrating it for the value of a child and for opportunity costs characteristic of various countries. He then employed the model to explain differentials in family size and in parental age in different countries—*explananda* that were widely considered to belong into the domain of sociology. In this case of what is often called *economics imperialism*, the economist Becker employed an economic model to address a sociological problem (Mäki, 2009). Subsequently, the model and its refinements were adopted by sociologists. But the initiative clearly came from Becker and his economic collaborators, without much involvement from the side of sociology. Becker's model of reproductive choice is thus an example where an A-agent exports an A-model to address a B-problem.

In contrast, transfer also occurs when a B-agent uses A-objects to address B-problems (case 3, and its inverse, case 6). We call this subtype *transfer by importation.* For example, economists in the 1980s discovered that biologists had developed novel models of strategic interaction, as well as new evolutionary solutions for them. Economists found that these models helped them address various intractable problems of their own discipline, and therefore adopted them for their own purposes. Biologists played no role in this importation (Grüne-Yanoff, 2011). Notably, economists soon employed the imported models in economic modeling practices, which differed substantially from the biological practices associated with these models before importation. This stands in noted contrast to the Becker case discussed above, where the exported models remained associated with economic modeling practices even in sociology. We conjecture that this difference is a generalizable feature of the two types: the disciplinary identity of whoever pursues the transfer of a model tends to determine to a large extent the practices associated with the transferred model. If an A-agent exports an A-model to apply it to a B-problem, then the model will remain associated with methodological practices common in A. Yet if a B-agent imports an A-model to apply it to a B-problem, then the model will be employed with methodological practices that dominate B.

[3] This of course assumes that agents, objects and problems can always be identified as belonging to one discipline only. While such identification is sometimes problematic in the real world of science, we believe it is unproblematic sufficiently often to warrant the usefulness of our model. Furthermore, while we limit our model to representing the interactions of two disciplines only, extensions to more than two disciplines are easily done.

[4] To be clear, these are only degenerate in the sense that they do not involve model exchanges, which is the topic of this paper. Exchanging disciplinary agents, as in these cases, might of course also have substantial consequences for the disciplines involved. But we disregard such effects in this model.

One case of transfer by importation presented in this special section supports our conjecture. Cat (2014) describes how the physicist Maxwell imported statistical models from the social sciences to address a physics problem. Yet one of Cat's main arguments is that Maxwell substantially relied on existing methods in physics—-specifically, error detection in data models—when adopting Quetelet's tools.

Another case of transfer by importation seemingly contradicts our conjecture. Lenhard (2014) describes the shift from quantum chemistry (QC) to computational quantum chemistry (CQC) that occurred with the increasing reliance on computational methods and a new (for QC) notion of computational model. Here A-agents (quantum chemists, described by Lenhard as "consolidated into a subdiscipline of chemistry", 2014) import B-models (computational models, developed with practices differing from those dominant in QC) to solve QC problems. Against our conjecture, one of Lenhard's main arguments is that the methodological practices associated with these computational models were the main drivers behind the shift (or, as Lenhard puts it, the "loosening of boundaries between disciplinary identities", 2014) from QC to CQC. CQC practitioners, although they imported the new computational models, did not replace the practices associated with them in other computational disciplines with the practices prevalent in QC.

However, Lenhard also provides an explanation for this phenomenon, which reconciles this case with our speculative hypothesis. As he points out, the development of the computational models is often located in different hands than those of CQC practitioners:

> "many practitioners of CQC work in a distance from relevant parts of modeling activity. The users of DFT-related software, the developers of this software, and the theoreticians of DFT are not identical." (Lenhard, 2014).

Thus, unlike the cases of evolutionary game theory or Maxwell's gas models, CQC practitioners, although initiating the importation of computational models, are typically not the designers of these models and hence have limited control over the practices in which the models can be used. Instead, the way these models are programmed by third parties to some extent determines how they can be used, thus exerting a considerable influence on CQC practices. This explains why this case of importation exhibits a larger degree of maintaining out-of-discipline influence on practices than is found in other, more typical cases.

In cases (10)–(12), agents from *both* disciplines pursue the exchange, and we classify them as collaborations. The exchange pursued might be a transfer (case 10). Or it might be a matter of constructing a new model, integrated from elements of both disciplines, and applied to a problem of one of the disciplines (case 11). Finally, case (12) captures collaborations that start by identifying a problem shared by both disciplines, and then proceed by constructing integrated tools employed by a group of agents from both disciplines.[5]

The case discussed by Knuuttila and Loettgers (2014) exhibits certain features of a collaboration of case (11). Physicists and engineers form a group of "synthetic biologists" that applies purportedly integrated tools from both physics and engineering to biology. However, as the authors show, having different uses of the concept of noise in mind, different synthetic biologists, largely according to their disciplinary origin, prefer different models and associate different practices with them. We therefore classify Knuuttila and Loettgers' case as a hybrid between collaboration and parallel development. Because the authors stress the relative independence of different synthetic biologists according to their background discipline, we will continue our discussion of this exchange under the heading of parallel development.

A case that more squarely fits the taxon of collaboration is the one described by Vorms (2014). There, groups consisting of both Mendelians and cytologists (e.g. the "Drosophila group") constructed a new model by integrating Menedelian and cytological representations, namely linkage maps. This model was used to address a problem that grew increasingly important for the Mendelian program, namely the explanation of transmission patterns of the genes in some model species, in particular *Drosophila Melanogaster*, together with the confirmation of the chromosome theory of heredity. Thus Vorms describes A&B-agents constructing an A&B-model to address A-problems (case 11).

In case (13) agents from both disciplines *independently* pursue an exchange of the same model from another discipline X and hence it falls within the taxon of parallel development. It is this relative independence between A and B that is of special interest here, as the respective ways in which the two disciplines deal with the same model is particularly informative about the specific features of disciplinary practices.

As already discussed above, the case Knuuttila and Loettgers present is a hybrid between parallel development and collaboration. By and large, the new sub-discipline of synthetic biology is staffed by both physicists and engineers. This diverse group works together, in that the members use the same models and conceptual tools. But Knuuttila and Loettgers nevertheless diagnose a substantial difference in outlook and practice within this mixed group, where the default line runs along differences of disciplinary origin:

> "…the two research areas overlap in various ways. For example, both branches make use of engineering concepts and aspire to understanding the organizational structures of biological systems in order to develop novel biological parts and systems. But, as we are going to show, the motivation for why and how the engineering concepts are introduced is different, and moreover, analogies to them are often drawn in different ways." (Knuuttila & Loettgers, 2014).

Furthermore, depending on their disciplinary background, particular synthetic biologists also tend to pursue different research goals. The engineering-oriented branch aims to design novel biological parts or organisms for the production of, for instance, vaccines, biofuels or therapeutics. The physics-oriented branch, in contrast, sees synthetic biology as a tool for the investigation of gene-regulatory networks. Thus, despite the collaborative efforts between A-agents and B-agents, and despite them using the same or at least similar representational and conceptual tools, A-agents and B-agents, within synthetic biology, use these tools differently and for different ends. Thus Knuuttila and Loettgers' case shares important features of the parallel development case (13).

Even clearer is Kuorikoski and Marchionni's case of the use of network modeling tools in both sociology and economics. Network modeling tools originate in neither discipline, but in mathematics, so here we have a transfer of a model into A (sociology) and B (economics) from a third discipline, X (mathematics). Both disciplines,

[5] Note that much of the current literature on interdisciplinarity focuses on exchanges similar to case (12): an interdisciplinary problem is identified, and found to be one that requires interdisciplinary tools that must be applied by an interdisciplinary group of agents. We think this focus is unduly narrow. None of the cases presented in this special section fit this description—although we believe that each of these cases satisfies the relevant properties of proper interdisciplinary exchange. Nor do most of the cases covered by our simple model match this case—although we believe that all but the degenerate cases satisfy the relevant properties of interdisciplinary exchange. We will come back to this point in Section 6.

Kuorikoski and Marchionni argue, use the tools for the same kind of purpose: to provide mechanistic explanations of social phenomena. But they do so in very different ways. Economists use network models as an analytical tool with which to unify all relevant economic explanatory projects. In contrast, sociologists use network models to represent mechanisms on different levels, thus capturing a collection of diverse but interconnected, simultaneously acting forces. Investigating this parallel development of network models thus helps one to identify and understand the methodological conventions and research practices of these different disciplines.

5. Learning from model exchanges about disciplinary practices

Philosophers of science have recently become more aware of and interested in the divergence and multitude of scientific practices. The trend is evident in the increased attention paid by philosophers to actual scientific practices and their diversity (Bailer-Jones, 2002; Grüne-Yanoff & Morgan, 2013; cf. also the Society for Philosophy of Science in Practice) as well as in their increased focus on individual sciences (such as in “philosophy of biology”, “philosophy of economics” and so on).

Yet in comparing different disciplines, one is not only comparing varying practices or styles of theorizing, modeling, representing, measuring or experimenting, but also different theories, models, representations, data, and phenomena, as well as different kinds of problem and objective. This is where the study of interdisciplinary exchange acquires an important role. On the one hand, during such exchanges, the substantial aspects of different disciplines are expected to make contact with one another, or even to converge. But on the other hand, the fulfillment of this expectation is shaped and impeded by differences in conventional disciplinary style and standard practice. In order to facilitate interdisciplinary exchange, scientists have to reflect on their own practices and objectives, as well as to negotiate the meaning and purpose of their practices with members of other disciplines. Thus, during episodes of exchange, disciplinary conventions and practices become more observable than usual.

Further, even when sharing an interest in substantial issues and problem solutions, scientists with different disciplinary backgrounds often come away from interdisciplinary exchanges with different results. That is, the differences in the conventional practices of theorizing often significantly shape the exchange results. In the diverging consequences of exchange episodes, disciplinary practices and their differences become more observable than otherwise. Both the way in which interaction happens and the results of these interdisciplinary exchanges reveal important insights into the disciplines’ respective methods, styles and tools.

The most relevant evidence for disciplinary respective methods and practices comes from cases of parallel development. Here two or more separate disciplines exchange (typically import) the same kind of tool for their respective purposes. These purposes cannot differ too much, as they are constrained by the sameness of the tool. Consequently, with an identical tool and somewhat related purposes, any differences in the way the two disciplines employ this tool must be attributable to differences in epistemological or methodological conventions and practices.

Both studies of parallel developments in this special section reveal such differences in epistemological or methodological practices. Kuorikoski and Marchionni argue that economists and sociologists have diametrically opposed understandings of the virtue of generality in modeling and consequently embrace very different modeling strategies. In economics, generality is understood as the virtue of unifying all explanatory projects under one analytic tool; while in sociology, generality is understood as the virtue of mosaic unity of levels of mechanisms, where the representational tool should relate evidence from different levels.

Knuuttila and Loettgers argue that engineering-oriented synthetic biologists understand and use the engineering concepts of their discipline differently from physics-oriented synthetic biologists. Specifically, the former see noise as a nuisance that one should get rid of, while the latter also consider the functional aspects of noise: they see noise as a crucial and distinctive characteristic of biological processes, and seek to explain these processes partly by referencing noise, rather than shutting it out.

Cases of parallel development thus are obvious test beds for hypotheses about the differences of disciplinary practices. But they are not the only ones. Cases of transfer also offer relevant evidence in this regard. Here a model from one discipline is employed to address a problem of another discipline, either by an agent belonging to the former (model exported) or the latter (model imported). In both kinds of transfer, there is a previous context of model use: the practices associated with the model in the original discipline. If these practices persevere after the transfer into the new discipline, one can investigate their compatibility with other pre-existing practices in the new disciplinary context. In an extreme case, newly transferred and pre-existing practices clash, leading to the creation of a new sub-discipline. Transfers of this sort thus provide plenty of evidence about disciplinary practices. If the practices change after the transfer into the new discipline, one can now directly compare practices associated with the model in the new and the original discipline, thus again yielding substantial evidence.

Both studies of transfer in this special section reveal such differences in epistemological and methodological conventions and practices. Cat describes how Maxwell imported the normal distribution (the “error law”) from the then emerging social sciences to apply it to the movements of molecules in gasses. Yet, as Cat argues, the practices associated with this new application did not come from the social sciences, but rather from the astronomical tradition of error theory.

> “Maxwell did not consider statistical methods in molecular theory from a blank slate. Considerations of measurement error and inductive methodology deployed in experimental color research provided the enabling conditions for the further statistical interpretation of error law and its molecular application.” (Cat, 2014)

It is these techniques and epistemology, derived from physics itself, that established the practices of how this newly imported tool should be used in physics. Cat’s investigation of this transfer thus gives us a particularly clear insight into the traditions and practices of mid-19th century physics.

Lenhard describes the shift from QC to CQC that occurred with the introduction of computational models. Crucially, because of the special circumstances of their provision, these new models also bring with them new methods—the importation of the model thus is accompanied by an importation of novel, discipline-identity changing practices. Specifically, while QC models relied on deduction from *ab initio* theory, CQC allows the construction of models whose physical meaning is determined by “semi-empirical adaptation” (Lenhard, 2014). This shift in identity thus reveals an instance of the differences in practice between chemistry and the computational sciences.

Cases of collaboration are perhaps not as obviously useful test beds for hypotheses about the differences between disciplinary practices. In transfer collaboration (case 10), the group that drives the use of an A-model to address a B-problem consists of both A-agents and B-agents. It therefore is more difficult to attribute the practices associated with the transferred model to any single discipline. Even more problematic is genuine collaboration (case 11), where both A-agents and B-agents jointly seek to build and apply an integrated A&B-model. Here, in contrast to transfers and parallel developments, it remains unclear whether the practices associated with the new A&B-model stem from any of the previous disciplinary practices, or whether they rather emerge as an outcome of the collaboration itself.

However, as Vorms (2014) shows, cases of collaboration can provide fruitful evidence concerning disciplinary practice, provided the right comparisons are made. Vorms identifies a contrast in disciplinary practices by comparing the Mendelian and cytologists' representational tools *before* the collaboration. Mendelians, she argues, employed a diagrammatic (non-spatial) representation of genetic distance, while cytologists employed a schematic (spatial) representation. Based on this contrast, she then analyses the mode of representation that emerged from the collaboration, and concludes that this amounted to a *"spatialisation of the gene"* (Vorms, 2014).

To conclude, episodes of model exchange are excellent sources of evidence for investigating disciplinary conventions and practices. As these practices play—and should play—an increasing role in the philosophy of science as its subject matter and evidential base, we recommend that philosophers of science pay more attention to such episodes and in particular seek out episodes of transfer and parallel development as the object of their study.

6. What studies of interdisciplinarity learn from philosophy of science: the rationality of disciplinary practices

Philosophy of science that takes into account the variety of disciplinary practices has something to offer to studies of interdisciplinarity in turn: a normative perspective that allows one to evaluate—from an analysis of the specific disciplinary goals and contexts—whether to favor disciplinary separation or whether more or less interaction or even integration between the disciplines would be epistemologically preferable. Some of the studies presented in this special section aim to contribute to this normative perspective. These include Kuorikoski and Marchionni, who discuss the "legitimacy of … disciplinary differences" (Kuorikoski & Marchionni, 2014), and Vorms, who sees her discussion of disciplinary representational models in the Kuhnian tradition of a "a novel approach to the epistemological issues that philosophy of science traditionally tackles." (Vorms, 2014).

Perhaps the most obviously normative stance on exchanges can be found in Vorms' study. Vorms argues that linkage maps constitute a new representational form, arising from an integration of the typical forms of representation of Mendelism and cytology respectively, but not reducible to either one. She identifies this new representation as a driving force of progress in genetics:

> "Maps do not only *suggest* a mechanistic explanation; they rather embody this explanation — their rules of construction and of interpretation involve it. They are genuine theoretical representations, from which new concepts emerge." (Vorms, 2014).

Implicit in this analysis is that the novel representational form allows conceptual development and explanatory progress that neither of the prior forms allowed. By engaging in collaborative exchange, Mendelism and cytology thus improved, epistemologically speaking.

Vorms's account fits well with the general outlook of interdisciplinary studies—which, roughly put, tends to focus on the success of interdisciplinary exchanges, and typically cashes out interdisciplinary success in terms of integration (for a critique of such views, see Grüne-Yanoff, 2014). But not all interdisciplinary exchanges succeed epistemologically, and many of those that do succeed do not involve integration. Most would agree that Maxwell's importation of the normal distribution into physics initiated a new and highly successful strand of research; Knuuttila and Loettgers argue that synthetic biology has been successful in providing better explanations and in offering new effective interventions; and Kuorikoski and Marchionni suggest that the use of network models provided new explanations both in economics and sociology. Nevertheless, all these authors argue that the success of these exchanges did not involve an integration of the respective disciplines. As Cat points out, Maxwell appropriates the social science tool by employing it with interpretations, methods and practices derived from other domains of physics, so that only "the thinnest and most abstract versions of the original instance" remain (Cat, 2014). Knuuttila and Loettgers stress the many negative analogies that synthetic biologists draw with engineering, as well as the internal rift that runs between physics-based and engineering-based synthetic biologists. Kuorikoski and Marchionni point out the substantial differences between the uses of network models in economics and sociology. Consequently, one cannot claim that the disciplines became integrated in any of these cases, despite the epistemological successes that these exchanges produced.

These observations are not mere recordings of contingent facts. That exchanges were successful despite the lack of integration and the retention of disciplinary differences suggests an important normative lesson: that sometimes, it might be justified to insist on keeping and protecting disciplinary identities and differences, against the hyperbolic rhetoric of unification and interdisciplinary integration.[6]

In this vein, Kuorikoski and Marchionni argue that although tools such as network theory are employed across different sciences, one should not conclude that such an observation warrants grand claims of unification (cf. Marchionni, 2013). To the contrary, we would like to add, different uses of the same tool might be warranted, for example, because in different domains of application, the same representational tool can be used only with different idealizing assumptions and at different levels of abstraction (for an example of such different uses of the same tool, see Grüne-Yanoff, 2013). This might be due to reasons such as the differences in nature of the represented objects, the differences in heterogeneity in the populations, or differences in the quality of evidence in the different domains.

Attempts to justify the disciplinary diversity of methods and tools against the background of different contexts, problems and objectives are not new. Mill, for example, provided a contextual justification of the *a priori* and a posteriori methods. He argued that some sciences study causes that are decomposable, such that even if a study observes multiple causes in operation, the result of their joint action is the same as the sum of their separate individual actions. Thus, knowledge about individual causes can be generalized and formalized and used for deductions about special cases (Mill, 1884, 267). Other sciences, however, study causes that are not decomposable in this way. Such heteropathic sciences cannot formalize generalizations about individual cases and use them for

[6] A word of caution here. For this to follow strictly from our observation one would need to show that the alternative course of action (namely the integration) would have produced less success. It is not enough to simply show that some success occurred without integration (thanks to Caterina Marchionni for this point). Yet to obtain such evidence, we would have to rely on controlled experiments or suitable observational studies—evidence to which we sadly do not have access at this point.

deductive purposes, but instead must work with observations of ensembles of causes (Mill, 1884, 267).

Mill's distinction, we believe, is not of the right kind to be of much help when distinguishing and justifying disciplinary practices of today. Yet currently there is no consensus as to how to map the epistemically relevant distinctions between disciplines. The approach we consider particularly fruitful is to employ the notion of epistemic modeling virtues to characterize different disciplinary practices. Given that these epistemic virtues often stand in trade-off relationships (Matthewson & Weisberg, 2009), it is prima facie plausible that disciplinary identities and conventional practices form around different configurations of epistemic virtues of their representational tools, and that such differences are justifiable in the light of different contexts, problems and objectives.

7. Conclusion

Philosophers of science stand to gain from investigating episodes of model exchange. They provide unique insights into the diversity of scientific practices of different disciplines. These insights are crucial if we want to go on doing philosophy of science in a way that is close to disciplinary practices.

At the same time, philosophy of science that takes this practice-orientation seriously also has a contribution to make to broader multi-disciplinary studies of interdisciplinarity. Philosophy of science probes and establishes justifications of divergence in scientific methods and tools. Such investigations offer arguments against a naïve plea for unitarian or non-pluralist versions of interdisciplinarity. Disciplinary divergences may have their justifications and, consequently, promoting exchanges that require the reduction of these divergences may not be justified. Philosophy of science thus offers not only an empirical basis from which to investigate interdisciplinary exchanges with a special focus on methods and tools. It also offers a normative perspective on interdisciplinarity, based on appraisals of disciplinary divergence.

Both of these important projects, we argue, should draw on the evidence that episodes of model exchanges provide. The discussion of interdisciplinarity in the philosophy of science is still in its infancy. Our presentation of five philosophically informed case studies of interdisciplinary model exchanges will, we hope, inspire increased activity amongst philosophers of science to investigate this rich and promising topic.

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