Finding Truth in Fictions:
Identifying Non-Fictions in Imaginary Cracks

I critically examine some recent work on the philosophy of scientific fictions, focusing on the work of Winsberg. By considering two case studies in fracture mechanics, the strip yield model and the imaginary crack method, I argue that his reliance upon the social norms associated with an element of a model forces him to remain silent whenever those norms fail to clearly match the characteristic of fictions or non-fictions. In its place, I propose a normative epistemology of fictions which clarifies a model's ontological commitments when the community of scientists lack any clear, shared norms of use. Specifically, I will introduce a variational account of fictions as an extension of Laymon's monotonic improvability account of idealizations. I will conclude by connecting this variational account of fictions to didactic fictions in general.

1. Introduction

Philosophers of science have widely acknowledged that our fundamental theories cannot be directly applied to all of the systems within their domains. This failure of direct applicability can occur for many reasons – the insolubility of equations, unspecifiable boundary conditions, etc. – which can often be circumvented by suitable idealizations and approximations. Recently, philosophers of science including Winsberg (2003, 2006a, 2006b, 2009), Giere (2009, 2010), Suarez (2004, 2009) and Teller (2004, 2009), following Fine (1993) and Cartwright (1983), have examined another tool frequently used in such cases: the scientific fiction. Roughly, fictions are a type of 'truth conducive falsehood'. They are elements introduced into a model to overcome its intractability and yield predictions and descriptions of physical systems, but which themselves are not directly concerned with true or accurate representations.

In this paper, I critically examine Winsberg's thesis that fictions ought to be defined in terms of their social norms of appropriate use.[1] I will do so by considering case studies in fracture mechanics, which include elements that seem to be falsehoods but lack the accepted social norms needed to be classified as fictitious on Winsberg's account. After briefly expounding Winsberg's thesis with a simple example of the use of a fiction, the strip yield model, I will analyze an 'imaginary crack model' and argue that Winsberg is unable to provide any assessment of imaginary cracks as either fictions or nonfictions. I then will propose an alternative account of fictions that can clarify puzzling cases like this by avoiding any appeal to the state of the scientific community that is using the potential fictions. I conclude by justifying the continued use of the term fiction by connecting my new account to a broader notion of

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[1] This position is not unique to Winsberg. Giere (2009, 2010) and Teller (2004, 2009) have defended similar positions, and Suarez (2004, 2009 and 2010) and Bokulich (2008 and forthcoming) have defended similar views on the locus and ontogenesis of fictionality.

didactic fictions.

2. Social Fictions

One of the most immediate philosophical questions surrounding the use of scientific fictions asks how such falsehoods could possibly produce good, scientific knowledge of physical systems. In the case of numerical approximations, the answer is obvious: the models produced using them are 'close enough' for the intended use of the model; greater exactitude in these specific parameters is simply unnecessary, and the difference can be proven to be negligible. Ron Laymon (1987) has argued that normal idealizations[2] can work in roughly the same way, by being demonstrably 'true enough', differing only by degree from more accurate descriptions of the system of interest such that gradual improvements to the realism of the idealization can gradually improve the representational accuracy of the models of which they are constitutive. I will return to Laymon's characterisation of idealizations later, but for now what is relevant is that fictions cannot work in the same way – they are pointedly not truth-like. As Winsberg puts it, fictions, unlike idealizations and approximations, are not "concerned with truth or any of its cousins" (2009, 2). They are "contrary-to-fact principles that are included in a simulation model and whose inclusion is taken to increase the reliability of the results" (2006a, 1). As such, the nature of their falsehood is starkly different from that of idealizations and approximations. Though all three are "truth-conducive", at least insofar as they are able to contribute to models which provide accurate predictions and genuine scientific explanations,[3] the tools that justify the

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[2] Winsberg (personal communication) considers fictions to be a type of idealization. I consider this merely a matter of semantics, and so for simplicity, I will treat fictions, approximations, and (non-fictional) idealizations as three different elements of a theoretical model.

[3] I will not here defend the claim that fictions can provide genuine scientific explanations, first because it is mostly orthogonal to the present discussion, and also because it has been thoroughly and directly addressed elsewhere, e.g. (Bokulich 2008) and (Bokulich forthcoming).

use of idealizations and approximations will thus not necessarily be applicable to fictions, and we are faced with the question of how these false assumptions could possibly lead to true predictions.

Winsberg turns this problem on its head: rather than trying to see *a priori* how and in what ways falsehoods could be used to generate knowledge, he begins with the observation that there *are* clear cases where simulated systems involving such falsehoods have later been corroborated by empirical studies (2003). Indeed, the strip yield model is just such a case – it has been extensively and successfully tested in both the lab and the field. The question for Winsberg, then, is not whether fictions could possibly yield knowledge, but how they in practice do. Normative questions of whether and why to trust fictions are replaced by descriptive questions about those successful practices in which scientists actually engage, focusing on those which may help one to identify something as fictitious. Indeed, he concludes that it is exactly its use that defines some element of a model as a fiction: a fiction is a fiction just because it is appropriately used as a fiction. Specifically, on Winsberg's account, "to hold out a representation as a nonfiction is ipso facto to offer it for a particular purpose and to *promise* that for that purpose, the model 'will not let you down,' when it comes to offering guidance about the world for that purpose" (2009, 3). An element of a model is fictitious just if it is appropriate for the types of purposes a fiction is used for, where "what a representation is for depends not on the intention of the author, but on the community's norms of correct use" (2009, 5).

Winsberg then points to a particular purpose that all and only nonfictional representations have, that of "describing some part of the world – its representational target ... [I]t is meant to be a reliable guide to the way this part of the world ... is" (2009, 3). Fictions, of course, are often not completely divorced from reality, and in fact are usually offered as a representation of some facet of reality. A novel, for example, could be deeply telling of the human condition, and in some respects may be called a representation of it. But the way it is a representation is in a general sense, by showing how things in general go, while deliberately getting the details wrong. Charles Dickens' *Great Expectations* may accurately represent the struggles of life, love, and loss, but no one would take it to be an accurate depiction of the life of an orphan named Pip. A fiction is representational insofar as we can take it to be telling us something about the world, but "it is not a guide to the way its prima facie representational target is" (2009, 3). Nonfictions, on the other hand, including nonfictional falsehoods like idealizations and approximations, "describe *and point to a certain part of the world* and say 'if you want to know *about that part of the world I am pointing to*, for a certain sort of purpose, I promise to help you in that respect and not let you down'" (2009, 4).

3. Fictions in the Strip Yield Model

To see how this works, consider the "strip yield model" that is used in fracture mechanics, the subfield of mechanical engineering that studies how minute flaws, imperfections and cracks in a structure can erupt into catastrophic fractures. The strip yield model is an attempt[4] to resolve an inconsistency in linear elastic fracture mechanics(LEFM). By modelling a structure as though it were ideally elastic (i.e. as though any stretching or bending would be undone were the stretching or bending force removed), LEFM can fairly easily and accurately model how stress can be focused near the tip of a crack. In the immediate vicinity of the crack

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[4] It is perhaps worth noting that, with the rise of computing, the strip yield model has largely been replaced with other models that are more able to be more accurate by incorporating computationally expensive inelastic mechanisms, replacing LEFM with "elastic-plastic mechanics".

tip, however, LEFM predicts an infinite stress singularity – a physical impossibility, since it would require an infinite amount of energy and would predict that the most minute push could rupture even the strongest structure.

Without going into the technical details,[5] the strip yield model corrects for this by positing that cracks are extended beyond the tip, through what is called the "plastic region" - a region that is known to behave inelastically, and thus to be beyond the purview of LEFM. This crack extension is then imagined to be undergoing a strong compression stress, made to be exactly strong enough to cancel out the exponentially increasing stress of the singularity. The equations of LEFM can then be solved to determine how long the crack extension (or, more physically, how large the plastic zone) must be to prevent a stress singularity. Combining the real and imaginary stresses provides a much more accurate expression describing how the plastic region diffuses stress around the tip of a crack, while holding the stress within the plastic region constant to prevent a stress singularity. The final step of the strip yield model modifies our picture of crack extension. Since stress plateaus within the plastic region, the actual mechanism of crack extension – stresses increasing until the atomic bonds holding together a crack tip break – will never be active. However, the size of the crack extension is proportional to the square of the distantly applied (real) stress, so the effective crack length will increase as more stresses are applied. This picture of crack extension is fairly realistic, though, since it falls out of the equations of LEFM, it is only an elastic crack extension – the crack is stretching rather than opening - and thus the extension would unrealistically be undone if a stress is removed. In spite of these caveats, the strip yield model is able to provide a powerful, computationally cheap

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[5] For a good introduction to fracture mechanics, including an in depth discussion of the strip yield model, see (Anderson 1994).

account of how stresses focus on crack tips, and how cracks grow with added stresses.

On Winsberg's account, the strip yield model clearly uses fictions. The crack extension and additional stress are truth conducive, insofar as they produce a model with a curtailed stress singularity at the crack tip and spread the excess stress throughout the plastic zone. But they are clearly not realistic descriptions of their “prima facie representational targets” – no practitioner thinks that the crack actually goes through the plastic zone or that there is a stress that acts only along that crack extension, but not on the rest of the material. Moreover, the fictional and nonfictional elements of the strip yield model are combined in a patently unrealistic, indeed impossible, way. The model effectively places two cracks on top of each other, subjects them to different, non-interacting stresses, and then combines the results as though the stresses were from two perfectly normal loads applied to the same crack. Clearly, then, the crack extension and the compression stress fail to come with a promise to be reliable guides to their representational targets. Furthermore, the success of the strip yield model can be explained in a typically fictitious way: the fictional elements replace a real mechanism in the system, inelastic (plastic) deformation, so that something else may be accurately modeled, the behavior of the crack under a distant load. As Winsberg puts it, “[w]e are deliberately getting things wrong locally so that we get things right globally” (2009, 8). The fictions are introduced in one part of the model without the referential promise so that that promise can be made with respect to some other element. Finally, this paradigmatically fictional use of the strip yield model is readily acknowledged by the shared norms of use of the community of researchers. For instance, one discussion in the literature describes the use of this model as follows: “Analysis is conducted by assuming the crack accompanying plastic zones as a fictitious crack and formulating integral equations based upon traction free and no stress singularity conditions," (Nishimura 2002, 1). This quote is far from peculiar; another group of researchers describes a similar model as follows: "The model consists of three regions: (1) a linear-elastic region containing an imaginary crack of half-length $a + \omega$, (2) a plastic region of length x ahead of the physical crack length a, and (3) a residual plastic deformation region along the crack surface," (Kim and Lee 2000, 74). These two passages are representative of a broad acceptance by the community of researchers that the crack extensions and compression stress are entirely fictional elements of the model that replace more difficult to model inelastic mechanisms that in fact exist around a crack tip, falsehoods which allow the rest of the model to be more accurate. The norms of use associated with the strip yield model thus allow inferences about the spread of stresses around a crack tip, but do not allow inferences about the actual structure of that tip or of the nature of the growth of the crack as a whole. By focusing on these social norms of use, Winsberg's account is able to identify the fictions in the strip yield model and direct us to an understanding of which uses of the model are deemed justified by that community.

4. Real and Imaginary Cracks

Winsberg's success with the strip yield model is not easily carried over into all of the simulation methods of fracture mechanics, however. I will now consider a case study, the 'imaginary crack method', that defies Winsberg's analysis.[6] This method is used when materials are known to contain flaws for which the nature, location, number, and size is unknown. One imagines that the structure has cracks, either distributed throughout or at key points. This simulation method has been carried out in many different contexts and ways; for simplicity, I

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[6] Taylor (2008) introduces this term to summarize the work of many other authors.

will focus on the work of Fjeldstad et al. (2008). Their method begins with the observation that manufacturing processes inevitably introduce flaws of various types and magnitudes throughout a material. These flaws stem from chaotic elements in the manufacturing process, leading to "variability of chemical composition, microstructure and mechanical properties" (Fjeldstad et al. 2008, 1186). Practitioners have long known that these flaws, regardless of their type, can be treated as though they are microcracks, so long as they are small relative to the structure and one is interested only in modelling the structure as a whole (Waddoups et al. 1971). The details of the flaws, then, are not nearly as important as knowing the tendency of the manufacturing process to produce flaws of any type. Moreover, since the flaws are introduced through chaotic processes, their effects can be modeled only through a statistical analysis.

Fjeldstad et al. begin by modelling the structure using standard finite element analysis (FEA): the structure is divided into elements, usually line segments connected by nodes or surfaces connected at boundaries. FEA allows researchers to model a continuous system by modelling just the stresses, deformations and other relevant quantities at the nodes and element boundaries. Analogous to converting differential equations to difference equations, FEA makes the model fairly easily calculable by ordinary numerical methods. The researchers then randomly distribute crack-like defects of various types throughout the model and calculate the stresses near each crack tip to determine which cracks will grow and by how much. This process is repeated as a Monte Carlo simulation (i.e. a statistical method where the simulation as a whole is repeated, randomizing parameters, to produce a broad statistical sample, from which inferences are drawn), allowing researchers to determine which cracks or combinations of cracks would cause the structure to fail. Combining these results with information about the stresses a structure is likely to bear can reveal the probable life of the structure in different conditions.

What makes this simulation process interesting for us is that cracks are randomly distributed because they are taking the place of unknown defects of various types, and thus are *prima facie* candidates to be analyzed as fictions. At one level, the cracks, which Taylor and Kasiri (2008) call 'imaginary cracks', clearly seem to be fictions in the same sense as the crack extension and compression stress in the strip yield model: they are often known not to exist, but are introduced to take the place of other types of flaws, which are governed by more difficult to model mechanisms. If we look at imaginary cracks more closely, however, their fictitiousness becomes less apparent. The result, as we will see, is that imaginary cracks cannot clearly be classified as either fictions or as nonfictions on Winsberg's account.

Imaginary cracks are not introduced with a clear promise to be a reliable guide to their prima facie representational target, actual cracks with those sizes and locations, but they do not have all of the social norms of use normally associated with fictions. According to Winsberg, as we have seen, fictions are introduced to replace peripheral mechanisms with simpler ones so that the focus of the simulation may be more easily modeled. In this case, however, what are of interest are those cracks that would erupt in a significant enough fracture to cause the structure to fail, suggesting that they are nonfictions. Fjeldstad et al. refer frequently to the real defects that they are attempting to model, and frequently note that the purpose is to identify the "[f]ailure of a component[, which] occurs when the crack has reached a predefined size, or if the stress intensity factor K has reached the fracture toughness K_{Ic}" (2008, 1186). These researchers are not inserting imaginary cracks so that they can study some other phenomena; they are inserting imaginary cracks so that *just those cracks* can be studied to determine when they will reach a critical point and cause fracture. On the other hand, the clearly nonfictional elements – structure shape, material composition, applied loads – are the peripherals that are used to model the behavior of imaginary cracks.

In light of the failure of a quick analysis, following Winsberg one may look for a consensus among engineers to establish the *social* norms of use in which fictitiousness ultimately inheres for him, since, for Winsberg, this must be the ultimate determinant of whether a model is a fiction. We shall see, however, that, while all would agree that there are some false elements, "[s]ome workers have identified this crack with a real, physical crack or zone of damage which occurs in some cases prior to final failure" (Taylor and Kasiri 2008, 1076).[7] These researchers are observing a long standing controversy concerning the appropriate ontological commitments one ought to have to an imaginary crack model.

In one sense the ontological commitments of an imaginary crack are obvious – it describes the world as though there were a crack at a location with a size and shape, what Winsberg terms a "prima facie representational target". The controversy is not over how the world is depicted by these models; rather, it is over the appropriate attitude to have to this depiction. The decades long debate is about what are the appropriate inferences that one may make from some facts about an imaginary crack (or a distribution of imaginary cracks, or a distribution of random distributions of imaginary cracks – recall that it is a Monte Carlo simulation) to facts about the actual structure being modeled. All engineers grant that something is false about this depiction; the question at hand is what, exactly, is false, and how these false elements ought to be interpreted. Taylor, Kasiri and others interpret imaginary crack methods as

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[7] They are referring to a different use of the same type of method as used by Fjeldstad et al., but the point remains. Taylor (2007) makes a similar point that encompasses all of the case studies considered here.

only giving us general information about the behavior of a structure under different conditions. Thus, everything about the individual cracks is fictional in just the way that Winsberg describes – the local is deliberately wrong so that the global may be right. Others have argued that, though many of the details are false, we can nonetheless interpret some imaginary cracks as stand-ins for real flaws. On this interpretation, then, we have warrant to interpret at least some imaginary cracks as direct stand-ins for specific actual flaws, and thus can reveal far more local information than Taylor, Kasiri, and their ilk would grant. This is a question about what information the resulting model is actually giving us and which elements are mere artifacts of the process, with no proper physical interpretation. As examples of the position that imaginary cracks need a more physical interpretation, consider the work of Ostash and Panasyuk, Lazzarin et al., and Usami et al. All three, as we will see, maintain that these cracks are nonfictions, but face difficulty when they attempt to spell out how they are nonfictions.

Ostash and Panasyuk begin by assuming that, “due to the cyclic deformation of a solid (an element) with a geometrical stress concentrator, a specific process (prefracture) zone is always formed in the notch root vicinity,” (2001, 627). In other words, they assume that, prior to full fracture, fatigue always causes a specific type of deformation to appear which is effectively identical to a new 'introduced crack', conceptually identical to Taylor and Kasiri's imaginary cracks (Taylor 2007, 39-40). Ostash and Panasyuk admit, however, that, “[i]t is difficult today to make a precise definition of the fatigue process zone. This zone is conceived of as some volume of material in which the micro- or macroplastic cyclic strain takes place, and the initial damage (multiple defects) of the material microstructure originates,” (2001, 627). They are introducing the concept of a fatigue process zone without physical interpretation, while assuming that there must be some interpretation or other that could make sense of it. The open question for them, then, is one of determining exactly how to offer the physical interpretation that they think is necessary for their models to be as reliable as they can be demonstrated to be.

Similarly, Lazzarin et al. develop a method of analysis based on what they call the 'intrinsic crack length', a property of an 'intrinsic crack'. However, in justifying their acceptance of the reality of this intrinsic crack (Taylor and Kasiri's imaginary crack), they admit that "attempts to provide a physical reasoning for the validity of the [intrinsic crack] ... have so far remained unconvincing. Nevertheless, several authors regard the 'intrinsic crack' as a real threshold" for crack propagation (1997, 647). To a certain extent, then, Lazzarin et al. are identifying a black box and filling it with the intrinsic (imaginary) crack, while admitting that their assumptions face serious physical shortcomings.

Finally, consider the work of Usami et al. who argue that fractures emanate from small, semicircular flaws in the material (1986). Similarly to Ostash and Panasyuk, these engineers assumed that small cracks would form from the grain of a material prior to fracture. Though Usami et al. go further than any of the the previous researchers towards justifying their assumption on physical grounds, drawing on empirical research investigating the different ways that flaws can be introduced into a material, they are still forced to admit that the model they are constructing "may have a problem in physical meaning," (1986, 745). For them, the problem arises later in their analysis, when observations force them to admit physically impossible values describing the strength of a material. Nevertheless, they maintain that some physical explanation *should* exist that would eliminate these anomalies.

Taylor and Kasiri, among several other influential founders of the various imaginary crack methods, most prominently Waddoups et al. (1971) and El Haddad et al. (1979), have argued at length that these practitioners are simply mistaken, that imaginary cracks (or prefracture zones or intrinsic cracks or grain expansions) do not require physical interpretation, and thus that any inference from these models to their prima facie representation targets must be deeply flawed. These examples should suffice to show, however, that a significant number of influential researchers are nonetheless operating under the assumption that what Taylor and Kasiri call imaginary cracks correspond to real cracks or flaws that exist in a material before it fractures. The dispute over imaginary cracks is not mere idle speculation – different interpretations will not only offer different pictures of the fine structure of the object, but also different recommendations for how to reinforce and strengthen it.

Though imaginary crack methods of one type or another have been around since the early seventies, controversy surrounding the appropriate ontological commitments that accompany their use has persisted to the present day. Given that this dispute concerns the proper interpretation of elements which are known to be false, we might expect a philosophical account of fictions to enlighten the scientific dispute. However, given the fact of the dispute, it nonetheless seems safe to say that imaginary crack methods simply do not have have established social norms of use in the sense Winsberg requires for a model or an element of a model to be either a fiction or a nonfiction.

In part, the confusion about the proper inferential attitude to have towards imaginary cracks stems from the fact that they seem to imply important facts about a system in a way that fictions usually don't. Imaginary cracks point to actual weaknesses in a material that are not otherwise modeled to identify the causes of crack formation. Fjeldstad et al. frequently refer to manufacturing defects, and observe that “[f]atigue cracks are prone to initiate and grow from such defects”, and that the lack of “repeatability in the manufacturing conditions leads to variability of” these defects, which can thus only be modeled statistically (2008, 1186). Additionally, the direct result of modelling imaginary cracks provides evidence of certain causal facts about the material that are not directly simulated, namely the weaknesses that will bring about catastrophic structure failure.

If we combine these two points, the social norms of use associated with imaginary cracks would seem to point in two different directions. They behave as nonfictions insofar as they are the focus of the study, and not peripheral elements that are clearly replacing some other mechanism. Moreover, they are often taken to yield clear knowledge about elements that are not represented by other elements of a model, the weak points and expected life of the structure, inferences which depend on facts about specific, individual imaginary cracks. But they also exhibit paradigm traits associated with fictions. They are nonexistent, and are normally introduced with the full knowledge of their nonexistence, and they are randomly distributed and are used in place of flaws of other types, a seemingly clear case of replacing one mechanism with another. As a result, the published record of the community of researchers clearly shows that there are no social norms, that is, there are no agreed upon standards about how imaginary cracks are to be used. Because of this, Winsberg's account of fictions must remain silent about the ontological commitments of imaginary cracks – they are, for Winsberg, neither fictions nor nonfictions, there simply is no fact of the matter about their fictitiousness.

The case of imaginary cracks may thus pose a challenge to Winsberg's attempt to “demarcat[e] the boundary between fictions and nonfictions” (2009, 4). Winsberg can easily deal with those models whose components are clearly fictions or nonfictions, so long as the community of researchers agree with this assessment, and to the extent that it can clarify cases like the strip yield model, then, I think it is successful. However, if the social norms of use are more mixed, as in the case of imaginary cracks, it cannot judge one way or another. By defining a fiction in terms of its social norms of use, Winsberg is forced to admit that there *simply is no fact of the matter* about whether something is a fiction when it has no clear social norms of use. This is a coherent position to take, since 'scientific fiction' is something of a neologism, and thus the exigencies of our need provides some latitude with respect to its meaning. Alternatively, one may maintain that scientific practice here has not yet evolved to the point of being either fictional or nonfictional – imaginary cracks may be immature fictions (or immature nonfictions), that will, with continued debate, develop one coherent set of norms or another.[8]

However, if one is motivated by a normative ideal for philosophy of science, in which philosophy of science ought to be an aid to these conceptual, interpretational debates, then one may find it problematic that a theory of scientific fictions applies only in those cases where scientists don't need a philosophical account of fictions to practice good science. In other words, one may find it unsatisfactory that the failure of a demarcation criterion to apply to a case squarely in its purview must be to say that there could be no possible demarcation. While it is true that the social norms associated with imaginary cracks may eventually coalesce as fictions or nonfictions as Winsberg describes them, it is not obvious that there can be no role for the philosopher in helping it do so. Rather than throw in this towel, I propose an alternative that does not rely on the opinions of the community of researchers, but instead looks at the intrinsic properties of the simulation. Such an account is designed to address cases such as this, and could

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[8] I thank an anonymous reviewer for raising this interesting possibility.

moreover guide future research by contributing to a normative epistemology by revealing the appropriate ontological commitments of models that have no clearly defined social norms of use in Winsberg's sense.

5. Ideal Confirmation

It is beyond the scope of this paper to delve into the general type of solution that I think can be developed, so I will here just sketch it in outline and then show how it could be applied to the case of imaginary cracks. My proposal is that we turn on its head an argument by Winsberg (2006b), in which he contends that the oddity of a truth-conducive falsehood other than idealization and approximation is sufficient to invalidate earlier research on scientific models. Specifically, he addresses Laymon's 'monotonic improvability' account of the confirmation of theories which can only be applied through the use of idealizations (Laymon 1987, 1989).

In response to Cartwright's (1983, ch. 6) thesis that theories are shielded from disconfirmation by being conjoined with idealizations, Laymon suggests that we consider how predictions change when a theory is conjoined with increasingly realistic idealizations. He argues that "[a] theory is confirmed if it can be shown that better [i.e. more realistic] approximations [or idealizations] lead to better predictions. A theory is disconfirmed if it can be shown that better approximations do not lead to better predictions," (1987, 211). He calls this property of yielding better predictions with better input 'monotonic improvability'. A well confirmed theory, then, is one that has made more accurate predictions as the inputs (models including idealizations, approximations and fictions) are made more realistic, and a theory is disconfirmed to the degree that improved inputs do not lead to more accurate predictions.

Winsberg responds to this view by arguing that fictions problematize Laymon's account of how idealizations can be legitimately used in theory confirmation. Fictions represent a class of objects that are superficially similar to idealizations and approximations, but which defy Laymon's analysis. First of all, fictions are pointedly designed to be less realistic than alternative inputs to the model; however they are used because they yield better predictions than a model that neglects them. We can thus imagine a short series of inputs that represents a move from less to more realistic inputs that begins with a fiction and then replaces the fiction with a more realistic description of the system. Assuming that the fiction were successful, however, it then would be used just because it is better at providing predictions. The improvement of inputs represented by replacing a fiction with an accurate description would not yield better predictions, and so the theory would be disconfirmed. The use of fictions is not taken to speak against the truth of a theory, however, and in fact has sometimes been taken to be confirmatory. Winsberg concludes that this speaks strongly against any account of theory confirmation like Laymon's.

I find Winsberg's argument problematic for several reasons, but I think that it can reveal some useful and interesting characteristics that can help identify fictions. Winsberg can successfully argue that fictions do not fit neatly within a broader account of idealizations such as what Laymon provides. This failure is not so straightforward a refutation of Laymon's account as Winsberg supposes, however. While some thinkers may be inclined to label all non-veridical elements of a model (including straightforward idealizations) as fictions (e.g. Teller 2009 238, Suarez 2009 158), neither Winsberg nor I do so. Thus, the fact that these fictions clearly defy Laymon's account could be taken merely to say that Laymon has a theory of how (nonfictional, truth-like) idealizations can contribute to theory confirmation. All that follows, then, is the unsurprising conclusion that fictions, idealizations and approximations must be treated as epistemically different in important ways. Laymon's account may suffice when we are concerned with only straightforward idealizations, but clearly some other account is needed to be able to understand theory confirmation in a context of fictions.

I would like to conclude by using these observations about Winsberg's rejection of Laymon's monotonic improvability thesis to propose an alternative defining characteristic of scientific fictions: fictions are those elements that, when introduced into a theoretical model, fail to fit into a monotonic series of improvements, and yet are still truth conducive. In other words, fictions are added to or incorporated into models to aid in producing accurate predictions, but are themselves more unrealistic than some tractable alternative which produces worse predictions. I am here identifying fictions by an internal, variational property, that of failing to be a part of a monotonic series of improvements, rather than the external property of being considered by a community to be properly used as a fiction.

Like Laymon, I am thinking of the range of possible inputs to a theory or model as being partially ordered from the less realistic to the more. Many of these inputs would, of course, be entirely intractable; I will for the moment ignore this difficulty, in part because I think that it can only be interestingly addressed in a case-specific way. Additionally, I will for simplicity be considering these inputs and posits as though they vary continuously; all that is in fact needed is that there is a meaningful way to say that one posit is more, less, or equally realistic compared to another. We can visualize how this would go by holding all elements of a model constant and varying just one from the more realistic to the less (imagining for ease of representation that such a ranking is more than partially ordered), and placing that variation on one axis on a graph, and compare the accuracy of the models that result. A fiction, on this picture, would be any 'hill' that has a valley between it and more accurate inputs (diagram 4), where higher points represent more accurate predictions. Laymon's account describes the last hill, which is more or less purely monotonic, and thus the inputs are all idealizations which together confirm the theory.

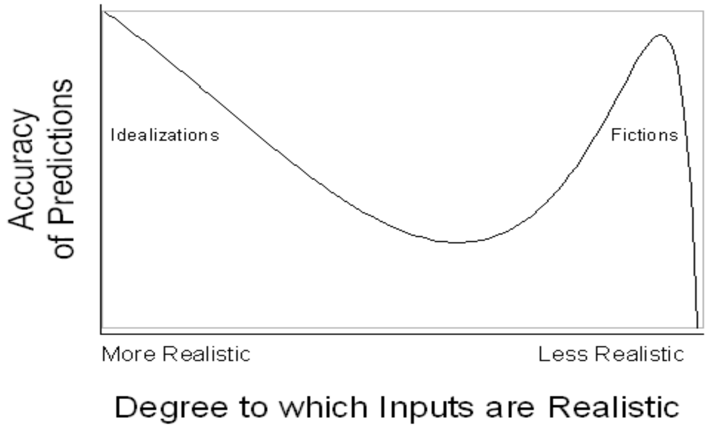

Diagram 1: Fictions are unrealistic inputs, for which a more realistic alternative provides a less accurate prediction.

The failure to be part of a monotonically improving series is not sufficient, however, to constitute a normative account of fictions, since it is a success condition: those elements that fail to be monotonically improvable, *and yet are still truth conducive*. But for many potential or purported fictions, we are using them to construct simulations exactly because we cannot perform experiments to investigate the phenomena of interest. It thus fails to be obviously normative except in the special cases in which the variational success has already been demonstrated, cases which I suspect we would frequently already have intuitively thought the elements were fictions or nonfictions.

6. Imagining Reality

What is needed is to draw out explicitly the epistemology of this success, to answer the normative question: what is it that makes these non-monotonically-improvable falsehoods truth-conducive? Answering this question constitutes the other horn of my proposed account of fictions, though it is an open ended project, since I suspect that there is not, and need not be, a single unique answer. For now, I will limit myself to an epistemology of imaginary cracks in an attempt to reveal the ontological commitments of their use.

To this end, we can take as our cue the critical analysis above that allowed us to conclude that Winsberg's account could not provide a clear answer whether imaginary cracks are fictions, and look at what facilitates the successful predictions. The simulation essentially tries to model the behavior of many possible flaws in the material, which are represented as tiny cracks. These individual cracks, as has already been noted, are introduced as fictitious substitutes for flaws of various types. Part of what complicated that analysis was the fact that what the cracks are replacing are themselves usually nonexistent entities, since there was no reason to think that there are flaws at exactly the locations of the randomly distributed cracks. The question, then, reduces to this: why does a random distribution of cracks suffice to model a particular distribution of flaws, and what is the random distribution actually saying about the world?

To begin with, it is necessary to look more closely at how the cracks are distributed through the material. The authors note that, though they are randomly distributed, it is not without constraints; rather, the random distribution is mapped to a particular probability distribution across the structure based on what is known about the manufacturing process and the existing wear on the structure (Fjeldstad et al 2008, 1187). Looked at in this way, then, the *distribution* of imaginary cracks (but not necessarily the cracks themselves) is a straightforward idealization – should the probability mapping be replaced with one that more realistically represents the actual distribution of flaws that results from chaotic production processes, then the resulting representational model would always be more accurate, since it would have to rely less on the emergent distribution's statistical representativeness.

In light of this, we can examine the fictionality of the individual cracks, which still do not correspond in number, location, size or type to any particular flaws. The source of the method's success is the fact that stresses tend to vary continuously, so if we place an imaginary crack at a particular location in some finite element, it will likely behave in a very similar way to a flaw anywhere else in that element. Thus, if one crack extends to fracture under normal loading, it would still fracture if it were in another region of the same finite element. By repeating the process (recall it is a Monte Carlo simulation), researchers can accurately identify regions within which the existence of a flaw would lead to fracture. Analysis of this distribution in combination with information about how the material and structure can be expected to fatigue can yield the probability of a material flaw existing or developing within the region that will erupt in fracture. We can then conclude that the distribution that results from the Monte Carlo simulation refers to regions susceptible to failure, which can yield probabilities of failure under certain circumstances.

Because of this fact, we can understand the individual imaginary cracks, *qua* elements of a (random, idealized) distribution as fictions replacing the flaws that are elements of a (chaotic, real) distribution. The cracks do not fit into a monotonically improvable series of inputs in two ways. First, a change that makes some of the crack more like a normal material flaw will result in a substantial decrease in computational tractability (and thus a lack of predictions, accurate or otherwise). More importantly, the causal properties that are revealed by this simulation (weakness in the structure and the behavior of stresses) are *only* revealed by the statistical Monte

Carlo simulation. A particular crack in a particular distribution does not have any proper representational content except insofar as it contributes to the interactions between different sources of stress and can potentially rupture in full fracture. And finally, while it is well known that cracks behave very similarly to other kinds of microflaws, if we were to make a crack more similar to a different kind of flaw such that it has some of the properties of both, it does not necessarily follow that the resulting mix will behave in a way that is any more similar to the actual flaws that will exist in the material.

Finally, though the individual cracks are not monotonicaly improvable, and are thus fictions, when we analyze together those imaginary cracks which turn into full fractures, we can conclude that the *fractures* they turn into are nonfictions (albeit probabilistic nonfictions). This is because they will roughly correspond in number, type, and size to actual fractures that could in fact erupt from the various microflaws that are in the structure under various conditions. And most interestingly, these fractures are nonfictions even though they are numerically identical to fictional imaginary cracks which, still, ought not to be taken to refer to anything in particular in the world.

By shifting from talking about entities or elements of a model as being fictional or nonfictional to talking about individually variable posits (or jointly variable groups of posits) as fictional or nonfictional, this variational account is easily able to pick out the level of description that we *ought* to use to pick the fictions from the nonfictions. It is normative in the sense that it can make an assessment of the ficitionality of a posit that is entirely independent of the assessments that the community of researchers have decided are appropriate. This is contrasted with Winsberg's account which can at best find the level(s) of description that scientists *in fact* *do* use to decide which inferences are permitted and which are not. This is not to say that these two accounts need be completely at odds – I would expect that most of the times when there is a scientific consensus, the best way to learn about how a falsehood is truth conducive is to look first to that consensus, and Winsberg gives us the tools to do that. What I find lacking in his account is a normative element: way of criticizing or helping to construct that consensus.

8. Conclusion

The account of fictions developed by Winsberg can adequately describe the epistemology of most truth conducive falsehoods. I have argued, however, that it falls short in exactly those circumstances where we most need an account of fictions: those which lack any distinct norms of use, and which thus do not have a clear ontological commitment. I would like to conclude by specifying exactly what type of shortcoming I take this to be by placing the present work within a broader understanding of didactic fictions. I briefly argued earlier that the concept of a scientific fiction is something of a neologism, and thus can mean whatever we find it useful for it to mean. This is of course, not entirely without constraint. We are using the word 'fiction' because scientific fictions are taken to be a real type of fictions. The sense in which I take it to be a neologism is in being a new type of fiction, roughly analogous to how the concept of historical fiction was developed and kept separate from poorly done history.

Specifically, I contend that my account is compatible with a general account of didactic fictions.[9] Didactic fictions get across some moral through the use of their falsehoods, they are stories with many false details which together are able to convey a general truth. However, for the moral to actually be applicable, it seems plausible that it must be the case that a similar story could be told that doesn't use fictional elements but describes real (or at least modal) elements in

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[9] I thank *redacted* for comments which helped development this point.

their place. Furthermore, the truer story would more directly conclude with what the general moral would dictate in a particular situation, but if the fictitious story were to be gradually made more realistic, it would for a while completely fail to give the moral at all. Didactic fictions, then, are non-monotonic with respect to their ability to convey their morals. This fact seems central what it is to be a didactic fiction.

One may object, of course, that there could be exceptions to the general rule I have laid out. Though I do not myself know of any exceptions, I would like to finish by speaking to what they could imply. It is important to keep in mind that the preceding analysis is analogical: scientific fictions are like didactic fictions in literature insofar as they perform a similar function in a similar way. But they are not identical. For one thing, it is easily conceivable that a theoretician may propose a model believing that one part of it is fictitious, but that that part may eventually be found to describe a real mechanism. In some respects, this is what happened historically with the development of imaginary cracks: the original researchers thought that they were more fictitious than later researchers like Taylor and Kasiri do. Such a possibility would be rather strange, however, in a medium like literature. Our credulity would be more than a little stretched by an anecdote of a novelist discovering that her novels describe real people and events. The upshot is that scientific fictions are at best only analogous to traditional fictions in literature, and thus the analogy should only go as far as it is useful for us to stretch it.[10] Should one find an example, then, of an element of a model that my account would treat as an idealization, but which seems fictitious, this does not necessarily refute my account. It may merely point out somewhere where the analogy fails, and so one must ask whether treating it as a

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[10] This dissimilarity may only be present for strongly fictional literature. Perhaps a closer connection would be a piece of historical fiction that filled in gaps between well documented events – it would be much less surprising should a part of one of those gaps be found to resemble reality.

fiction rather than an idealization is actually useful.

One may additionally argue that, since I am merely drawing an analogy with didactic fiction, and since the word “fiction” has such strong connotations, it is in fact unjustified. I contend, however, that didactic fiction, historical fiction, and literary fiction also bear only an analogy to each other, and yet this analogy is sufficient to establish the family resemblance that underlies their shared use of the term. Scientific fictions are similarly analogous to these genres (some more than others, of course), and, especially since they so closely mirror the defining attributes of didactic fictions, their inclusion into the family of fictitious representations seems natural and unproblematic. Moreover, as some of the quoted passages above illustrate, many scientists who themselves label some elements of their models as fictions find such a label unproblematic, and so the negative connotations are clearly surmountable.

What I have taken myself to have done here, then, is to have constructed an account of a type of scientific practice that bears a strong analogy to didactic fiction and a family resemblance to other fictions, and to have shown that distinguishing scientific fictions is a fruitful enterprise. Since this is only an analogy, my argument against Winsberg was not framed by pointing out somewhere where he missed the mark by designating something as a fiction or a nonfiction. Rather, I have argued that he fails to include in his categorization a group of phenomena that may be fruitfully analyzed as fictions, which prereflectively seem similar to fictions. My criticism, then, is not that Winsberg's account of fictions is wrong, *per se*, so much as that it faces some serious pragmatic challenges which are overcome by my competing account. It remains an open empirical question, of course, whether my concept of a fiction is able to do everything that we may want an account of fictions to do. I have succeeded, I hope, in showing that, in at least some situations, my account can be preferable to Winsberg's, and thus at least deserves a place alongside it in further work on the philosophy of fictions.

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