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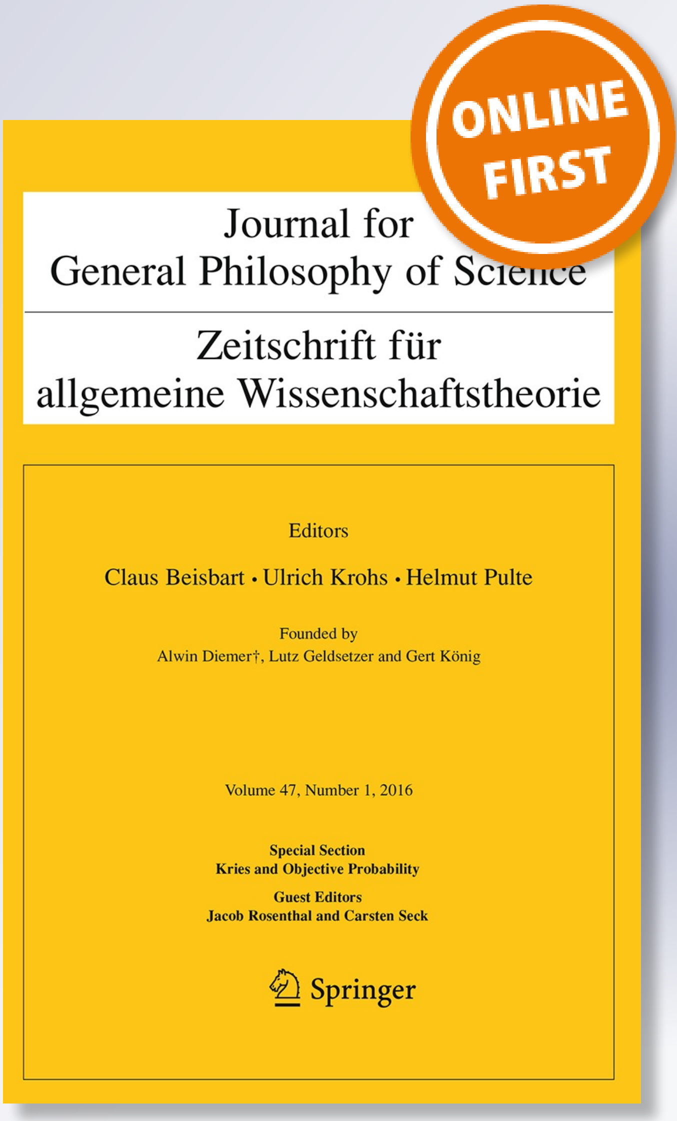

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ARTICLE

Interdisciplinarity as Hybrid Modeling

Rolf Hvidtfeldt[1]

Abstract In this paper, I present a philosophical analysis of interdisciplinary scientific activities. I suggest that it is a fruitful approach to view interdisciplinarity in light of the recent literature on scientific representations. For this purpose I develop a meta-representational model in which interdisciplinarity is viewed in part as a process of integrating distinct scientific representational approaches. The analysis suggests that present methods for the evaluation of interdisciplinary projects places too much emphasis non-epistemic aspects of disciplinary integrations while more or less ignoring whether specific interdisciplinary collaborations puts us in a better, or worse, epistemic position. This leads to the conclusion that there are very good reasons for recommending a more cautious, systematic, and stringent approach to the development, evaluation, and execution of interdisciplinary science.

Keywords Interdisciplinarity · Modelling · Philosophy of science · Scientific representation

1 Introduction

'Interdisciplinarity' has more buzz than most current scientific buzzwords, and indeed there are good reasons to believe that the combining of different scientific approaches is central to the processes through which we develop and expand our understanding of reality in the broadest sense. Undeniably, the history of science is rich with cases of successful scientific achievements more or less due to efforts which could be considered interdisciplinary in one way or the other. On the other hand, everybody has his or her favorite horror

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story featuring some specific, obviously misguided or even faux, interdisciplinary collaboration.

Curiously, however, very little effort has been put into the development of ways to distinguish between "good" and "bad" interdisciplinary collaborations. Especially the epistemic vices and virtues of interdisciplinarity are rarely and only cursorily discussed. A virtual discipline, which one might name *interdisciplinarity studies*, is devoted to studying interdisciplinarity as such, but in the related literature there are no measures, or even any apparent attempts to develop measures for the epistemic benefits of interdisciplinary collaborations (Aldrich 2014; Frodeman 2014; Frodeman et al. 2010; Hoffmann et al. 2013; Klein 1990, 2005). In many treatments of the topic of interdisciplinarity, it seems to be taken for granted that "something" is gained through interdisciplinary collaborations. But what exactly is gained, and perhaps whether anything is lost, remains unaddressed. In order to better understand whether and under which circumstances interdisciplinarity leads to beneficial results, we thus need to develop adequate tools of assessment more or less from scratch. To achieve this goal, we must first figure out what is actually going on when two or more disciplines are combined. For this purpose, many issues, of which I point out some in this paper, require considerably more attention than they usually receive.

The basic idea in interdisciplinarity is to combine two or more scientific approaches (loosely speaking) into an integrated approach. The motivation for this kind of scientific crossbreeding is that through the combination of different scientific approaches, it might be possible to construct some form of hybrid, which is somehow an improvement of (at least one of) the original inputs. In this paper, I present an analysis of hybridizations in science which takes as its starting point a confrontation with the notion that conventional taxonomies of disciplines provide a fruitful ground for analyzing combinations of scientific approaches. From this criticism I go on to suggest that a focus on activities of representation might prove to be more fruitful. I base my arguments on the assumption that central and important aspects of scientific activities are the products produced. "What are the products of science?", one might reasonably ask. This question can be answered in many ways, of course. For present purposes my answer is this: Most tangibly the products of science are the publications produced, but it is obviously the propositional content of these publications that are of interest. I assume in the following that the most central propositional content of scientific publications consists of presentations of novel ways of representing (more or less) specified phenomena by means of (more or less) specified vehicles of representation (often referred to as 'models'). Sometimes a publication includes presentations of novel vehicles of representation; sometimes the central idea is an application of an established vehicle of representation to an object different from what has traditionally been targeted by means of the particular vehicle of representation applied. Finally, sometimes publications are about the re-application of a previously presented vehicle of representation (perhaps with certain adjustments) to a previously targeted object in order to reassess its value or previous results (so-called replications).

My analysis below will be based on recent and ongoing discussions of scientific representation (Giere 2006b; Giere 2010; Godfrey-Smith 2009; Suarez 2003, 2004; Thomson-Jones 2012; Van Fraassen 2008; Weisberg 2013). When interdisciplinarity is analyzed in this way, several fundamental and problematic issues are revealed, which have not received appropriate attention in the extant literature on the topic—neither in the relevant philosophical literature, nor in the treatments within interdisciplinarity studies. Viewed in this perspective, it becomes evident that reflections focused on strictly epistemic issues should, to a higher extent, be included in the selection, development, execution, and evaluation of interdisciplinary scientific projects.

In the quite small existing philosophical literature on interdisciplinarity, perspectives from the philosophy of scientific representation are, more or less, absent. One can find interesting discussions of communication between scientists from different fields (Galison 1997; Holbrook 2013), discussions about implications from social epistemology for understanding interdisciplinary collaborations (Andersen and Wagenknecht 2013), discussions about whether philosophy is a necessary part of well-executed interdisciplinary collaborations or, indeed, whether philosophy itself is by nature interdisciplinary (Frodeman 2013; Fuller 2010; Hoffmann et al. 2013). These are all important questions worthy of attention.[1] But so is the question of how, and to what extent, interdisciplinary activities affect the products of science—the vehicles of representation produced and the ways these vehicles are used.

Patricia Kitcher's analysis of the failure of Freud's project of developing an all-encompassing science of mind is one of the best philosophical discussions of interdisciplinarity that I have come across. However, her approach is based on an extended historical case study, and, as she explicitly states, she does not engage in the sort of abstract analysis I am concerned with (Kitcher 1992, 4; 2007).

My approach in this paper is in many ways akin to the philosophical literature on scientific pluralism. In different ways philosophers such as Kellert (2009), Longino (2006), Cartwright (1999) and Giere (2006a) have advocated ideas along the line that (often) topics of interest "cannot be fully explained by a single theory or fully investigated using a single approach. As a consequence, multiple approaches are required for the explanation and investigation of such phenomena" (Kellert et al. 2006, vii). Further, as Kellert states: "A thoroughgoing disciplinary pluralism […] suggests that sometimes […] perspectives do not fit nicely together on the same plane: they overlap or conflict or cannot both be held at the same time, and yet both are needed to understand the phenomenon" (Kellert 2009, 38).

The question I pose is: what happens when one tries to integrate incompatible perspectives? It seems highly relevant to the discussion of interdisciplinarity to address issues such as under which circumstances specific perspectives are incompatible and what the consequences are if attempts are made at integrating such incompatible perspectives. Sandra Mitchell's position called *integrative pluralism* may at first appear to come close to what I claim to be missing, since she specifically addresses representational integration. However, Mitchell deals for the most part with the integration of various biological models at different levels of explanation, e.g. combining evolutionary models with genetic or ontogenetic models (Mitchell 2002; Mitchell 2003). In (1997), Mitchell, along with Gigerenzer et al., have indeed discussed the integration of models and transfer of statistical tools between biology and social sciences (see also Gigerenzer 2004; Gigerenzer et al. 1989). These are certainly fascinating and important discussions highly relevant to my present concerns. But biology and (at least certain parts of) the social sciences are in many ways much closer related than, say, literature studies and neurology. For an analysis of interdisciplinarity to be able to capture collaborations of the latter type, a different framework with a broader scope is required. Not only since the theories, tools, and methods used by participants involved in such collaborations are much more diverse, but also since participants can be expected to share background assumptions and theoretical insights to a significantly lesser extent than a group of biologists and sociologists aiming for an integrated, less idealized account.

I believe that if we are to do these questions justice, a good place to start is to dig deep into how scientific representation is accomplished and spell out general difficulties related

[1] I thank an anonymous referee for urging me to pay due respect to some of these authors.

to combining different (incompatible) perspectives which involve, as will be discussed below, individual representational distortions. There are no good reasons to assume that the combination of two distorted perspectives adds up to anything like an asymptotic approach to a non-distorted, non-idealized representation (Mitchell 2002; Wimsatt 1987).

The discussion in this paper constitutes a first stab at uncovering these (to some extent unapparent) difficulties involved in integrating two or more ways of representing phenomena. I am going to suggest that an analysis based on representational activities lends new meaning to the central notion of "integration" in the evaluation of interdisciplinarity. It incorporates the social factors, which have received most of the attention in the literature (Frodeman et al. 2010), while emphasizing the importance of the complex relations between vehicles of representation and the targets they are used to represent. It seems to be a reasonable requirement that these relations are at least included in the discussions of whether interdisciplinarity is worth pursuing in specific cases as well as in general. That this paper constitutes a first stab means that I do not pretend to be able to provide the reader with an exhaustive analysis of all consequences of the representational approach to interdisciplinarity or, for that matter, detailed studies of actual cases of interdisciplinary collaboration. Nonetheless, I believe that the criticisms and suggestions I present are reasonably put forward in this form at this stage, though they require further elaboration.

2 "Inter-" and "Discipline"

Everybody seems to agree that there are no commonly accepted definitions of terms such as 'multidisciplinarity', 'interdisciplinarity', or 'transdisciplinarity'—all considered to be variations of collaborations between people with different disciplinary affiliations (Klein 2010). Nevertheless, the term 'multidisciplinarity' is widely used in reference to situations in which two or more scientific disciplines are juxtaposed (perhaps in an effort aimed at solving a common problem) but without integration of the involved disciplines. Hence, the collaboration does not result in the development of any hybrid or novel approaches. 'Interdisciplinarity' refers to situations in which two or more scientific disciplines are somehow integrated during the process. 'Transdisciplinarity', then, refers to situations in which the effort in a quite comprehensive way transcends academia and involves people outside institutions traditionally focused on research.

In this paper I will focus exclusively on interdisciplinarity in the loose sense defined above. Let me initiate the discussion hereof with the following somewhat banal observation: The concept "interdisciplinarity" presupposes as a minimum that some sort of *inter*-action and *integration* between at least two relevantly different *disciplines* takes place. Further, a temporal aspect is implied. There is a pre-interaction state of affairs in which the involved disciplines are distinct, and there is a post-interaction, or integrated, state of affairs in which, unless the effort has been completely futile, some product of the integration of the involved disciplines has come into existence.

Unfortunately, the basic concept "discipline" is quite ambiguous. Is philosophy a homogeneous discipline? Is statistics a discipline distinct from the rest of mathematics? Do all the disciplines belonging to the humanities share common characteristics which set them apart relative to disciplines belonging to the natural or social sciences? One quite nebulous issue is, for instance, at what level of detail distinctions between different disciplines are to be made. How such questions should, or could, be answered is far from clear.

If the epistemic aspects of scientific hybridizations are to be adequately captured, there is a need for more detailed distinctions than the disciplinary boundaries, which are the result of historical contingencies or administrative divisions. An adequate representation of scientific hybridizations would require a way of drawing boundaries that aligns more closely with actual differences in scientific approach.

To reach a clearer appreciation of the relevant epistemic differences between scientific approaches as well as the processes involved in interdisciplinary interactions, I suggest focusing narrowly on scientific representation. It is a central claim of this paper, a claim that is endorsed by a large group of the most influential contemporary philosophers of science (Cartwright 1999; Giere 2006b; Suarez 2009; Van Fraassen 1980; Van Fraassen 2008; Weisberg 2013), that representation is *the* central scientific activity. The further claim I make is the following:

If it is the case that representation is the central scientific activity, and if interdisciplinarity has any significant effect on scientific practice, then the effect of interdisciplinarity must somehow be reflected in the post-interaction representational activities by the involved scientists.

In other words, it must be possible to single out important aspects of *the effects of interdisciplinarity* by comparing the representational activities in the pre- and post-interaction states of affairs of the research activities of the collaborators involved in any alleged instance of interdisciplinarity.

Whether or not it is reasonable to choose representation as the focal point for an analysis such as the present one depends on whether representation is central, not just in some sciences, but in a relevantly similar sense in all scientific activities that might be involved in interdisciplinary activities of the sort one intends to capture. In many cases, that means including scientific approaches traditionally categorized as belonging to the humanities and the health sciences as well as the natural and social sciences.

My position is that such an understanding of scientific representation is attainable without straining generally accepted conceptualizations beyond coherence. Indeed, I believe that many philosophers engaged in the debate on scientific representation would agree, though they rarely, if ever, discuss scientific representation in, say, the humanities. As an example, in his seminal work on scientific representation, Bas van Fraassen states the following:

> Scientific representation is not exhausted by a study of the role of theory or theoretical models. To complete our understanding of scientific representation we must equally approach measurement, its instrumental character and its role. I will argue that measuring, just as well as theorizing, is representing. (Van Fraassen 2008, 2)

For the present purposes I stretch "representation" even further. As is common in philosophy of science, van Fraassen focuses on the most prestigious natural sciences (arguably, physics, chemistry, and biology are the places to make your mark if you want to "be someone" in contemporary analytical philosophy of science). But as just mentioned, attempts to introduce aspects of methodology from the natural sciences in, e.g., the humanities are abundant and for analyses of such integrations to be adequate, a level of abstraction is required at which the relevant aspects of all (potentially) involved disciplines are incorporated.

I claim that the categorizations belonging to disciplines in, for instance, the humanities can, at an appropriate level of generalization, reasonably be considered to be equivalent to the measurements of the quantitative sciences. The concepts of, for instance, literature theory are presumably less stringent and less well coordinated than the measurements of thermodynamics. But nevertheless, literature theorists use the concepts of literature theory to indicate that the conceptualized target has certain characteristics and plays a certain role in a larger theoretical scheme. Thereby, literary concepts fulfill the most basic requirement of van Fraassen:

> There is no representation except in the sense that some things are used, made, or taken, to represent some things as thus or so. (Van Fraassen 2008, 23)

This is exactly what literature theorists do: They use some things to represent some other things (e.g. certain concepts used to represent characters in a novel or vice versa) as 'thus or so'. Bas van Fraassen states that if he were to propose a theory of scientific representation, which he stresses that he has no intention of doing (sic!), the above quote would be its *Hauptsatz*.

In the philosophical literature on representation, 'models' is the standard term for denoting *the vehicles by which other things are represented* (Cartwright 1983; Giere 1999a; 2006b; Van Fraassen 1980; Van Fraassen 2008)[2]. I follow this trend, but in much the same vein as I claim that conceptualizations are equivalent to measurements considered at the right level of abstraction, I also construe the category of modeling to incorporate representational practices in all kinds of disciplines. That is, literature theorists, psychologists, and philosophers construct and use models on an equal footing with physicists and biologists according to my use of the term 'modeling'.

This is in stark opposition to a widely held position in which 'model' is conceived as short for 'mathematical model' and therefore exclusively connected to the quantitative sciences. I agree on this issue with Thomson-Jones' (2012) argument in favor of a propositional view of modeling according to which most (perhaps all) mathematical models are somehow embedded in sets of propositions (sometimes, perhaps, in combination with tacit knowledge which may be skill-like, see Collins 1985). These sets of propositions may for instance indicate how the mathematical structures of the model relate to its target system(s). On the other hand, many (non-mathematical) models consist solely of sets of propositions. The propositional view on models is especially useful in relation to an analysis of interdisciplinarity in which one needs a way of construing the vehicles by which 'things are represented' that encompasses various divergent scientific approaches. Thus, I use 'model' in this perhaps controversially broad sense to refer to all vehicles by which other things are represented as part of a scientific effort. Consequently, concepts as well as measurements are to be understood as special kinds of models along with mathematical models.

Once this somewhat controversial move is accepted (at least for the sake of argument), the next step, of course, is to attempt to spell out what these underlying propositional structures consist of. I suggest that a fruitful way of construing at least important parts of these structures would be in terms of something like *propositional algorithms*, (i.e. more or less explicit sets of rules for carrying out certain conceptual operations). I will return to and attempt to clarify this issue below.

Van Fraassen's main point with his *Hauptsatz* is the central importance of understanding representation as an activity and not as a simple (truth-)relation between, e.g., linguistic units and aspects of reality. For A to be a model of B someone has to *use* A as a model of B (in an act of representation) and thereby indicate that B is to be thought of in a

[2] By no means do I mean to suggest general agreement on this issue, though. See Weisberg (2007) for recommendations of a more restrictive use of 'model' and for the suggestion that scientific representation comes in several forms, some of which do not involve modeling at all.

certain way. Since nothing in itself represents something else, it is necessary to specify which elements of the model are taken to be similar to the target and in which way (Giere 2010).

To borrow an example from Ronald Giere (1988, 70 ff.): $P = 2\pi\sqrt{l/g}$ is a mathematical model commonly used in physics textbooks to represent a certain aspect of the movements of a pendulum. The equation fits the experimental results of Newton and Galileo, which showed that the period (P) of a pendulum is proportional to the square root of its length (l) divided by the gravitational constant (g), and that the period is independent of the mass of the bob (which is, as a consequence, not represented).

The above equation can, given the required specifications, be used as a strongly idealized model of the movements of a pendulum, but a pendulum could also be used as a model of the equation, e.g. as a clarifying exemplification. In any given context it is exactly *the use* that determines whether the equation is a model of the pendulum or vice versa.

The focus on *use* and *representation as an activity* thus makes clear why representation is asymmetrical and consequently cannot simply be a question of similarity or resemblance between target and model.[3] Further, it provides a solution to another issue that would seriously threaten a similarity-based construal of "representation". If representation were a question of similarity between a model and a target system, one might reasonably wonder what level of similarity would be sufficient for a relation between a model and a target system to be a *scientific* representational relation. Is a geocentric construal of our solar system sufficiently similar to its target to count as a scientific model of it, or is heliocentricity required? Few would probably be prepared to say that Aristotle did not represent the heavenly bodies in his cosmology, or for that matter that his works were not representational in at least a proto-scientific sense.

The focus on use dissolves the problem of figuring out what level of similarity is sufficient for counting as a scientific model since there are no minimum requirements apart from someone using A to represent B for some illustrative (scientific[4]) purpose. I might actually use my pencil and an open-faced cheese sandwich to illustrate some difficulties concerning the landing of a lunar module on the moon (the holes in the cheese might neatly represent some of the deeper craters in which it would be unfortunate to land). This might sound odd at first, but it actually opens up a lot of possibilities, which will serve us well if we attempt to analyze the integration of representations from very different scientific approaches. It is especially important to allow ourselves to include bad modeling in the discussion of scientific representation, since we need to be able to figure out whether interdisciplinarity may be instrumental in improving poor science. Really bad and immature science, the kind of science one would suppose could benefit the most from interdisciplinary collaborations[5], would be excluded from consideration if strict similarity were chosen as the criteria for scientific representation.

[3] … which has been an intensely debated topic! (Chakravartty 2010; Giere 2004; Goodman 1976, 3 f.; Suarez 2003; Suarez 2004; Van Fraassen 2008, 17 f.).

[4] At this point some might think: "Hey wait, don't we need a clear-cut definition of science so that we know exactly what we are talking about?" My answer is: "No." We are dealing with a large group of diverse activities which are all more or less similar to activities in the prototypical sciences ("proper sciences" some might wish to say). Definitions are good for ruling things out, but that is not the kind of business we are in right now.

[5] It should not be very controversial to claim that an experienced scientist from a more mature science would know a few tricks of the trade and be able to point out some basic mistakes to avoid.

3 Idealizations

To elaborate a bit further on the equation for the movements of the simple pendulum, note that the equation is far from an exact description of how an actual physical pendulum moves. In the model, it is presupposed that the bob of the pendulum is a point mass that moves without friction in a massless suspension. Furthermore, the equation only deals with the horizontal movement of the pendulum, for which reason it is presupposed that the amplitude of the swing is very small, so that the vertical movement is reduced to a minimum (Giere 1988, 70 ff.). Notice also how important the introduction of additional specifications and explications are to the use of the above equation as a model of a pendulum. Without these, one might have some problems figuring out how the one is supposed to represent the other. Even with the appropriate mathematical insight one might reasonably claim that the equation does not specifically resemble a pendulum more than any other harmonically oscillating system (which is part of the reason why a pendulum could reasonably be used as a model of the equation).

From the above we can already conclude that the mathematical model of the simple pendulum is quite strongly idealized. Point masses and massless suspensions are prototypical instances of what in the literature are called Galilean idealizations (McMullin 1985). No such things exist in physical reality. One might easily realize that the equation is further idealized (in the Aristotelian sense of idealization in which some properties or elements are omitted from representation leaving only the "essential" ones) in that it does not include aspects such as the materials of which the pendulum is made [the choice of material of the bob does, however, matter in relation to how much the movement of the pendulum is affected by the earth's magnetic field, while the length of the suspension may vary (to different extents depending on the coefficient of thermal expansion of the materials used) as a result of changes in temperature]. Moreover, this model in no way takes into account the moving pendulum's reflection of light, its history (e.g. the role played by pendulum movements in the determination of the standard meter), or its potential hypnotic effect. Consequently, one must conclude that the equation of the simple pendulum is far from an exhaustive description of the movements of physical pendulums—no one, of course, claims that it is.

An examination of a quite different model will show that these characteristic representational idealizations are repeated across traditional disciplinary boundaries. The *diathesis-stress* model is an example picked from psychopathology. The model is used in attempts to cast light on why some people develop pathological mental disorders when placed in certain situations which others appear to be able to cope with without similar consequences (Ingram and Luxton 2005).[6]

'Diathesis' here refers to a person's level of vulnerability and may be understood as the opposite of resilience. 'Stress' is to be understood in a broad sense, as any major or minor life event that disturbs the stability of a person's physical, emotional, or cognitive mechanisms. In the simplest version the diathesis-stress model has only two parameters: vulnerability and accumulated stress. According to the model, these determine if the person suffers "a breakdown" and develops a mental disorder.

Graphically, the diathesis-stress model can be represented as in Fig. 1.

[6] Diathesis-stress-models come in many more or less specified versions and goes back at least to Pierre Briquet's systematic studies of hysteria in 1859 (Ellenberger 1970, 142). More recently Paul Meehl's discussion of Schizophrenia is a well-known example (Meehl 1962). Presently I make use of a very abstract version of the diathesis-stress-model since it provides the best basis for comparison with the model of the simple pendulum.

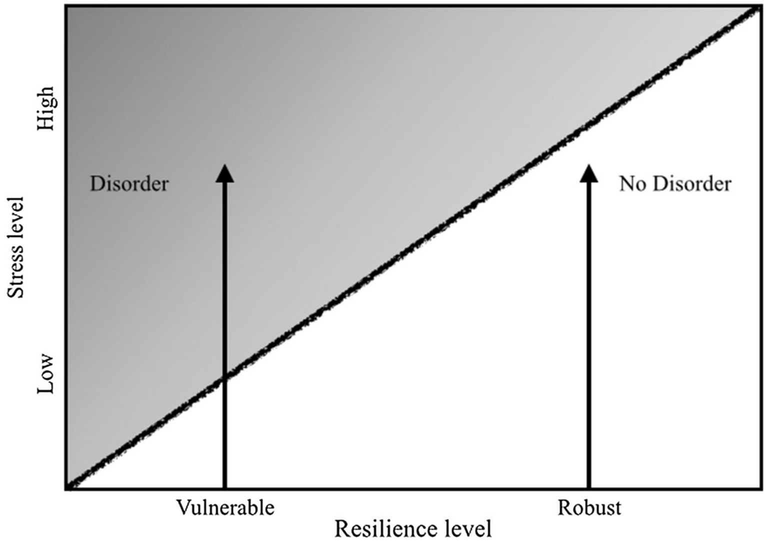

Fig. 1 The diathesis-stress model

Much like the equation for the movements of a pendulum, the diathesis-stress model is strongly idealized. The model is focused upon an alleged isolatable and generalizable aspect of reality, i.e. the relation between the tendency of human beings to develop mental disorders and their exposure to stressors. The two parameters, "diathesis" and "stress", are also strongly idealized (in the Galilean sense). Vulnerability seems to be a very dynamic and complex phenomenon, quite far from being a static innate threshold. Similarly, stress can hardly be claimed to accumulate in any simple way through a person's life until a "critical mass" has been reached, and a mental breakdown occurs. The breaking of a leg can, of course, be a significant stress factor. But it seems reasonable to claim that the resulting stress decreases as physical functionality is regained. Similarly, at least in many cases, the stressfulness of emotionally traumatic events, such as the divorce of one's parents or the loss of a loved one, tend to decrease over time. And even though one might develop a persistent hypersensitivity in situations that are somewhat reminiscent of the one in which the original crisis occurred, this by no means indicates a simple accumulation of stress over time.

Furthermore, the diathesis-stress model is idealized (in the Aristotelian sense) since several (relevant?) factors are left out of the equation (so to speak). Helpful social relations might significantly reduce the impact of a given stressor, so might the acquisition of coping-strategies. And, as the old wisdom goes, the overcoming of previous stressful situations might actually serve to strengthen one's ability to combat novel trials.

The diathesis-stress model and the equation of the simple pendulum are two very different models. They nevertheless share certain patterns, which make them comparable as potential vehicles of representation. The main difference between the two models is that the equation of the simple pendulum, obviously, has a mathematical structure doing a significant part of the work, while the diathesis-stress model involves no apparent mathematical tools (though it is sometimes, as above, somewhat deceptively depicted as a linear function). But the models are also similar in the sense that their use in both cases require quite large sets of more or less explicated sets of propositions and propositional structures (e.g. entailment relations, rules for idealization, definitions etc.), which has the function, among others, of pointing out which aspects of the world the model is supposed to be about. It is quite obvious that the model of the pendulum entails much more specific claims about (aspects of) how an actual pendulum is supposed to move in order for the model to fit its target than does the diathesis-stress model. We might say that the pendulum-model is a stronger model than the diathesis-stress model in that it makes much more specific claims about reality. The diathesis-stress model is no less a model, however, even though it is somewhat feeble.

Reflection on the above examples reveals important tensions between the concepts of "specificity", "idealization", and "explication". It is easier to construct exactly specified models if its elements and internal relations are strongly idealized, but strong idealization makes it more demanding to explicate the relation between the model and the phenomena it is about.

Michael Weisberg's notion of what he calls the *construal* of a given model can add useful nuance to the discussion at this point (Weisberg 2007). In Weisberg's terms, the construal of a model involves the *assignment* of which parts of the model to be used in a given act of representation. This assignment also involves explicitly pointing out which aspects of the model are to be ignored in its present use. Next, the specification of *scope* is Weisberg's term for the pointing out of which aspects of the targeted phenomena are supposed to be represented by the model. In my use, 'specificity' denotes the level of exactitude of assignment and scope in the construal of a model, i.e. a measure of to what extent the assignment and scope is clearly identified. By 'explication' I refer to the process of making clear the construed internal relations in the model as well as the relations between the model and the target (including the involved processes of idealization). Finally, Weisberg operates with two *fidelity criteria*, which point out how tightly the model must fit its target to count as adequate. *Dynamical fidelity* concerns to what extent the model succeeds in predicting the behavior of the target system. *Representational fidelity*, on the other hand, concerns whether such predictive success is achieved for the right reasons, i.e. whether the structure of the model actually fits the causal structure of its target. A demand for high *representational fidelity* would obviously be much stronger than a demand for high *dynamical fidelity*.

In the ordinary use of the model of the simple pendulum, it is made very specific what its elements are and which parts of which phenomena they are used to represent. Further it is made very explicit which relations are taken to hold between the elements of the model. This specificity, however, very much depends on the high degree of idealization of the model's internal relations and elements. In the case of the diathesis-stress model the elements of the model and the relations between them are less clearly specified and explicated, though still highly idealized. Importantly, it is worth noting that the literature on simple pendulums is also very explicit about the idealizations used in the model (Giere 1988, 69 f.). This is unfortunately not the case in the treatment of diathesis-stress models, in which the transformation from real-world phenomena to idealized elements of the model is left in the dark (Ingram and Luxton 2005). The consequence is that while the model of the simple pendulum is highly idealized its elements are still tightly tied together, and further, the elements are tightly tied to the phenomena the model is used to represent. In other words, it is a Hi-Fi-model. In the case of the diathesis-stress model, all connections are quite loose, which, among other problems, makes it difficult to conclusively decide whether the model is empirically adequate. In Weisberg's terms, we might simply be unable to determine whether the model lives up to its fidelity criteria (if such had been worked out in the first place).

In the next section, I will attempt to show how I believe the thus enhanced, or at least broadened, conceptions of “model”, “modeling” and “representation” can be put to work in the analysis of interdisciplinarity considered as instances of hybrid representations.

4 Representational Crossbreeding

The following also builds on van Fraassen’s *Hauptsatz*: Since representation is an action, a representational relation will always involve an agent: *Someone* uses A to represent B.

Ronald Giere operates with a very similar agent- and action-based conception of scientific representation, though he stresses the need for a fourth element: *purposes* (Giere 2004; Giere 2010). Giere suggests the following 4-place-relation as the minimal requirement for an analysis of a scientific representational relation:

S uses *X* to represent *W* with the purpose *P*

This formalization is to be understood in the following way: A scientist, or a group of scientists, (*S*) uses something (*X*) to represent an aspect of reality (*W*) for one or more specific purposes (*P*) (Giere 1999b; Giere 2004).

It is of course important to get clear on what can take the place of the variable (*X*). Giere’s position is this:

> So here, finally, we have a candidate for the *X* in the general scheme for representation […]: Scientists use *models* to represent aspects of the world for various purposes. On this view, it is models that are the primary (though by no means the only) representational tools in the sciences. (Giere 2004, 747)

So, *X* is a placeholder for models—what about *W*? As many a philosopher of science, Giere is mainly occupied with understanding the sciences of physical phenomena. But it is worth emphasizing once again that nothing prevents us from expanding our understanding of *W* to include all kinds of phenomena, as for instance the friendship of Watson and Holmes, the influence of meteorological circumstances on the choices of color among impressionistic painters, as well as the movement of a pendulum or the function of a Higgs-field.

Armed with Giere’s formalization of the representational relation and the propositional view on modeling, we are in a good position for singling out the specifically epistemic aspects of interdisciplinarity. If we suppose that scientific approaches can be characterized by how and what they represent, we can understand interdisciplinarity as the combination of two or more representational relations.

Let us assume, for the sake of simplicity, two distinct groups of scientists using distinct models to represent distinct aspects of the world for distinct purposes. In the pre-integrated state they could, based on Giere’s four-place-relation, be represented in this way (Fig. 2):

This way of modeling pre-integrated representation is obviously strongly idealized in a number of ways. First, it is unlikely to find a group of scientist operating with only a single model, which they apply to only a single aspect of reality. Second, the relation between *X* and *W* is far less simple than displayed in Giere’s four-place-relation. The relation between *X* and *W* is often mediated through layers of treated and corrected data-models and generalizations, and there is, hence, most often *not* a direct relation between model and world (Suppes 1962; Van Fraassen 2008). Third, as discussed above, the models are embedded in a web of more or less implicit propositions, which, among other things, serve to relate the models to the aspects of reality they are about. Thus, there are a lot of quite central aspects of the relation between *X* and *W* that are entirely left out of Giere’s model of representation. Fourth, it is in itself an idealization to isolate the relation between *X* and

Fig. 2 The Giere duplex

S_1 uses X_1 to represent W_1 for the purpose P_1

S_2 uses X_2 to represent W_2 for the purpose P_2

W from the involved *S's* and *P's,* as I will do below (probably, to the dismay of those who would give *S* and *P* first priority). Nevertheless, I believe it is beneficial initially to consider interdisciplinarity in this very simplistic way, so I will stick with Giere's basic four-place-relation for now, only to return to some of the mentioned complications and their consequences below.

The duplex version of Giere's representational relation leaves us with four obvious parameters for integration. *S*-integration and *P*-integration are thoroughly dealt with in the extant literature on interdisciplinarity (Frodeman et al. 2010). The strong focus in the literature on the social and purpose-related aspects of interdisciplinary integration, however, seems to partly occlude the complexities of *X*- and *W*-integration.

In the case of *P*-integration, the cynic might be tempted to claim that an obvious common purpose for two groups of scientists engaging in interdisciplinary collaboration might simply be to obtain funding. The initiations of interdisciplinary collaborations are, indeed, probably sometimes motivated by the need for funding, not least since interdisciplinarity is often almost a formal requirement for approval. But such sarcasm might nevertheless be somewhat misplaced. According to Giere, at least, funding is not a representational purpose of the relevant type. In Giere's construal, 'purpose' denotes the goal which the specific representational activity is intended to achieve, such as Watson's purpose of representing the physical structure of DNA (Giere 2004, 749). The set of purposes to which funding belongs is a matter for economical, psychological and sociological, rather than representational, analysis. The purposes discussed in the interdisciplinarity studies literature are more along the lines of "we should do a lot of interdisciplinary collaboration *because* then we can solve a lot of complex problems" which is yet another version of 'purposes'. Bottom line is that 'purpose' is used in many different senses, which needs to be kept apart in order to avoid confusion.

S-integration is a question of unifying otherwise distinct groups of scientists. How is that done? At first glance *S*-integration may seem quite simple: The two (or more) original groups must be united (to some extent). The involved scientists may be physically placed in the same building, perhaps complete with a plate on the door saying "*The Interdisciplinary Center of XYC*", or, less thoroughgoing, administratively placed under a common leadership (perhaps, if denied a door-sign, at least provided with a website). Nevertheless, in many cases the interdisciplinary group will be provided with some money, which they are to spend on carrying out their interdisciplinary collaboration.

As indicated, *S*- and *P*-integrations are, though far from straightforward, carefully dealt with in the literature. Put in somewhat provocative terms, there is little epistemic challenge in agreeing on a common representational purpose or which people to invite for participation. Interestingly, it is in many cases exclusively along these two parameters that interdisciplinary projects are evaluated. As one example, one may consult the *Guidance for evaluators of Horizon 2020 proposals*—Horizon 2020 being the EC's 70 billion € research initiative (EC 2014). In this guide it is made clear that "fostering multi-actor engagement"

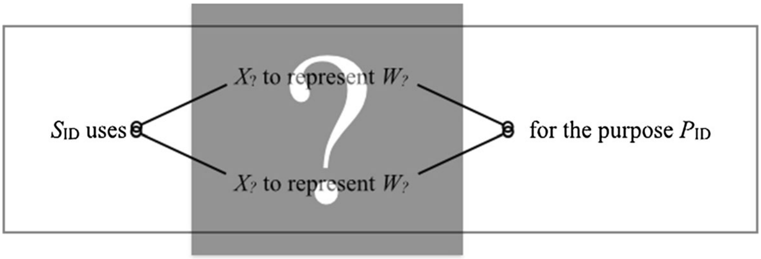

Fig. 3 The blackbox

alongside issues such as the nationality of as well as the male-to-female ratio among participants are important criteria in the evaluation of research proposals. Moreover, it also specifies which kinds of scholars are expected to participate. Interdisciplinarity is meant to contribute to the 'quality' and 'excellence' of the research proposal, but no criteria for evaluating epistemic aspects of this contribution are provided.

In such cases, the way interdisciplinary projects are evaluated administratively completely circumvent the epistemically substantial aspects of scientific collaborations. The interesting and difficult central epistemic issues in the representational relation are placed in an academic and administrative black box, as illustrated in Fig. 3.

This leaves the two philosophically most interesting parameters of the duplex version of Giere's model of representation unaddressed, and consequently little, if any, attention is paid to the most central epistemic aspects of interdisciplinary collaborations. In the next part, I will attempt to explain why it is a grave mistake to disregard these aspects of interdisciplinarity.

5 Problems of Integration

Integrating two or more approaches to scientific modeling is a tremendously complex process. There are innumerable ways in which such combinations can be made. I do not suggest that I am able to deliver an exhaustive analysis in this paper. What I will attempt to do, though, is to provide some illustrative exemplifications and point out some of the causes of the great complexity of such integrations.

With a class of models as inclusive as the one I stipulated above, one ends up with an enormous diversity with regards to structure, types of explanations used, degrees of universality or individuality, level of specificity etc. This diversity inspires a lot of questions. One obvious question is whether there are any limitations as to which types of modeling are possible to integrate. It is difficult to see principled limitations here. There is nothing that hinders the construction of models that combine elements from chemistry, biology and, perhaps, psychology or literature theory to ascribe agency to molecules or bacteria, for instance.[7] Obviously, there are relevant questions of adequacy, validity, power of

[7] I thank an anonymous referee for bringing to my attention, by criticizing me for using imagined examples of metaphorical use of psychology to understand bacteria, that it is too subtle to support this claim with only a reference to *The Pasteurization of France* by Bruno Latour. With an enchanting touch of modesty Latour himself refers to this book as his "Tractatus Scientifico-Politicus" (Latour 1988, 7). By consulting this seminal and explicitly interdisciplinary work within actor-network theory, one will be able to see that the ascription of agency to the tiniest of non-human "actors" is (1) apparently *not* meant metaphorically, and (2) *not* something that I have imagined!.

prediction and so on. But these are questions related to the evaluation of the resulting models, not to the possibility of constructing them (Collin 2011; Latour 1988).

Because models are embedded in propositional complexes, it is possible, and sometimes fruitful, to transfer propositional structure from one propositional level of one model to the same, or even a different, propositional level of another model. What do I mean by that? Nothing prevents one from transferring, for instance, mathematical structure from a physics model to an economical model. For instance, Philip Mirowski (1989) argues energetically how modern economics came into existence as a transferal of mathematical structures from the field of energetics into social science. But it is not necessary to transfer concrete mathematical equations; one might just import a principle such as the principle of conservation or, at an even more general level, the impression that complete quantification is a beneficial regulative principle. Thus, in some interactions, it is parts of the propositional structures of a certain way of modeling that are transferred.

In the ideal case, it may sometimes be that a specific way of representing contains elements and structures which might cast light on an underdeveloped aspect of another act of representation. A good example can be found in the history of evolutionary biology, where Darwin had postulated inheritance and variation as central parts of his model of natural selection without being able to explain the underlying mechanisms. The publication of Dobzhansky's *Genetics and the Origin of Species* very successfully provided the missing link [pardon the (intended) pun] (Dobzhansky 1937). Without doubt, the introduction of genetics added considerable strength, nuance, and accuracy to evolutionary biology.

6 Operational Definition

To illustrate one problematic aspect of importing smaller parts of theoretical structures, let me mention the introduction of operational definition in the *American Psychiatric Association's* diagnostic manuals since the so-called DSM-III (American Psychiatric Association 1980; its present version is the DSM 5, released in 2013). Operational definition is a clear example of what I would call a propositional algorithm.

Definitions are commonly used as conceptual tools for making clear what one is referring to in a given context. There are various types of definition, which are used more or less explicitly in different scientific settings (e.g. operational definitions, stipulative definitions, explicative definitions, lexical definitions). In some scientific contexts you might not need definitions at all, in other contexts they are crucial. The problem addressed by means of definitions is the need to decide whether or not some phenomenon belongs to a certain category. Definitions will, as a consequence, often play a significant role in representational activities.

I suggest that we think of definitions as essentially algorithmic structures, i.e. as sets of rules to be followed in conceptual procedures. What I refer to by 'propositional algorithm' in the following is the structure of a certain type of conceptual procedure. Significantly, the propositional algorithm of operational definition does not in itself serve to represent anything. It is not a model, but rather a tool deliberately constructed to point out phenomena to assign a specific role in a given act of representation. The algorithm thus serves as part of the crucial process of connecting *X* and *W*.

My point in the following example is first to show that propositional algorithms are candidates for being transferred during interdisciplinary integrations on a par with other elements involved in modeling. Further, I want to show that it is necessary to explicitly address under which circumstances propositional algorithms perform well, and to which extent they can be expected to continue to do so when placed in different theoretical contexts. At least in the following example, it should be clear that the propositional algorithm of operational definition is ill suited to perform the task it is used for in psychopathology.

Operational definition is a notion developed by the physicist and 1946 Nobel laureate P.W. Bridgman who introduced the idea in his book *The Logic of Modern Physics* (1927). Bridgman was motivated in part by the general commotion caused by the theory of relativity, as well as by specific challenges he faced as part of his work in *high-pressure physics*. The central, and radical, thought Bridgman put forward was that any meaningful theoretical concept should be defined by a set of operations. The outcome of carrying out this set of operations would decide whether the concept applied in the specific situation or not. Bridgman believed (at first) that this was a fruitful method for replacing vague and intangible concepts (Bridgman 1927, 5; Bridgman 1954).

The introduction of the use of operational definition in psychopathology is usually traced back to a paper presented by Carl Hempel at a conference on psychiatric nosology in 1959. To take Hempel's example, one might operationally define "harder than" in the following way:

> […] a piece of mineral x is to be called harder than another piece of mineral y, if the operation of drawing a sharp point of x under pressure across a smooth surface of y has as its outcome a scratch on y, whereas y does not thus scratch x. (Hempel 1961, 8)

The American Psychiatric Association, as a response to a series of unpleasant challenges (Cooper et al. 1972; Rosenhan 1973) combined with a general desire to punch psychoanalysts in their noses, chose to base their diagnostic system on operational definitions in the 1970s.[8]

Hempel himself pointed out some problems inherent in this approach. In his 1959 paper, Hempel made clear that one needs to base one's definitions on a foundation of concepts which require no further definition in order to avoid the obvious danger of regress. Hempel emphasized that not all concepts will serve equally well as the regress-stopping "certain" foundation on which operational definitions may be based. As a solution to this problem, Hempel recommended basing operational definitions in psychopathology on "antecedently understood […] terms […] used with a high degree of uniformity by different investigators in the field" (Hempel 1961, 11). "Theory-free" layman concepts (i.e. concepts allegedly free of etiological assumptions) were chosen to play this important part in the subsequently developed psychopathological system.

Now, there is a huge difference between the reliability, stability, and conceptual coordination between basic concepts of contemporary physics (e.g. 'temperature', 'length', 'velocity' etc.) and the concepts that serve as the inputs into the operational definitions in psychopathology ('inattentive', 'uneasy', 'nervous', 'worried'). Remember, it is not the case that these terms are used in special "specialist's senses", which simply recycle lay terminology. Indeed, to a large extent it is genuine laymen (patients, parents, schoolteachers, etc.) who carry out diagnostics in contemporary psychiatry by assessing various difficulties via multiple-choice tests.

This, then, is a story about transferring a propositional algorithm from one scientific setting to another. The propositional algorithm is not changed during the process, but the inputs fed into the algorithm are of very different kinds in the two settings. The algorithm of operational definition is well suited to define or redefine central basic concepts if fed

[8] This is a very simplified version of this story. Consult Fulford and Sartorius (2009) for a more elaborate exposition of what actually happened.

accurate and reliable inputs. On the other hand, this algorithm is *not* suited for defining complex concepts by being fed vague and indeterminate inputs. Operational definition has its fair share of problems in physics. But in physics, operational definition is virtually built directly on bedrock, as opposed to the soft soil on which psychopathology rests.[9]

Operational definition is an example of what I would refer to by 'propositional algorithm'. In similar ways, one might spell out algorithmic structures for different ways of observing, doing experiments, modeling data, analyzing, manipulating statistics, conceptualizing, constructing models, and so forth. As has been the focus in the quite comprehensive literature on physics, some of the involved steps may involve skills for, e.g., constructing and using technological equipment (Collins 1985; Galison 1997). Similar arguments can be made for other sciences. For instance, in psychopathological research skills regarding establishing rapport with patients (or making them participate at all) are extremely important parts of determining targets for representation in the first place. But even though there might be some parts of such processes which are skill-like in a non-propositional way and therefore cannot be easily communicated, a very large and significant part can (with some effort) be spelled out, I believe. If this were not the case, the efforts put into publishing scientific results would be in vain, it seems.

This hopefully gives an impression of how I construe the propositional algorithms constituting (some of) the underlying propositional structures of scientific representation, namely as a series of, if not specified then at least to a large extent specifiable, procedures one goes through in order to connect X and W. By analogy, one might think of the involved propositional algorithms as having specific functions like the different parts of a car engine. There are different types of car engines that require different types of parts to perform the overall function, i.e. to convert energy into motion. Some of these parts are interchangeable, while others are important in one type of engine and superfluous in others (e.g. sparkplugs without which a gasoline engine will not work, but which serve no purpose in a diesel engine). Similarly, some conceptual or mathematical tools might be indispensible in some scientific approaches, but superfluous in others; transferable between some scientific approaches, but not others.

As mentioned above, it is hard to see principled limitations as to which of these algorithms might be combined. So a central part of an analysis of an interdisciplinary collaboration along these lines will be to spell out the (most significant and distinctive) propositional structures at play, and the functions they perform in the pre-interaction approaches. Doing this will enable one to probe which of these structures, if any, are combined in the post-interaction states of affairs.

7 Integrating Targets

The third parameter for integration, W-integration, involves a common object of study. Of the four parameters in Giere's representational relation, integration at this parameter is probably the one that seems most straightforward at first glance. Isn't it simply a question

[9] In these years, there is a small but growing appreciation within psychopathological research that the introduction of operational definition bears a large part of the responsibility for the slow rate of scientific progress and the explosion in the numbers of diagnoses as well as people diagnosed with psychiatric diseases over the last decades (Frances 2013; Hyman 2011; Parnas 2013). For a pessimistic assessment of the state of affairs in *clinical* psychiatry however, see Sato and Berrios (2001). For interesting studies of how operationalized diagnostic systems undermine conceptual and scientific accuracy see Fried and Nesse (2015); Jansson et al. (2002); Jansson and Parnas (2007).

of scientists belonging to different disciplines applying their different perspectives to a common object and thereby reaching an enriched, deeper, broader, more accurate, and more nuanced appreciation of the common object? A little consideration will show that things are not that simple.

Interdisciplinarity (as well as multidisciplinarity, of course) is often understood as a process in which various approaches and different perspectives are applied to a common object. This raises the question of whether one can claim that "an object" observed in two or more ways is in a relevant way "the same" object, and further, whether what is observed must be "the same" object for the result of the process to contribute constructively to the generation of knowledge in one or more of the involved disciplines.

Such considerations may seem unduly academic. But since it is widely accepted[10] today that all observations are to some extent theory-laden (Hanson 1958), it is a reasonable question whether two different approaches are able to capture the same object.

Let us return to contemplating the movements of a pendulum. Kuhn has argued that if Aristotle and Galileo had observed the same pendulum in movement they would have registered distinct phenomena. Galileo would have measured period, length of the suspension, and amplitude, whereas Aristotle would have measured weight, elevation above ground, and the time it would take for the pendulum to reach rest. Would Galileo and Aristotle have had a common object (Kuhn 1962, 123)? It seems reasonable to argue, as Mitchell does, that differently idealized models do not target the same ideal system, even though they may ultimately center on the same real world phenomena (Mitchell 2002, 66).

Here we are touching upon Kuhn's classical discussion of incommensurability. But in spite of the abundant commotion caused by Kuhn's considerations in- as well as outside of philosophical circles (Davidson 1974; Hacking 1983; Putnam 1975; Shapere 1966) and the grimness of the problem if Kuhn were right, the issue has received relatively little attention in discussions of representation—and perhaps for good reason. Perhaps the focus on representation takes us to a level of detail at which the problems of incommensurability crystallizes into specifications of the way in which a given model is idealized. In fact, Kuhn's own discussion of the incommensurability of the Galilean and the Aristotelian understanding of pendula seems to be a perfectly good example of an explication of two different ways of representing one and the same (type of) phenomena.

Thus, there is reason to believe that incommensurability in the classic sense is less of a problem for interdisciplinarity (given adequate explication) than one might have initially supposed. But this does not mean that hybrid representation is deemed unproblematic. Indeed, insights gained from the study of representation bring forth a row of issues not easily overcome.

8 Distortions

Giere's and Van Fraassen's discussions of scientific perspectives (Giere 2006b; Van Fraassen 2008) highlight how representation is selective in the sense that certain aspects are emphasized at the expense of others. This results in various forms of distortion. Generally, it is a central part of their analyses that representation always involves distortion. In the words of Van Fraassen: "It seems then that distortion, infidelity, lack of resemblance in some respect, may in general be crucial to the success of a representation"

[10] Except, perhaps, in psychopathology….

(Van Fraassen 2008, 13). In the following, I will attempt to show why distortion is even more significant when considering interdisciplinarity.

The overall goal of modeling activity exactly is to emphasize certain aspects, to make some point or show that some aspect is particularly significant. When we represent a person as suffering from schizophrenia we are not interested in his or her digestion. Therefore, processes of digestion are excluded from most models of schizophrenia, because we want to focus solely on issues relevant to the phenomena we wish to understand, partly due to concerns for cognitive economy. Incidentally, there is a widespread agreement that digestion does not play a vital part in the etiology of schizophrenia. Nevertheless, the idealization involved in excluding digestion from consideration results in a somewhat distorted representation, since suffering from schizophrenia actually do involve digestive processes.[11]

As another example, look at scaled models. An example of a scaled model could be a $30 \times 30 \times 12$ cm model of a 10x10x4 m wooden cabin. Scaled models involve important (and to some extent unapparent) types of distortion. The diminished wooden cabin is necessarily distorted since not all properties of wood changes proportionally when they are up- or downscaled. The weight of a cylindrical beam is proportional to its volume, which is again proportional to the cube of its radius. But the strength of the beam de- and increases proportionally to the square of its radius, why, obviously, the mass to strength ratio will not de- and increase proportionally even through perfect geometrical up- and downscaling (Van Fraassen 2008, 49f.). This example is banal by the standards of contemporary engineering. Nevertheless, it is an example in which it is impossible to create a non-distorted scale model. One cannot simultaneously retain the geometric proportions and the mass to strength-ratio. Up- and downscaling along various other dimensions will often involve more or less apparent distortions. One must choose between different distortions, and appropriate choices based on the characteristics of a given model can only be made if the adequate insights are available.

One might, at this point, allow oneself to draw inspiration from Hans Reichenbach's discussion of universal vs. differential forces (Reichenbach 1958, §3). Universal forces, according to Reichenbach, are, somewhat simply put, forces that affect all objects in the same way, whereas differential forces affect objects relative to their composition. Reichenbach mentions gravity as an example of a universal force and heat as an example of a differential force. We might instead of forces consider characteristics as displayed in representations, and say that some characteristics are invariant (universal is too strong for our concern) through (specific) representational transformations while others are differential, i.e. dependent on the specific transformed circumstances. That some salient surface characteristics, like geometric proportions, are invariant through a given representational transformation has the potential to divert attention from the fact that less apparent properties, like strength, are changed through the process.

When interdisciplinarity is considered as hybrid modeling, it becomes evident that interdisciplinarity is simply a subspecies of representational transformation. There are certain characteristics of representational transformation, some of which have been pointed out above, which also apply to interdisciplinarity. Distortion is certainly one.

Since all representations involve (more or less unapparent) distortions, one could provide innumerable further examples. For now, let the above suffice to show that for the non-expert it is sometimes far from evident which unapparent distortions are imported along

[11] In the very trivial sense that you have to be alive to suffer from schizophrenia, and you won't stay alive for long if you do not digest, one way or the other….

with perfectly proportional surface transformations. When we add to this that interdisciplinarity most often involves dealing with theoretical material (propositional structures) from outside one's central field of expertise, we may safely assume that the appropriate insights are not always available. This is equally problematic for the person who pushes her expertise onto a field different from her home discipline as for the person who has only limited understanding of this "imported" theory.

One might also imagine that there are grades of problems with transferability. It is probably much more challenging to transfer propositional structure from quantum mechanics to musicology than from, say, physics to biology or from biology to economics. Due to the larger difference in the initial approach, much more of the tacit knowledge of quantum mechanics is likely to be lost in the process resulting in the import of superficially understood theoretical material. This indicates the danger that the very prestigious cross-faculty collaborations are actually the ones most likely to show poor results. On the other hand, people are perhaps liable to be deceived by surface similarities, e.g. in mathematical forms, when importing elements from more familiar approaches. Patricia Kitcher has discussed related problems in a non-representational framework in her interesting work on Freud (Kitcher 1992; Kitcher 2007). But these very important issues do not seem to have caught the attention of scholars involved in discussions internal to *interdisciplinarity studies*.

There are, thus, very good reasons for increasing the focus on representation and the issue of distortion in connection with interdisciplinarity: In the combination of two or more approaches to modeling, all aspects of the involved representations are at play, including idealizations, propositional algorithms, and basic assumptions. And one cannot simply identify and replace distorted elements of one modeling approach with undistorted elements of another. Further, two combined distortions cannot be assumed to level each other out. Presumably some sort of interference will occur between the combined (distorted) elements when two or more modeling approaches are integrated. Whether this will result in more or less valid, robust, or relevantly purpose-serving representations must be evaluated through careful analysis on a case-by-case basis. A good place to start such case-by-case analyses would be by figuring out how construals and underlying propositional structures are changing as part of the transformation, and how this affects specificity, explicitness, and fidelity criteria.

The representation-based analysis of interdisciplinarity, therefore, identifies a number of pressing difficulties which ought to generate ample motivation for caution in the development, evaluation, and execution of interdisciplinary research projects.

9 Conclusion

As I have attempted to illustrate above, there are many complex unanswered questions regarding the epistemic value of interdisciplinary activities. A lot of these questions are not just unanswered but even entirely unaddressed in the central literature and debates on interdisciplinarity. This is regrettable, since reflection on the above-mentioned (as well as related) issues might serve to qualify the efforts considerably, while simultaneously strengthening the possibility for singling out hopeless or faux interdisciplinary projects.

In this paper, I have not been able to deliver exhaustive answers to all the questions raised above. Still, the points raised may at least serve to heighten the awareness of the treated issues and their challenging nature, thus acting as a starting point for the development of more specific recommendations.

One challenge to this approach is that representations in their published form obviously rarely (if ever) contain exhaustive accounts of all the overt as well as implicit and tacit assumptions, which are part of the particular representational practice. And not even the most rigorous analysis will be able to spell out all the propositional structures as well as conventional and skill-like practices involved in any given scientific approach. Therefore one might imagine cases where some significant epistemic changes have resulted from interactions and integrations between two or more approaches, even though there are no apparent changes at any discernible propositional or mathematical level. However, if confronted with such a case, one might fairly claim that the burden of proof lies with the person(s) claiming that there are substantial benefits gained through the process.

A further substantial issue, which I have not even begun to discuss, is the difficult question of how to evaluate the epistemic quality of the integrated approach as compared to its inputs. Improvements of scientific quality can be construed in many ways, but are ultimately dependent on the purposes the scientific approaches in question are intended to serve. Paradigmatic examples of improved scientific quality might be increased explanatory power, adding of detail or nuance, improved precision (e.g. in terms of prediction or distinction), increased scope, more general implications, increased conceptual coordination, improvements in terms of cognitive economy (a.k.a. simplicity), or an improved ability to intervene in processes and produces and to prevent or control specific phenomena.[12] These are all standard textbook suggestions for evaluating scientific quality. On the basis of the discussion above, one might add rising levels of specificity, explicitness, and fidelity as obvious candidates for evaluation.

Even though none of this is uncontroversial, and there is clearly a long way to go before matters of scientific evaluation are settled, it is worth emphasizing once again that explicit discussions of the ways in which interdisciplinary activities are supposed to result in scientific improvements are largely absent in existing treatments of the topic of interdisciplinarity. So just setting the stage for such a discussion constitutes a step forward.

Finally, various non-epistemic aspects of the activities involved in scientific practice may be considered good or bad by the involved scientists (or others), of course. For instance, it is valuable to be able to maintain a living, and it is very attractive and quite difficult to get a job in academia. Consequently one might expect that there is ample motivation for *opportunistic interdisciplinarity*, again a possibility that has received little attention—possibly because it presupposes a critical examination of whether interdisciplinary collaborations are always necessarily good.

Hopefully, this paper adds to the existing body of literature at least a small measure of insight relevant to the understanding and epistemic appraisal of interdisciplinary research projects.

[12] One might initially think that a way to go about this would be to engage in a Bayesian analysis of whether the integrated approach works better than the input-approaches. Unfortunately this would not work (at least not in all cases). Since the integrated approach may not be aimed at solving the original problems of any of its inputs there is no basis for direct comparison. One might even imagine cases in which the integrated approach may be reasonable even though it has a much lower probability of being true than the theories from which it was constructed.

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