

**Historical Context and Philosophy of Science:  
Reply to Peter Simons' "Coincidence and Kite Flying" (revised 4 March 2009)**

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In his review of my book<sup>1</sup>, *Wittgenstein Flies A Kite: A Story of Models of Wings and Models of the World*, Peter Simons implies that I take a coincidence as my starting point and, from it, attempt to infer a connection (between the methodology of scale models and the view of propositions in Wittgenstein's *Tractatus*.) Actually, Wittgenstein spoke of a connection between scale models and propositions. What I do is provide the historical context to explain what the term "scale model" meant to someone situated as he was when he made the connection, and, more importantly, explain the development of the accompanying notion of physical similarity in that historical context. Simons tries to argue against historical context being very important, but his argument relies upon mistaken ideas about how isolated Wittgenstein was throughout 1914. I feel it is unfortunate that Simons puts so much importance on being able to identify a specific causal path of influence from the contemporaneous landmark paper on the methodology of scale models with which I see a thematic connection, and misstates or neglects so much of the book that should be of interest to philosophers of science.

The important notion in the historical context is that of *physically similar systems*. It is a generalization of the notion of *geometrically similar figures*. That is, it is a generalization of the notion of similarity found in the discipline of geometry to various notions of similarity in physics (kinematic similarity, dynamic similarity, hydrodynamic similarity, electrical similarity, usw.). The notion was quite consciously revived from the Renaissance (specifically from some lesser-known writings of Newton and Galileo) in the late nineteenth and early twentieth centuries. Around 1911 - 1914, the topic of similarity took on heightened prominence in a variety of scientific fields. Thus I spoke of the "variety of publications on similarity" around 1914 (p. x) that shaped the surrounding landscape of ideas, and wrote in the preface (p. vii): "This book traces threads in the background of the first private moment" of an insight that Wittgenstein reported having.

In this reply, I'd like to correct the misconception Simons' review gives (that my book "is about a coincidence" of two isolated moments widely separated in space). I also want to identify and correct the errors of historical fact he makes and relies upon to

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<sup>1</sup> Simons, Peter . "Coincidence and Kite Flying" *Metascience*, March 2009.

argue against the significance of the historical context. Finally, I point out a few things in my book that he omits to mention that tell against some of the other things he says in his review of my book.

Let's get the historical facts straight first. Simons' review is not correct about where Wittgenstein was when. My approach appealed more to what was available in the scientific milieu than to any specific interaction (p. 205), but since Simons relies on where he (mistakenly) thinks Wittgenstein was at various times in 1914 in order to discredit my suggestion that Wittgenstein could have been cognizant of discussions in Vienna about what was going on in science at the time, these errors of factual detail matter to defending my more general claims. Simons states: "Ludwig Wittgenstein, thinking hard in his Norwegian fastness about how propositions represent the world and how logic works, recalled reading about the use of a small model of streets and cars used in a Paris court to represent a traffic accident, and hit upon the idea of a proposition." Now, Simons is just plain wrong about Wittgenstein being in Norway when this happened; as noted on page ix of my book, and per the oft-quoted excerpt from von Wright's widely available memoir:

"There is a story of how the idea of language as a picture of reality occurred to Wittgenstein. [There exist several somewhat different versions of it.] It was in the autumn of 1914, on the eastern front. Wittgenstein was reading in a magazine about a lawsuit concerning an automobile accident. "<sup>2</sup>

Every scholarly biographical work I know of places the event after Wittgenstein entered the military, in the Fall of 1914; I don't think there is any dispute about this. Simons goes on to say, mistakenly, that in 1914 "Wittgenstein had long given up aviation in favour of logic and was geographically and culturally isolated in Norway (the outbreak of the war took him by surprise.)" First, the issue is not whether he had given up aviation, as we are talking about physics here, and one of the premier physics journals at that; Wittgenstein later mentions the very physics journal (*The Physical Review*) that later published the full-length version of the paper Simons is referring to, in a letter to his

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<sup>2</sup> Von Wright, G. H. (n.d.) *Wittgenstein*. Minneapolis: University of Minnesota Press. p. 21

student W. H. Watson.<sup>3</sup> Second, Simons is mistaken here, too, on the facts of Wittgenstein's whereabouts. Actually, Wittgenstein was not isolated in Norway up until the war began: he spent July 1914 in Vienna, and did not enter military service until early August. I state this in the book (e.g., p. 204), it is reflected in many biographies, and it is definitively established by a letter he wrote from Vienna on July 3rd, 1914.<sup>4</sup> I might add that, as mentioned in my book, July 1914 was the month that the National Academy of the Sciences issued the brief communication "On Physically Similar Systems" by Edgar Buckingham, a sort of synopsis of the main theoretical points in his forthcoming paper of the same name.

There was one other break in Wittgenstein's seclusion in Norway, and I mention it in the book as well -- Christmas of 1913, which he spent in Vienna at the wishes of his mother. Again, that he was in Vienna then is definitively indicated by a letter he wrote to Russell from Vienna dated Christmas 1913.<sup>5</sup> And here I might add that it just so happens that December 1913 was when Heike Kammerlingh Onnes gave his Nobel acceptance lecture on the use of mechanical similarity in conjunction with the kinetic theory of gases. I discuss the significance of Onnes' lecture, with its discussion of the theory of corresponding states, in Chapters 6 and 7 of the book.

Thus, Simons claims about how isolated Wittgenstein was in 1914 are mistaken. Now, about the significance of the year 1914, the year in which Wittgenstein said he had his insight. The year 1914 was a decisive one in the history of similarity, in which the insights that Galileo and Newton had about how to generalize the notion of similarity from geometry to mechanics was finally made fully general, and given a formal basis. The year began with a landmark paper by Stanton and Pannell in 1914 synthesizing the theoretical work in physics and the experimental work done at Britain's National Physical Laboratory on "similarity of motions." The formal basis for the method of physical similarity, especially dynamic similarity, was presented mid-year by Edgar Buckingham, a philosophically minded physicist drafted to serve on the US Advisory Committee on Aeronautics, but he laid no claim to developing it, only to articulating it clearly and laying

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<sup>3</sup> *Wittgenstein in Cambridge: Letters and Documents 1911 - 1951*. Edited by Brian McGuinness. Oxford, UK: Blackwell Publishing. Revised edition, 2008. Letter number 163 from Wittgenstein to W. H. Watson.

<sup>4</sup> Ibid. Letter number 43 from Wittgenstein to G. E. Moore is dated July 3rd, 1914, and written from the family home on what was then the outskirts of Vienna.

<sup>5</sup> Ibid., Letter number 33.

out its foundations. He did so because the state of flight research at the time lacked such a reference articulating the theoretical foundations of the method in use. As I explain in my book, there was a gradual progression from specific notions of similarity to more general accounts of similar motions in hydrodynamics, to the more mathematical, and finally, logical accounts of it in the early twentieth century. Galileo had referred to geometry when he spoke of the book of nature being written in the language of mathematics; Buckingham's more general account of similarity and invariants gives this place to any symbolic system having certain features and expressive capabilities, and formalizes similarity in such a way that the important consequences about similarity follow from a principle of logic. Of course his account of the foundations bore some marks of his own academic training in Europe under Ostwald, and reflected his philosophical inclinations.

However, I do not, as Simons implies I do, point to a single person and a single theorem in isolation from the intellectual currents of the time. What I give is an account of some currents in the intellectual and scientific milieu, one of which is the attention directed to the urgent problem of the credibility of the use of experimental models (people knew how to build and use experimental models; what they lacked was an account of the validity of what they were doing that would satisfy physicists and mathematicians.) The ideas about physical similarity found in those currents in 1914 are the culmination of centuries of work on similarity from Galileo onwards, including Newton, Helmholtz, Fourier, Froude, Reynolds, and Rayleigh, to mention only a few. I discuss a great number of discussions about this problem at the time by others (e.g., Stanton and Pannell's work, Gibson's text, James Thompson's anthology of papers on similarity published in 1913). I discuss other work related to similarity, too, besides the efforts to justify inferences from experimental models in wind tunnels.

That these ideas were circulating in those currents around 1914 is reflected in D'Arcy Thompson's work, which later led to his *On Growth and Form*, a book containing one full chapter devoted to dimensional analysis. Much of the literature he cites about scale in biology in that chapter is work that was done in the service of solving problems about flight. Among the many other things I discuss is the explicit mention in physics venues of Galileo's *Two New Sciences*. The work is known for its discussion of the problem of how one can make inferences about a large machine from the behavior of a small machine. There was a publishing project underway then to remedy the fact that no

translation of it was then readily available. I discuss Heike Kammerlingh Onnes' crediting of Isaac Newton for the inspiration to apply mechanical similarity to the molecular motion of gases. I presented these as part of the milieu in which Wittgenstein was thinking about logic, but it turned out a more concrete connection existed, too: it has recently been discovered that some of the books in Russell's library originally belonged to Wittgenstein. (Hide 2004) One of these was a copy of Galileo's *Two New Sciences* purchased before the new translation was available: at the time Wittgenstein purchased it, it was rare and very expensive and a somewhat unusual purchase; he owned no other works by Galileo. Another of the books belonging to Wittgenstein was the very work of Newton's in which he discusses his use of mechanical similarity. I discuss the significance of the finding of both these books in my book (p. 130 - 134), so I think it unfair of Simons to begin his review with the statement: "This book is about a coincidence", then mention only a single proof by Buckingham.

Simons also misleads by implying that I mention the paper by Buckingham mainly for a proof it contains. He misstates the significance of the paper by implying that it is just about a proof, then misstates the gist of the proof as one about reducing the number of variables in an equation of the sort used in physics. Buckingham's 1914 papers on physically similar systems are about how to establish that two physical systems are physically similar, which doesn't depend upon that proof. There are two kinds of equations under discussion: (i) physical equations, such as occur in science, and (ii) dimensional equations, which are of a different sort logically speaking. There are likewise two distinct points that Simons runs together: one is that a physical equation (the kind of equation used in physics) can be rewritten as an undetermined function of dimensionless parameters. The significance of this is that the system behavior is determined by a (nonunique) set of dimensionless parameters, even if the experimenter cannot predict or describe the system's behavior in terms of them. The other point is that the dimensional equations can be used to determine the minimum number of dimensionless parameters needed to characterize the behavior of a system, even in the absence of knowledge of the specific governing equation. The theorem is about determining that minimal number.

Simons takes the advance in Buckingham's "On Physically Similar Systems: Illustrations of the Use of Dimensional Equations" to be just about the theorem. Perhaps it is no wonder, then, that Simons doesn't think there is much of logical significance here.

It seems to me that Simons, who does not mention the difference between physical equations, dimensional equations, and undetermined relations between dimensionless parameters, misses something important in not even mentioning the emphasis I put on the role of dimensionless parameters, too -- these are not by any means special to Buckingham's paper, but run throughout works in the history of similarity. It is equality of dimensionless parameters that establishes similarity of physical systems. This is a very central point about similarity. Here I see an analogy with the role of facts on the account in the Tractatus. To avoid reproducing the bulk of the last chapters of the book here, I simply refer the reader to the book.

To Simons' complaint that the analogy I draw looks only superficial so far as he can tell, I can only say that one can lead a person to a pattern, but cannot make him or her see what one sees in it. I would also emphasize that although I cite a specific paper, the concepts and framework I cite are not one person's view: what I cite is the method of physical similarity, the notion of physically similar systems. The framework I cite in drawing the analogy is as basic to the method of physical similarity as the basics of differentiation and integration are to calculus or the methods of algebra are to algebra. That Simons centers his review on the question of a causal path from Buckingham to Wittgenstein indicates that I may not have made this as clear as I should have. So the claim I am making in drawing the analogy is mainly that the view in the Tractatus mirrors the methodology of physical similarity, which is the methodology implemented in using scale models. I can only suggest studying the analogy I have laid out, while well aware of the tedium this involves until the analogy is grasped.

Here it seems to me that the aforementioned failure to distinguish between physical equations and dimensional equations is an obstacle for Simons. He writes: "the kind of independence that Wittgenstein envisaged holding between propositions was wholly different from the independence of variables in a physical equation. One is logical, the other functional, independence." Actually, on page 239 of the book I point out that independence of dimensionless parameters is established as a matter of logic. This is in part why Buckingham said that so much that had earlier been thought to be an empirical matter actually followed from a principle of logic. I wouldn't blame Simons for an initial disbelief of my claims here; that would only be natural to anyone who understands something of the significance of what I claim in drawing the analogy. The first reaction of anyone who really understands what is being claimed or revealed by Buckingham's

formulation is disbelief that certain things could possibly be a matter of logic alone. As someone has put it, at first dimensional analysis seems like magic. In more recent work, I have delved more deeply into where this information comes from; it has to do with the empirical content already built into the relationship between systems of units suitable for use in a physical science and systems of scientific laws. (Sterrett 2006, Sterrett 2009) As for Simons' complaint that Wittgenstein's recursive application of a general truth-functional operator is very different from equations involving "products of powers of quantities", as he puts it, Simons neglects to mention what I do say about operations: that dimensions on Buckingham's account are regarded as the results of operations and that the exponent indicates that the operation has been recursively applied. (p. 191 - 192) In fact, I point out that what numbers turn out to be are exponents of operations, on both the exposition Buckingham gives of dimensional equations, and on the account Wittgenstein gives in the Tractatus. Here again I refer the reader to my book (p. 192) , to avoid reproducing lengthy portions of it here. I thank Simons for paying so much attention to the details of the analogy presented in the book, though. These are crucial points to look at. I expect that a better understanding of the logic of dimensions and dimensional equations might clear up the points at issue and, hopefully, reward such headache-inducing efforts.

I have two other criticisms of Simons' review that are matters of omission. The first is that he neglects to mention that all of Wittgenstein's journals, notes, and papers prior to 1914 were destroyed (at his request). Thus, as I mention in the book at several places, one cannot take lack of evidence of this sort as evidence of lack of any particular interest on Wittgenstein's part. As for actual documents from this period of Wittgenstein's life, there is little more than the letters he wrote that others have kept. I did not think that I did anything to raise expectations that I would establish a specific causal connection, as Simons implies I do --- just the opposite: from the beginning, on pg. xx in the Preface, I clearly state that any direct evidence in the form of his journals, notes, or papers he read was unavailable. (The books previously owned by Wittgenstein that were recently found in Russell's library are from that period and, as mentioned above, bear out my suggestion of an interest in physical similarity.) Although I did describe an initial motivation of investigating a connection between Buckingham's paper and Wittgenstein, I thought that I made it clear that I soon realized that there was no other choice than to

proceed as I have done, by triangulation from other currents in his milieu and from what he wrote that was not destroyed (e.g., p. xx, p. 205).

My other complaint about omissions in Simons' review is that he neglects to mention the new critical-historical contributions in the book about Wittgenstein's childhood that I dug up: the discussion of phonautographs (sound records never meant to be played back, but only to be viewed) that were standard fare in children's science textbooks of the era, and the prominent role that the problem of scaling played in one of the most popular boys' novels of the time: H. G. Wells' *Robur the Conqueror*. The first is discussed at length in the first chapter and is the subject of a self-standing philosophical paper (Sterrett 2005a) in which I draw attention to his emphasis on gramophone discs, which are meant to be played back, over the phonautograph type of representation in the passage about gramophones in the *Tractatus*. The problems of flight explicitly discussed in the H. G. Wells' youth novel connect directly with a popular lecture on aeronautics that Boltzmann gave in Vienna practically around the corner from Wittgenstein's home when he was six years old. Why Simons does not even mention the elements in the childhood milieu that I include among the threads in the background of the moment of insight in 1914, but brings up jet engines (which are from a later era and are not mentioned in my book) is a little puzzling. The threads from the milieu in Wittgenstein's very early life that I lay out in the beginning of the book are picked up near the end of the book and woven into the account I give.

Simons uses the metaphor of kite-flying to have a little fun at my expense in the closing of his review. Replying with a like metaphor, I might say: History indicates one ought not be too hasty in saying whether something can fly or not -- especially on the basis of pure thought. I appreciate that Simons has at least tried to grasp the analogy the book presents, and have some sympathy with his frustration. It is not a simple analogy. Yet I hope that at least the general gist of the analogy can be comprehended without too much trouble. I wrote the book so that it might provide something of interest for anyone who reads it, not least of all philosophers of science.

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