

In Defense of Analogical Reasoning

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Abstract: I offer a defense of analogical accounts of scientific models by meeting certain logical objections to the legitimacy of analogical reasoning. I examine an argument by Joseph Agassi that purports to show that all putative cases of analogical inference succumb to the following dilemma: either (1) the reasoning remains hopelessly vague and thus establishes no conclusion, or (2) can be analyzed into a logically preferable non-analogical form. In rebuttal, I offer a class of scientific models for which (a) there is no satisfactory non-analogical analysis, and (b) we can gain sufficient clarity for the legitimacy of the inference to be assessed. This result constitutes an existence proof for a class of analogical models that escape Agassi's dilemma.

Resumé: Je réponds à certaines objections contre la légitimité des raisonnements analogiques afin défendre une description analogique des modèles scientifiques. J'examine un argument de Joseph Agassi dans lequel il laisse entendre que toutes les prétendues inférences analogiques succumbent aux dilemmes suivants : un raisonnement analogique est soit (1) désespérément vague, et ainsi n'établit aucune conclusion, ou soit (2) préférentiellement analysable comme s'il était un raisonnement non analogique. Ma réfutation consiste d'une classe de modèles scientifiques pour lesquels (a) il n'y a aucune analyse non analogique satisfaisante, et (b) on peut obtenir suffisamment de clarté pour légitimer l'inférence analogique. Ce résultat prouve qu'il y a une classe de modèles analogiques qui échappent au dilemme d'Agassi.

Keywords: analogy, animal studies, scientific models, skepticism about analogical reasoning.

1. Introduction

The importance of models in scientific methodology is commonly accepted, but there is considerable debate concerning how the modeling process should be understood. This debate centers on the relation between the model and the world. Two distinct approaches to this question have been developed. On the *representational account of models*, the relation between components of the model and components of the world is best explained in terms of reference or denotation, much like components of a language can refer to or denote things in the world. The model does more than simply

establish a referential link to the world; it must also have an internal system of transformation rules that allow us to derive results that can then be applied to the real world system. The representational view is especially adept at accounting for the relation between abstract/mathematical models and the world. For example, the relation between Galileo's geometrical models and the motions of objects at the earth's surface is naturally seen as one of denotation, where the vertical axis of a geometric diagram can be used to refer to or denote time intervals of some real world kinematic phenomenon.

On the *analogical account of models*, the relation between model and world is explained by appealing to similarities or resemblances between the two. The type of ampliative inference at work here matches the traditional format for analogical inference: features of the model are held to resemble or be similar to features of real world phenomena, and on account of this base similarity relation, we are able to establish further results concerning the target on the basis of information we glean from study of the analogue. Proponents of the analogical account contend that it does a better job of characterizing many models used in science, such as scale models (wind tunnels, crash dummies, wave tanks, etc.), animal studies, cell and tissue studies, and computer modeling.

For many models in science, the representational view offers the only plausible construal (the equations found in mathematical models do not resemble any natural phenomena). However, the analogical approach is arguably the more plausible when it comes to accounting for what goes on when a wave tank is built or an animal study is conducted. While advocates of the analogical approach, such as Mary Hesse (1966), Rom Harré (1970, 1986), and Dedre Gentner (1980, 1983), do not deny the viability of the representational approach, they see it as adequate for a very circumscribed set of models, typically found only in theoretical physics and mechanics. If we look to other branches of science, such as biology or applied physics, we find myriad examples of models more amenable to similarity-based or analogical analysis.

Proponents of the representational account contend that, while analogical models may prove useful heuristically, they provide no real epistemic justification for the hypotheses they are meant to support. To justify their rejection of the analogical account of models, representationalists have recourse to some long established logical objections to the epistemic legitimacy of analogical reasoning. Those who hold that the analogical account gives the correct picture of a certain class of models, and who also believe that such models can form the basis for epistemically legitimate inferences, are required to make some response to these logical objections. In this paper, I will first review an argument that

purports to undermine the epistemic legitimacy of analogical inferences in science by showing that such inferences are either logically flawed if irreducibly analogical or subject to logical interpretation that eliminates the analogy (thus depriving the analogical component of any independent epistemic role). My aim is to refute this argument. In order to do so, I will have to provide an interpretation of the logical structure of scientific analogies that allows for the possibility of their epistemic vindication.

2. The case against analogy

Suspicion of analogical, resemblance-based forms of reasoning has a long philosophical pedigree. For the skeptic, analogies are best understood not as legitimate forms of reasoning meant to provide evidence for some conclusion, but instead as psychological shortcuts. Analogical thinking may be practically useful in the messy world of everyday choice and action, and thus of interest to the cognitive psychologist, but analogical inference is not a rigorous form of reasoning that can stand up to careful logical scrutiny. Analogy belongs among the methods or processes of discovery (the heuristic resources human agents use to come up with new ideas and hypotheses), but has no role in the logic of justification, understood as a rational reconstruction of the evidence available for a particular conclusion. On this view, analogical thinking may prove heuristically valuable, but it fails to provide any justification or epistemic support.¹

What is the source of this skepticism about analogical reasoning (if it can be described as such)? One complaint is that purported cases of analogical reasoning turn out, on closer inspection, to rely on a series of background arguments. Whatever strength analogical arguments seem to have as epistemic vehicles is really derived from these background arguments; analogies are logical borrowers that deserve no epistemic credit themselves.

However, this objection is hardly peculiar to analogical reasoning; one could claim that all forms of ampliative inference,

¹ “Merely heuristic” and similar phrases often carry a derogatory sense, implying some sort of second-class or pseudo-epistemic status. However, it is important to note that the skeptical position described here need not be dismissive of heuristic devices, nor deny their importance in the development of scientific theories. Scientific models can reasonably be divided into two broad categories: exploratory and explanatory. Exploratory models are heuristic devices for hypothesis formation and extension, and it is now commonly agreed that they play an indispensable role in scientific theory formation. Explanatory models purport to capture (to some degree of grain) the underlying mechanisms responsible for the phenomena under investigation. The analogical skeptic can be understood to hold that all irreducibly analogical models are exploratory rather than explanatory.

such as straightforward inductive generalizations, rely on background arguments and assumptions. As Goodman (1983, pp. 81-83) has shown, the logical status of inductive inferences depends on all sorts of factors external to the argument itself (such as the projectability of the predicates). No ampliative inference can be logically evaluated on purely formal grounds—all have total evidence conditions that require the consideration of any relevant background information. In this sense, all ampliative inferences are logical borrowers. So, as grounds to reject analogical reasoning specifically, the mere fact that it relies on background arguments is insufficient.

The following rejoinder is open to the analogical skeptic: while all forms of ampliative inference depend on background assumptions in some sense external to the argument itself, it is not the case that these arguments can be totally replaced by such background arguments. Thus, while logical assessment of an inductive generalization requires that we consider any relevant background information, we cannot simply eliminate the generalization itself in favor of these background considerations. In the case of analogies, however, critics contend that once such background knowledge is made explicit, the analogy itself becomes superfluous. Keynes (1957), Nagel (1961), and Hempel (1965) all reach the conclusion that the analogy itself does no epistemic work. Elimination of the analogical component is not only possible, it is preferable, since only then will we have a clear account of whatever evidence may exist. Hempel states the matter succinctly: “For the systematic purposes of scientific explanation, reliance on analogies is thus inessential and can always be dispensed with.” (Hempel, 1965, p. 441)

Most analogical skeptics hold something like the eliminativist view just outlined. Arguments for analogical eliminativism focus on the dependence of analogies on unanalyzed similarity or resemblance relations. I will examine Joseph Agassi’s concise version of this common argument.

In his article “Analogies Hard and Soft” (1988), Agassi argues that analogical thinking cannot constitute a legitimate form of inference. He supports this claim with the following dilemma:

- [D] for any putative argument by analogy, either (1) the thinking is merely suggestive (in the heuristic sense) and does not establish any conclusion, or (2) the argument can be reductively analyzed into a non-analogical form.

Since these two possibilities are, Agassi contends, exclusive and exhaustive, the dilemma entails that analogies cannot constitute a legitimate form of inference.

Agassi's argument for [D] stems from a challenge he poses to analogical thinking. Analogical thinking depends on the existence of similarity or resemblance relations between the analogue and the target. However, Agassi contends that in order to have a clear understanding of this "inference", we must be able to analyze the purported resemblance. In other words, we need to know what features or properties the analogue and target share in order to determine whether the inference is at all justified. Confronted with the challenge to specify the similarity or resemblance relation, there are only two possible results: either (a) the shared properties can be analyzed and elucidated, or (b) they cannot. According to Agassi, if the features or properties that the analogue and the target share can be specified, then the claim that the two are analogous becomes superfluous. If one identifies in what respects the analogue and the target resemble each other, then the claim that they are similar can be eliminated and replaced with an assertion of common class membership, or with the claim that they both fall within the extension of some predicate. Once this is accomplished, the road is clear to replace the analogical argument itself with either a straightforward inductive generalization, or a statistical syllogism, or some combination of the two. However, if one cannot specify with any clarity the shared features or properties that constitute the resemblance between the analogue and the target, then the thinking remains unanalyzed to such a degree that we cannot claim to understand the inference in any rigorous sense. Clearly, we cannot logically evaluate any inference that we do not understand. Though the fuzzy sort of comprehension we have of the unanalyzed similarity relation might well be suggestive and a spur to interesting new ideas, it cannot provide evidentiary support for any conclusion regarding the target.

Perhaps a mundane example will help illustrate Agassi's point. Consider the following bit of analogical thinking:

"Hey coach, I've got a great idea for a new player for our volleyball team. Her name's Stacy, and though she has never played volleyball, she's a great basketball player."

In standard analogical format, the reasoning seems to be the following:

1. Stacy's basketball skills are great
 2. Basketball skills are similar to volleyball skills.
- ∴ Stacy's volleyball skills are great.

To analyze the basis for this inference, we need to ask in what respects the analogue (Stacy's basketball skills) is similar to or

resembles the target (Stacy's volleyball skills). If this question cannot be answered, then the purported reasoning cannot be rigorously analyzed, and the logic of the argument cannot be assessed. However, in this case it would seem that some specification of shared features is possible. Basketball skills and volleyball skills both involve hand-eye coordination, leaping ability, and quickness. Both sports require the ability to coordinate play with teammates, and in both cases height is an advantage. The list of shared features could be developed further, and we could assign a predicate label *S* to summarize these properties. With the similarity relation thus analyzed, we can eliminate the deceptive analogical form, and replace it with the following non-resemblance based series of generalizations:

1. Most people who are good basketball players are people with skill set *S*.
 2. Stacy is a good basketball player.
 3. Stacy probably has skill set *S*. (from 1 & 2)
 4. Most people with skill set *S* are people who are good volleyball players.
- ∴ Stacy is probably a good volleyball player.

For Agassi, “[t]he simplest (legitimate) approach is to formulate the analogy to the full and discover that it is a generalization.” (1988, p. 402) The clear analytical advantage of replacing the analogical form is the elimination of the vague resemblance claim. So, in summary, either the thinking remains truly analogical (in the sense that it is resemblance-based), and therefore is at best heuristic and does not provide epistemic support for the conclusion, or the analogical element can be eliminated in favor of more rigorous, non-resemblance based forms of ampliative inference.

3. Response to the eliminativist argument

To defeat Agassi's dilemma, I will offer an existence proof for the following claim: there is a class of *prima facie* analogical inferences for which, (a) the analogical component cannot be eliminated in favor of some non-analogical form without important conceptual remainder, and (b) the grounds for these analogical inferences can be specified with sufficient clarity for it to be possible to determine their logical status (i.e., they are not hopelessly vague). The challenge is to produce an example of an experimental model in science for which both (a) and (b) hold; such a result would show that Agassi's two alternatives for analogical thinking are not exhaustive, and that epistemically legitimate resemblance-based analogical inferences are possible.

Though it is not a view I endorse, it is consistent with my position that most or even all analogies in science could fail to satisfy our epistemic criteria for rational acceptability. Thus, my argument is not meant to refute all challenges to the epistemic legitimacy of analogies in science, but instead to show that the epistemic illegitimacy of analogies cannot be established via Agassi's logical eliminativist argument. By defeating the logical objections to analogical inference, my argument leaves open the possibility of their epistemic vindication; whether or not they are in fact epistemically vindicated will have to be decided on a case-by-case basis. (I return to this issue in the conclusion.)

I believe that much of the plausibility of Agassi's dilemma rests on overly abstract conceptions of how models are used in scientific practice. Seeing the inadequacy of the eliminativist approach requires that we look much more closely at how models are employed. In the remainder of the paper, I will offer a fairly fine-grained account of the use of animal models in human fertility studies as an example that satisfies both conditions (a) and (b).²

The example: in the last twenty years, a number of studies have identified an apparent decline in human male sperm quality (measured in terms of sperm count, sperm morphology, and sperm motility) and in human male fertility generally. Numerous scientific studies have made extensive use of animal models to investigate the possible negative effects on human male reproductive fertility caused by exposure to environmental toxins and environmental estrogens. The common protocol for studies that aim to assess effects on fertility is called "The One- or Two-Generation Reproduction Toxicity Study". Under this protocol, the test substance is administered in graduated doses to several groups of males of the animal model. Males of the parental generation are dosed during growth and for at least one complete spermatogenic cycle in order to elicit any adverse effect on spermatogenesis by the test substance. Clinical observations and pathological examinations are performed on the animals for signs of toxicity, with special emphasis on effects on the integrity and performance of the male reproductive systems and on the growth and development of the offspring. The rat is the preferred species for use under this protocol, but studies using mice and rabbits have also been conducted.

The relevance of animal models for human populations is premised on common features shared by both animal test subjects and human beings. In the case of male fertility studies, the relevant common features shared by animal model and human population include the following:

² Information regarding these studies was gleaned from a survey of such studies by I. Mangelsdorf and J. Buschmann (2002).

- processes of male germ cell development are almost identical in both;
- spermatogenesis involves nearly identical processes of coordination of endocrine; autocrine, and paracrine influences;
- factors affecting accessory sex glands, prostate, and seminal vesicles influence sperm quality;
- the processes of normal erection and ejaculation are crucial to male fecundity.

These common features make inferences based on findings in the animal experiments to claims about potential effects on the human population possible.

But are the common features shared by animal model and human population the only relevant factors that a complete analysis of such studies need consider? Not at all. Assessment of species *differences* is also crucial to understanding the evidentiary strength of such studies. Some differences are general to any toxicity study employing animal models. For example, most low dose effects take years to be expressed as disease, and the life spans of animals are often too short to reveal such effects. Further, there can be large differences in sensitivity to toxins between human beings and laboratory animals. Regarding the fertility animal studies specifically, there are important differences in terms of spermatogenesis between laboratory animals and human males to accompany the common features described above:

- sperm are shaped differently in rodents and humans;
- the endocrine requirements for the quantitative maintenance of spermatogenesis may be different in rats and men;
- testosterone alone maintains spermatogenesis in rats, whereas both testosterone and FSH appear to be required for maintenance of spermatogenesis in human males.

Apart from these differences in spermatogenesis, there are also important differences in terms of reproductive parameters between the laboratory animals and human males:

- the human male is of relatively low fertility when compared to most laboratory mammals;
- the human has small testes relative to the rat (0.08 compared to 0.4% of the body weight);
- the efficiency of sperm production per gram of testicular parenchyma is only 20-40% of any other mammal studied;

- the percentages of progressively motile sperm and morphologically normal sperm in human semen are lower than in any other animal models studied;
- the duration of spermatogenesis is longer in humans than in other species;
- large numbers of sperm are needed in the human case because of the enormous reduction in the number of sperm in transit through the female tract: the number of sperm reaching the human ovum is less than 1 for every 100,000 ejaculated.

Further differences can be inferred from the known ones, e.g., since the duration of spermatogenesis in humans is considerably longer than in rats, mice and monkeys, it can be inferred that the duration of recovery after a toxic insult is longer in human subjects than in lab animals.

Returning to our logical preconditions for legitimate analogical models, we can ask first whether the reasoning in the human fertility studies can be analyzed into non-analogical form without conceptual remainder (condition (a)). I contend that the reasoning employed in the fertility studies is analogy-laden, i.e., the resemblance-based, analogical elements cannot be eliminated in favor of some non-analogical form without important conceptual remainder. The eliminativist alternative analysis of these experiments should be rejected because it fails to capture, and thus distorts, the essential analogical character of these ampliative inferences.

The example of animal studies in fertility research demonstrates that an adequate understanding of such models requires that we be cognizant not only of all relevant common features of the sample and the target, but also of the relevant *differences*. It is my contention that the eliminativist non-analogical analysis ignores this essential feature. What would an eliminativist analysis of the animal studies look like? To utilize normal, non-analogical causal reasoning, we must first define a common class that contains both the experimental lab animals and the target population of human males; this class (call it M) is defined by the common features shared by both. In the experiment, sample M's are dosed and later tested for reaction in the manner described above. Results discovered among the experimental population are then extrapolated to the entire M class. The causal inference is captured by the following inductive argument forms:

1. Z% of *sampled* mammals exposed to test substance developed fertility-related properties P.
- ∴ Z% of mammals exposed to test substance develop fertility-related properties P.

1. $Z\%$ of mammals exposed to test substance develop fertility-related properties P.
 2. Human males are mammals.
- \therefore $Z\%$ of human males exposed to test substance will develop fertility-related properties P.

What such an analysis clearly leaves out, i.e., the analogical conceptual remainder, are the relevant differences between the animal model and the target human population. By ignoring this component, the eliminativist reduction gives a distorted picture of the reasoning involved in the use of animal models in biomedical research. In the standard non-analogical case, causal reasoning involves a probabilistic inference from what happens to X's in the model to what will happen to X's outside the laboratory. But animal models are doubly probabilistic, since here researchers make inferences from what happens to X's (some non-human animal model) in the laboratory to what will happen to Y's (human beings) outside the lab. LaFollette and Shanks (1995) put the same point this way: "there is probabilistic causality within the (non-human) laboratory population, probabilistic causality within the human population outside the laboratory, and an uncertainty about whether the results observed in the non-human animal population will be (statistically) relevant to the human biomedical phenomena of interest." The most accurate way to characterize the relation between the non-human animals used in biomedical research and the target human population is to say that they are *analogous*, i.e., that they share certain traits and differ in relevant respects as well. Traditional treatments of analogy (e.g., Mill), as well as more contemporary accounts (e.g., Hesse), coincide in stressing the importance of differences (or disanalogies) in understanding what makes an inference an analogy. And while it is surely wrong to suggest that Agassi and other analogical eliminativists are unaware of these differences, their preferred rendering of the reasoning involved in the use of animal models has the effect of making those differences irrelevant to the evidentiary status of the model. The same point can be made with regard to other models commonly used in science, such as physical scale models, cell and tissue studies, and computer models—they are all analogy laden. In each case, the putative advantages of a non-analogical analysis come at the cost of distortion and misrepresentation.

The role of disanalogy in analogical reasoning is unique. Analogy is the only form of ampliative inference that posits relevant differences between the sample and the target.³ The

³ Of course, difference between the sample and the target will exist in any case (e.g., spatial-temporal indices, etc.). Non-analogical forms of inference posit identity with respect to certain features, but not identity *tout court*. Such strict

eliminativist position gains considerable credibility from the assumption that reducing the similarity-based account to one that avoids such appeals brings greater analytical clarity. However, in cases where relevant differences exist, the reductive approach ignores these important features. Since the eliminativist approach leaves this important conceptual remainder, the reductive analysis should be rejected.

Having established that the human fertility studies cannot be analyzed into a non-analogical form without conceptual remainder, we can now address condition (b): are we in a position to determine the logical structure of such an argument, or are we left with a hopelessly vague understanding of these irreducibly analogical inferences? Can sufficient rigor be achieved within an analogical framework? I contend that the grounds for analogical inferences can be specified with sufficient clarity for it to be possible to determine their degree of evidentiary support. What is required is a set of transformation rules or modeling principles for analogical arguments that would allow us to project or extrapolate results gleaned from study of the analogical model onto the target. Only when such transformation rules can be elucidated do we have sufficient grounds for confidence in the conclusion based on the evidence discovered through study of the model.

A fairly rigorous methodology has been developed in many scientific fields for transferring results from analogical models to various real world phenomena. For example, in the case of toxicology studies using animal models, various researchers have developed what are called “Interspecies Extrapolation Factors” or IEFs. These ratios aim to define the relationship between the dose necessary to produce a toxic effect in a test animal and the dose which produces the same effect in man. Formulation of the general principles for extrapolation from animals to humans requires the consideration of numerous factors. The notion of “equivalent dose” can be determined through consideration of body weight, caloric demand, or body surface. In the case of the fertility studies, general IEF principles for extrapolation of effects on reproductive fitness were obtained for sperm count. Other effects, or “endpoints”, proved more difficult to extrapolate. For example, overall fertility is difficult to extrapolate because of the differences mentioned above and because lab animals remain fertile even if their sperm

identity proves impossible *a priori* for ampliative inferences since there is always at least one relevant difference between the sample and the target: the first was directly investigated (sampled), while the second was not. But this *a priori* distinction is the only difference posited as relevant by non-analogical inferences. For purposes of capturing essential differences between sample and target *qua* sample and target, they are built solely on assertions of common features shared by base-sample and target. Such arguments posit no other relevant differences between sample and target.

counts drop by 90 to 99%. Luckily for the fertility researchers, there are often ways around such problems. Sperm count (for which extrapolation principles are determinable) is strongly correlated with sperm morphology and motility in humans and animals. Thus, intraspecies assessment factors can supplement interspecies extrapolation principles. In other disciplines, similar modeling principles have been developed to make sufficiently precise predictions possible on the basis of results gleaned from analogical models. For example, Eigen ratios perform a similar function to IEFs within the configurational analysis typical of scale models in applied physics.

4. Conclusion

What such methodologies promise is a rigorous way to determine which results will prove transferable from model to target, and thus which analogical inferences merit our confidence. While such modeling principles do not eliminate the relevant differences between the analogue and the target (indeed, the principles themselves would be superfluous if such differences did not exist), they do show how the analogical or similarity-based relation between model and target can escape the charge of being hopelessly vague or merely suggestive. Epistemically justified resemblance-based analogical inferences are possible. As mentioned earlier, this result is consistent with the position that holds that members of some subset of analogical models (or even the entire class) do not meet our epistemic requirements for rational acceptability. For example, LaFollette and Shanks argue for various reasons that biomedical studies that aim to establish causal claims about human populations based on animal testing are all unconvincing. While they reject these studies, LaFollette and Shanks agree that the use of animal models in scientific experiments is inherently analogical, so they accept my condition (a). Further, while they give a negative assessment of the cogency of this particular class of analogical arguments, their rejection is not premised on claims about the irreducible vagueness of resemblance-based analogical reasoning. On the contrary, their negative assessment is based on what they consider a clear understanding of the logical structure of such arguments, and the flaws and uncertainties they claim are revealed through their analysis: thus, my condition (b) is satisfied as well. As these considerations make clear, defeating Agassi-style objections to analogical reasoning does not vindicate the epistemic legitimacy of any specific analogical argument. But that is as it should be. With the *a priori* logical arguments against the possibility of analogical inference cleared away, we are now in position to engage in debate

as to what epistemic criteria an analogical argument must satisfy to be considered convincing.

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