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On Theories

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3.1 Poincaré on "true relations"

Poincaré's discussion of what he refers to as "true relations"—relations in the form of functional relations connecting various physical parameters—occurs in the chapter of *Science and Hypothesis* entitled, 'The theories of modern physics.' The discussion is guided by two observations: (1) physical theories are often *indifferent* to the nature of the constitution of the things whose behavior they seek to describe; (2) physics thrives on the discovery of relations which yield various different but concordant determinations of physical parameters; these are the true relations Poincaré wishes to highlight. We will see how these two observations inform Poincaré's remarks on modern physical theories and, in particular, how the emphasis he eventually came to place on true relations led him to change his view of the hypotheses about which physics should—and should not—be indifferent. In addition to being supported by the relevant texts, the account of Poincaré I will propose allows us to understand very clearly why he found Perrin's research on Brownian motion, together with certain other discoveries which accompanied it, so decisive for the demonstration of molecular reality.

Those of Poincaré's remarks about the primacy of relations that derive from (1) may have been partly suggested by his familiarity with Newton's discovery that for many astronomical calculations it suffices to consider just the centers of mass of the bodies whose motions we seek to describe. This is not a philosophical thesis but a mathematical theorem that depends on demonstrable properties of the relevant forces, and it evidently allows us to set entirely to one side the complexities which questions of the constitution of bodies raise. In this case, our ability to ignore the "nature" of the objects under study does not rest on a general view about their opacity to further study. But although the example of Newton may have been importantly suggestive, it is clear that it does not exhaust the considerations which led to Poincaré's emphasis on the *indifference* of physical theory to the constitution of the things whose dynamical behavior it seeks to describe.

Poincaré cites three examples to illustrate the thesis that the abiding import of physical theories consists in their ability to direct our attention to true relations. The first example concerns the isolation and characterization of periodic phenomena. We are in possession of a characterization of periodic systems that is general and abstract in the sense that it is independent of the mechanical or electrical nature of the periodic process to which it can be applied. Its discovery was facilitated by theoretical principles of energy conservation and least action, and the characterization captures both the periodicity of the motion of a pendulum and the periodicity of an electric current. Poincaré cites this example to stress that the recognition of the periodic character of an electric oscillation is an important advance whatever our view of the source of such oscillations. The example illustrates the fact that theoretical advances often abstract over specific commitments to the nature and constitution of the physical systems and the origin of the phenomena to which they can be successfully applied. Of the three examples we will be considering, this one comes closest to supporting a "structuralist" interpretation of him: periodicity is a property that is susceptible to a purely abstract mathematical characterization. But there is a large gap between the perception that physics benefited from the trend toward abstraction that characterized mathematics toward the end of the nineteenth century and the thesis that our knowledge of the material world is restricted to its mathematical or structural properties.

Poincaré next cites an example which illustrates the possibility that diverse and even mutually incompatible theoretical starting points can agree in the relationship they find between two physical parameters, namely, the relation between absorption and anomalous dispersion.[83] Of this example, Poincaré writes:

> Numerous theories of dispersion have been proposed. … But, what is remarkable, is that all the scientists who came after Helmholtz reached the same equations, starting from points of departure in appearance very widely separated. I will venture to say that these theories are all true at the same time, not only because they make us foresee the same phenomena, but because they put in evidence a true relation, that of absorption and anomalous dispersion. What is true in the premises of these theories is what is common to all the authors; this is the

[83] Normally waves of lower frequency advance through a medium with greater speed than waves of higher frequency because of the relation between the velocity of the waves and the refractive index of the medium. When a medium exhibits anomalous dispersion, the speeds of for example red and violet light are reversed.

> affirmation of this or that relation between certain things which some call by one name, others by another. (1902, authorized (Halsted) translation, p. 141)

Poincaré's stress on the agreement in equations is not an abstract metaphysical claim about the importance of "pure form" or structure. As Poincaré notes on the immediately preceding page, the point of the equations in which he is interested is to express relations; and in the present case, the equations on which everyone since Helmholtz converged express the relation between the known quantities of absorption and anomalous dispersion.

The sense of this passage is seriously distorted in the unauthorized translation, reprinted in the Dover edition where, on p. 162, we read:

> …But the remarkable thing is, that all the scientists who followed Helmholtz obtain the same equations, although their starting-points were to all appearances widely separated. *I venture to say that these theories are all simultaneously true; not merely because they express a true relation—that between absorption and abnormal dispersion*. In the premisses of these theories the part that is true is the part common to all: it is the affirmation of this or that relation between certain things, which some call by one name and some by another.

The sentence I have italicized omits altogether Poincaré's contrast between foreseeing phenomena and isolating true relations, and it fails to make clear that what Poincaré is concerned to isolate as the common part of these different approaches is the fact that the equations they share express the same relation between absorption and anomalous dispersion. Without this qualification it is easy to misinterpret the sentence with which the passage concludes, and to suppose that the parameters whose relation the equations express might be represented purely abstractly by variables of the appropriate type. Poincaré's point is not this, but is rather the physically interesting observation that despite the fact that the nature of light and the refractive medium were differently conceived in different theories, all of them nonetheless converged on the relation between absorption and anomalous dispersion.

But my primary interest is Poincaré's last example, since it is most closely related to our study of the shift in his evaluation of the molecular hypothesis. It involves the case of a problematic theory—the classical kinetic theory of gases—leading to the nineteenth-century discovery of the correct relation between osmotic and gaseous pressure. Poincaré's point is that the relation to which the example draws our attention retains its interest and importance whatever the status of the molecular model that suggested it:

> The kinetic theory of gases has given rise to many objections, which we could hardly answer if we pretended to see in it the absolute truth. But all these objections will not preclude its having been useful, and particularly so in revealing to us a relation true and but for it profoundly hidden, that of the gaseous pressure and the osmotic pressure. In this sense, then, it may be said to be true. (1902, authorized (Halsted) translation, p. 141)

The background to Poincaré's point may be explained as follows.

When a solution of sugar in water is separated from a pure solvent—such as water—by a membrane that allows water but not sugar to pass, then water forces its way through the membrane and into the solution. This process results in greater pressure on the solution side of the membrane; this pressure is osmotic pressure. Once it was known how to measure osmotic pressure, there arose the question of how to determine its relation to the concentration and temperature of the solution. This was a non-trivial problem which led eventually to extending the kinetic theory of gases to include liquids, a development that figured prominently in Perrin's argument for his connecting link (as we saw in Section 2.3, above). The key to its resolution was van't Hoff's observation that with sufficiently dilute solutions the osmotic pressure is the same as the pressure which the dissolved substance would exert as a gas.

Although the identification of gaseous and osmotic pressure is readily suggested by transposing the model of gas pressure (as the impact of gas molecules on the sides of a container) to the collisions of the molecules of the solution with a semi-permeable membrane, its justification did not require this hypothetical picture, but was made compelling by focusing on the behavior of the relevant parameters without appealing to their atomistic interpretation. Thus it was first discovered that the relation between osmotic pressure and the volume of the dissolved substance—sugar in our example—at a fixed temperature satisfies Boyle's equation, pV = constant. Then it was discovered that the constant in the relation between pressure and concentration varies with temperature in accordance with Gay-Lussac's law, pV/T = constant, and that this constant is independent of the nature of the solvent and the dissolved substance. Finally it was recognized that this constant can be so represented that it has a value very close to that of the universal gas constant. On the basis of these considerations it was concluded that "the osmotic pressure is exactly the same as the gas pressure which would be observed if the solvent were removed, and the dissolved substance were left filling the same space in the gaseous state at the same temperature."[84] It is also possible to express the general significance of the identification of osmotic and gaseous pressure as a discovery that stands whatever the fortunes of the hypothetical model: every mole of any non-electrolytic dissolved substance in a dilute solution has the same characteristic energy regardless of the non-electrolytic substance or the nature of the solvent.

There are three lessons that can be drawn from the example of osmotic and gaseous pressure. First, if we focus on the argument for their identification that is independent of the molecular model of gases and liquids, it is clear that its appeal to the fact that osmotic pressure, like gaseous pressure, satisfies the equations of Boyle and Gay-Lussac, is not in aid of the idea that our knowledge of these parameters, in so far as it rests on their inclusion in certain equations, is "wholly structural." The value of these equations does not consist in their expression of mere "mathematical forms" which are shared by gases and liquids, but in what they express about the relations between the known physical parameters which enter into them. Secondly, reservations about the identification of osmotic and gaseous pressure had nothing to do with the nature of our knowledge of pressure, but were concentrated entirely on the molecular model of gases and liquids. The reasons for such reservations were many, but certainly an important one among them was the fact that a commitment to this model is not needed to motivate the identification. Thirdly, the principal significance of the discovery of the identity of gaseous and osmotic pressure is that it enables the empirical determination of the relation of osmotic pressure to many other properties of solutions; indeed, this is the problem that

[84] The quotation is from the English translation of the first (1893) edition of Nernst's textbook. It is quoted by Seth and Smith to whom my discussion is indebted. (See p. 18 of their *Draft*; see also van't Hoff's Nobel Lecture, 'Osmotic pressure and chemical equilibrium,' December 13, 1901.)

eluded Pfeffer and Clausius and that was solved by van't Hoff.[85] From the perspective afforded by these considerations, it is clear that Poincaré's emphasis on relations is not the expression of any particular philosophical position regarding our knowledge of the properties of bodies.

We have established that there were two sources for Poincaré's emphasis on relations: (1) physical theories are often *indifferent* to the nature of the constitution of the things whose behavior they seek to describe, and (2) physics thrives on the discovery of relations which yield various independent and concordant determinations of physical parameters. We must now consider how Poincaré's emphasis on the importance of physics' discovery of such true relations led him to modify his view of atomistic concepts like those involved in the molecular hypothesis.

In 'Hypotheses in physics,' [86] Poincaré took his analysis of the value of atomistic theories as possibly suggestive guides for future research a step further, and argued that the question of the atomic constitution of matter concerns "an indifferent hypothesis," meaning by this that it is a hypothesis whose assumption is at best a heuristic aid which complements the cognitive style with which some theorists approach their calculations.[87] But in his (1912a) Poincaré came to recognize that, in assigning this methodological status to the atomic hypothesis, he had conflated the question of whether we might dispense once and for all with continuity with the more restricted question of whether the molecular and atomic hypotheses can ever achieve the status of scientific fact.[88] By its

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[85] Cf. van't Hoff (1901).

[86] Ch. 9 of *Science and Hypothesis*.

[87] Referring to the atomic hypothesis, Poincaré writes: "Hypotheses of this sort have therefore only a metaphorical sense. The scientist should no more interdict them than the poet does metaphors; but he ought to know what they are worth. They may be useful to give a certain satisfaction to the mind, and they will not be injurious provided they are only indifferent hypotheses" (p. 142 of the Halsted translation).

[88] "Continuity' is a somewhat inadequate term for expressing Poincaré's idea: it is not merely the use of continuous mathematics in physics that is at issue, but the nature of the underlying medium as well. But it would be incorrect to express Poincaré's thought with 'the continuum' in place of 'continuity,' given the constructive nature of his view of infinity. As noted in Demopoulos, Frappier and Bub (2012, p. 223), Poincaré uses a neologism which, as Melanie Frappier has noted, translates virtually directly as *continuism*; this seems intended to address these considerations, and it contrasts simply with its contrary, atomism. By contrast with the preoccupation of contemporary analytic metaphysicians with the existence of a "fundamental level," Poincaré's interest in the continuism-atomism debate is motivated by the place of continuity, in all its manifestations, in current and future physics.

very formulation, the former question seems to invite a picture of unstable vacillation between alternative resolutions.[89] But Poincaré argued that this is *not* the situation with the latter question in light of the justification the molecular hypothesis acquired with the recent discovery of appropriate relations. The principal one among these relations is the identity of the mean kinetic energies of the Brownian particles and the molecules comprising the fluid in which they are suspended. It afforded a means of empirically determining the values of molecular parameters that had previously been lacking. And as we learned from our discussion of Perrin, Thomson, and others, the concordance between this determination of Avogadro's constant and the determinations of it that are afforded by its relation to parameters of a wholly different character was a key premise in the argument that molecular reality no longer rests on merely hypothetical reasoning.

The *robustness* of the empirical determination of N on which Poincaré laid such stress required the discovery of its functional relation to a variety of empirically determinable parameters, all of which are concordant in the values they yield for N. These results were compelling precisely because of the independence of the sources of evidence they employed. As Poincaré saw, this contrasts with the case where the novelty of a phenomenon is only apparent as it is when its connection with those phenomena for which the hypothesis was originally adduced is so close that any hypothesis which accounts for the one, "must by this very fact account for the [other.…] This is not so when experience reveals a coincidence which could have been anticipated and could not be due to chance, and particularly when a numerical coincidence is involved. Now, coincidences of this type have, in recent times, confirmed the atomistic concepts."[90] Such coincidences are the true relations which, Poincaré argued, constitute the legacy of modern physical theories.

Like others before him, Poincaré was adamant in questioning the persuasiveness of the hypothetical reasoning that is characteristic of the method of hypothesis: such reasoning can yield only an assessment of the molecular hypothesis as an indifferent hypothesis. What Poincaré argues cannot be assigned to chance is the robustness of the

[89] But see the final paragraph of the introduction in Demopoulos, Frappier and Bub (2012) which indicates how the question has acquired a subtlety in contemporary physics that Poincaré could not have envisaged.

[90] Poincaré (1912b, p. 90).

empirical determination of N and of those molecular parameters which are functionally dependent on it. The isolation of the relations that established the well-foundedness of the properties of molecules—the isolation of the relations which facilitated the determination of their properties by robust, theory-mediated measurements—revealed a surprising connection between Poincaré's views by showing how a set of relations involving a variety of independent parameters bear on the accessibility of molecular reality. It changed his evaluation of the molecular hypothesis and led him to elevate the hypothesis from the status of an indifferent hypothesis to the level of scientific fact:

> …since we have a second means to count molecules, absolutely independent from that of M. Perrin, let us compare them; this time we find 650 thousand billion billion. This is a surprising agreement, quite unexpected. You can well understand that a few thousand billion billion doesn't make a difference.
>
> This time, there is cause for wonder, especially since more than a dozen entirely independent processes that I would not be able to enumerate without tiring you lead us to the same result. If there were more or fewer molecules per gram, the brightness of the blue sky would be entirely different; incandescent bodies would radiate more or radiate less, and so on. (Poincaré 1912a, p. 224)

There then follows Poincaré's often-quoted remark, to which we called attention in Section 2.4, that there is no denying that we see molecules.

As we saw in our discussion of Perrin, well-foundedness is a desideratum that the application of hypothetico-deductive reasoning and the method of inference to the best explanation simply overlook. And although the satisfaction of this desideratum does not suffice to establish the molecular-kinetic *theory*, Poincaré argued that it provided an adequate basis for securing molecular *reality*. This recognition did not rest on a change of view about proper scientific methodology—still less on a conversion from one philosophical persuasion to another—but is wholly explained by Poincaré's consistent application of his ideas about the limitations of the method of hypothesis and the need for theoretical parameters to be empirically based in true relations.

3.2 Robustness versus consilience

It is important to distinguish a justification of the molecular hypothesis which, like the one presented here, appeals to the robustness of theory-mediated measurements of molecular parameters, from one which appeals to Whewell's consilience of inductions.[91] The manner in which we are able to obtain information about molecular parameters like N—and the way in which the information so obtained is held to bear on the truth of the molecular hypothesis—is very different in these two accounts. A justification in terms of consilience begins from the premise that when the molecular hypothesis enters into a variety of theoretically different calculations of the same predicted value of N, its confirmation is different in kind from the confirmation it receives from its occurrence in a single calculation. In this respect, consilience constitutes an *enhancement* of the method of hypothesis, rather than a challenge to it.

It is not my purpose to reject consilience-based arguments in favor of the molecular hypothesis, or to argue that consilience played no role in the eventual acceptance of the molecular hypothesis by Poincaré and other of its critics.[92] But it is not the decisive justification of the molecular hypothesis that, I have been urging, was suggested by the discovery of robust, theory-mediated measurements. Moreover, this justification has certain strengths which are not shared by alternatives which appeal to consilience.

An account of Whewell's views on consilience that raises exactly the issues I wish to highlight is given by Laudan:

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[91] See e.g. Krips (1986, pp. 46 – 47) who argues that "[Poincaré's] reason for accepting the existence of atoms can be seen as a special case of what Whewell called 'consilience of inductions,' i.e., an hypothesis gains support because it figures essentially as part of several different explanations of … the *same* phenomenon…." Krips's exclusive focus on consilience of inductions is encouraged by the unauthorized translation of the passage from *Science and Hypothesis* that we remarked on earlier. The passage is quoted by Krips (p. 58) and plays an important role in his account. It will be recalled that the unauthorized translation fails to capture Poincaré's separation of mere predictive success from the discovery of true relations.

[92] In the text surrounding our quotations from his 1912a and 1912b Poincaré recognizes the distinctive confirmation value of independent sources of evidence for the molecular hypothesis and to this extent showed his appreciation of the importance of considerations deriving from consilience.

> If, instead of being able to predict only phenomena of the same kind as the hypothesis was invented to explain, we can explain and predict with its help, cases *of a different kind* (relative of course, to other theories), then we have *indubitable* evidence for the truth of our theory:
>
>> These instances in which this [consilience] has occurred, indeed, impress us with a conviction that the truth of our hypothesis is certain. No accident could give rise to such an extraordinary coincidence. No false supposition could, after, being adjusted to one class of phenomena, exactly represent a different class, where the agreement was unforeseen and uncontemplated. That rules springing from remote and unconnected quarters should thus leap, to the same point, can only arise from *that* being the point where truth resides.[93]

Laudan then comments that in the conclusion of this passage Whewell

> seems to be suggesting that this attitude is *logically* justified, that it is simply impossible in principle that any hypothesis could achieve a consilience unless it were the true hypothesis for explaining the phenomena under investigation. But neither in this passage nor elsewhere does Whewell offer any valid argument to support his logical (as opposed to his psychological) claim.[94]

In support of his assessment Laudan presses the point that it is not possible to transform an argument based on the predictive success of a hypothesis—even predictive success in the form of consilience—into a logical demonstration of its truth. This undoubtedly correct observation would have to be taken into account in any assessment of the utility of a consilience-based justification for an existence claim. But the cogency of the justification existential hypotheses receive from robust theory-mediated measurements does not rest on the possibility of turning a non-demonstrative inference

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[93] Laudan (1981, pp. 168 – 169, all italics are Laudan's). To his quotation from Whewell's *Philosophy of the Inductive Sciences* (Vol. 2, p. 65) Laudan adds a footnote in which he remarks that "[i]n a similar vein [Whewell] observes [on p. 285 of the same work] that 'when the explanation of two kinds of phenomena, distinct and not apparently connected, leads us to the same cause, such a coincidence does give a reality to the cause, which it has not while it merely accounts for those appearances which suggested the hypothesis.'"

[94] Laudan (1981, p. 169, Laudan's italics).

into a demonstrative one. The fact that the parameters which qualify the hypothetical entities are empirically determinable sets them apart from entities whose properties are accessible to us only on the basis of the predictive success of the explanatory hypotheses in which they occur. Consilience may be a significant refinement of the notion of predictive success that is appealed to by scientific realists in support of their view that successful prediction would be miraculous if the theory were not true. But this just highlights the difference between the goals of scientific realism—and the method of argument by which it seeks to achieve those goals—and our use of robust theory-mediated measurement to establish existential hypotheses.

The fact that we can empirically determine the properties of molecules by robust theory-mediated measurements shows them to be epistemically accessible in a way that hypothetical entities which are merely explanatorily successful are not. As we argued earlier, this is precisely what Poincaré should be understood as having conveyed with his rhetorical remark that in light of Perrin's discoveries there is no denying that we see molecules. Consilience of inductions, like hypothetico-deductive reasoning and inference to the best explanation, misses the distinctive contribution of theory-mediated measurement to the justification of the molecular hypothesis: molecular reality is secure because such measurements show molecules to be epistemically accessible rather than merely indispensable to the explanatory hypotheses in which they occur.

3.3 Poincaré and scientific realism

The representation of Poincaré as an early advocate of a scientific realist defense of science rests largely on an interpretation of certain of his appeals to chance. One appeal which has been cited in support of this interpretation occurs in 'Hypotheses in physics':

> We have verified a simple law in a considerable number of particular cases. We refuse to admit that this coincidence, so often repeated, is a result of mere chance and we conclude that the law must be true in the general case.
>
> Kepler remarks that the positions of the planets observed by Tycho are all on the same ellipse. Not for one moment does he think that, by a singular freak of chance, Tycho had never looked at the heavens except at the very moment when the path of the planet happened to cut that ellipse… [I]f a simple law has been

> observed in several particular cases, we may legitimately suppose that it will be true in analogous cases. To refuse to admit this would be to attribute an inadmissible role to chance.[95]

According to the scientific realist interpretation, this passage shows Poincaré to be endorsing the notion that the best explanation of the predictive success of a theory like Kepler's is that it is approximately true, since its success would otherwise be a miracle—"a singular freak of chance." In light of our reconstruction of the nature of Poincaré's argument for the molecular hypothesis as a justification that allows for reservations about the truth of the theories of which it is a part, there should be a far less tendentious and more compelling interpretation of the passage. And indeed there is: Poincaré's remark is entirely captured by the claim that, on the assumption that Tycho's sampling of planetary positions is fair, *ordinary inductive reasoning* suffices to account for the methodological basis of what is nondemonstrative in Kepler's derivation of his law.

Given Poincaré's consistent hostility to the method of hypothesis, the scientific realist suggestion—that for Poincaré the truth of a hypothesis can be inferred from its predictive success—is even less plausible in cases like the molecular hypothesis, and similar constructive components of theories, than it is in the case of Kepler's law. Scientific *structural* realists have attempted to address this point by distinguishing an inference to *structure* from one to *content* or *ontology*, and then characterizing the realism they advocate as a combination of realism about structure and agnosticism about ontology. Poincaré was certainly deeply skeptical of the possibility of forming a scientific judgment about atomism, which is of course an ontological and contentual assumption even when it is glossed as the assertion that matter has a granular structure. So his eventual change of view—focused as it was on an ontological or contentual question about the constitution of matter—should be deeply puzzling to a structural realist interpretation of him. And it counts against both structural and nonstructural scientific-realist interpretations that Poincaré's rejection of his earlier skepticism did not rest on his

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[95] Quoted by Worrall (2012, p. 81). Worrall's quotation is from the Dover edition of *Science and Hypothesis*, pp. 149 – 150; for the Halsted edition see p. 133. There is no essential difference between the translations of this passage in the two English editions.

having come to embrace the thesis that the predictive success of the molecular hypothesis would be miraculous were it not true.

The basis for Poincaré's mature view of molecular reality was the same as Perrin's: once it could be shown to be epistemically accessible by robust theory-mediated determinations of the properties of molecules, molecular reality was on the path to being secured. But this is a justification of the molecular hypothesis that can be maintained in the face of serious questions about the truth of our theories of molecular reality. It would therefore be a mistake to represent Poincaré as having mounted a defense of science that was a pre-echo of scientific realism. Poincaré's view is more nuanced than this and combines both realist and non-realist aspects. It is realist in so far as it fully supports the reality of entities that transcend observation; but it does so on the basis of experimental and theoretical advances within science that are related to measurement. By *not* resting on the supposition that the predictive success of theories would be miraculous were they not at least approximately true, Poincaré's position differs from standard conceptions of scientific realism. It is however important to emphasize that his methodological remarks avoid certain aspects of realism without falling victim to the then emerging positivist consensus—represented perhaps most prominently by Mach—that theory should be reduced to observation. Mach expressed this thesis with a distinction between theories that use only "direct descriptions"—by which he meant abstract principles which employ only descriptions of what is observable—and theories that incorporate "indirect descriptions," which go beyond what is observable. It is, according to Mach,

> not only advisable, but even necessary, with all due recognition of the helpfulness of theoretic ideas in research, yet gradually, as the new facts grow familiar, to substitute for indirect descriptions direct description, which contains nothing that is unessential and restricts itself absolutely to the abstract apprehension of the facts.[96]

Poincaré nowhere commits himself to Mach's view of the preferred relation of theory to observation or to what is typically represented as Mach's view of the prominence of observation. And to the extent that for Poincaré the value of theories is "instrumental,"

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[96] Quoted by Laudan (1981, p. 209).

this must include their instrumental value in revealing relations which guide us to a representation of the constitution of a reality that lies hidden from observation.

3.4 Russell and Poincaré

In his review of *Science and Hypothesis*, Russell said of Poincaré that he holds "[q]uestions concerning the real, as opposed to the relation of real things, … to be illusory and devoid of meaning (pp. xxiv, 163)." He then continues,

> Certainly we have much more belief in the accuracy of our perceptions of relations than in that of our perceptions of qualities. When we see green in one place and red in another, we are willing to believe that secondary qualities are subjective, but not that the fact of difference between what is in the two places is an illusion. It is only by holding fast to relations as perceived that science manages, on an empirical basis, to construct a world so different from that of perception. Why we should trust in our perception of relations I do not know; but it is a fact that we do so. But I do not see how it can be maintained that questions as to the qualities of real things are *unmeaning*. The proposition amounts to this, that if *a* really exists, a statement about *a* has no *meaning* unless it asserts a relation to a *b* which also really exists. The fact seems to be, not that such propositions are unmeaning, but that, except in psychology, they are unknowable. We may even push the theory further, and say that in general even the relations are for the most part unknown, and what is known are properties of the relations, such as are dealt with by mathematics. And this, I think, expresses substantially the same view as that which M. Poincaré really holds.[97]

The passages Russell cites show Poincaré to be at the very least equivocal between the unknowability of certain questions and their meaninglessness. In the first passage (p. xxiv) Poincaré writes that "the aim of science is not things in themselves, as the dogmatists in their simplicity imagine, but the relations between things; outside those

[97] Russell (1905, pp. 76 – 77, Russell's italics). Russell's review appeared in 1905 in *Mind*. My page references to are to its reprinting in the 1966 edition of his *Philosophical Essays*. Russell's citations of *Science and Hypothesis* refer to the English edition that was subsequently reprinted by Dover in 1952. I am indebted to Stathis Psillos for reminding me of Russell's review and for calling my attention to the fact that it contains what is likely the earliest formulation his structuralism.

relations there is no reality knowable." While in the second (p. 163) he defends the exclusion of the question of the truth of "the images we have formed to ourselves of reality," on the ground that such "questions which we forbid you to investigate, and which you so regret, are not only insoluble, they are illusory and devoid of meaning." Both passages are part of a broader polemic against certain "dogmatists."[98]

The view Russell attributes to Poincaré is evidently very close to one he himself favors. Russell's own theory arises from reflection on what can be inferred about the causal sources of the objects of perception on the basis of our perceptual experience. Coming as he does from the tradition of British empiricism, Russell is skeptical about the thesis that we can know of the qualities of material objects that they are the same as the properties that qualify our experience of them. The passage we have quoted from his review considers whether this doubt should not also apply to the relations between material objects. The relation Russell chooses, in order to illustrate that such doubts are not always warranted, is the relation that holds between two "places" when what qualifies something in one of the places is *different* from what qualifies something else in the other. Notice that this is a purely logical relation, and that since, for Russell, mathematics is just a development of logic, this concession does not contradict what Russell proposes as the central tenet of his theory, namely, that what can be known of the material world is only its mathematical properties. But Russell's main point in this passage is to argue for an emendation to what he takes to be Poincaré's view that relations between material objects are unlike their qualities in being knowable. Russell proposes that Poincaré "push [his] theory further, and say that in general even the relations are for the most part unknown, and what is known are properties of the relations, such as are dealt with by mathematics."[99] This is Russell's structuralist reconstruction of Poincaré.

Russell's assimilation of Poincaré's views to his own version of structuralism

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[98] The remark Russell quotes from Poincaré's Preface is directed at Édouard le Roy.

[99] *Ibid.* Russell's statement is ambiguous: when he writes 'the relations are for the most part unknown' does he mean that most relations between material objects are unknown? Or does he mean that every such relation is for the most part unknown—that is, unknown aside from its mathematical properties? I take him to be claiming the latter, but nothing in my discussion hinges on this choice since I intend to show that Poincaré's views about relations are orthogonal to both claims. It is interesting to note that this ambiguity appears at this very early stage of Russell's thinking about these issues and, as we noted earlier (in Section 1.5), it persists in his exposition of his views as late as *AoM*.

receives considerable textual support from Poincaré's paper, 'Science and reality.' The paper occurs as the final chapter of *The Value of Science*, which was published in the same year as Russell's review. In this paper, Poincaré appeals to the example of the relations of sameness and difference to illustrate the possibility of making objective assertions about phenomena that are themselves irreducibly subjective. The point of Poincaré's remarks is not to draw a contrast between the attribution of relations and the attribution of qualities to physical objects, but to exhibit the possibility of an objective comparison of a relation that holds between my sensations with the relation that holds between the sensations of another, however subjective the sensations themselves. Here is the relevant passage:

> The sensations of others will be for us a world eternally closed. We have no means of verifying that the sensation I call red is the same as that which my neighbor calls red.
>
> Suppose that a cherry and a red poppy produce on me the sensation *A* and on him the sensation *B* and that, on the contrary, a leaf produces on me the sensation *B* and on him the sensation *A*. It is clear we shall never know anything about it; since I shall call red the sensation *A* and green the sensation *B,* while he will call the first green and the second red. In compensation, what we shall be able to ascertain is that, for him as for me, the cherry and the red poppy produce the *same* sensation, since he gives the same name to the sensations he feels and I do the same.
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> Sensations are therefore intransmissible, or rather all that is pure quality in them is intransmissible and forever impenetrable. But it is not the same with relations between these sensations. (Poincaré, 1905, p. 136)

It is therefore not surprising that in a Letter to the Editor of *Mind*, published a year after Russell's review, Poincaré should have concurred with Russell's understanding of him:

> I have said that questions related to the qualities of real things are *unmeaning* because for a question to make sense, we need to be able to conceive of an answer that would make sense. Now this answer could only be made with words and these words would only be able to express psychological states, i.e. subjective

> secondary qualities, that would not be those of real things. At the end of the paragraph he devotes to this question, Mr. Russell says: "We may even push the theory further, and say that in general even the relations are for the most part unknown, and what is known are properties of the relations, such as are dealt with by mathematics. And this, I think, expresses substantially the same view as that which M. Poincaré really holds." Russell is not deceiving himself. That is my thought.[100]

It should, however, be abundantly clear from our earlier discussion that Poincaré's views regarding the qualities of experience—even when expanded to include the qualitative dimension of relations as we experience them—are entirely orthogonal to his views on true relations. Those views represent a contribution to our understanding of the character of contemporary developments in physics—most significantly, to our appreciation of the epistemic status of the molecular hypothesis—rather than a general theory of our knowledge of the external world in the tradition of Russell's causal theory of perception. This point has been missed in the current tendency to see in Poincaré's remarks on true relations an anticipation of Russell's structuralism, or of structural realism.

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[100] Poincaré (1906, p. 142). Translation from the French by Melanie Frappier. I am indebted to Stathis Psillos for calling my attention to Poincaré's letter.

Bibliography (exclusive of works cited in Section 4)

Achinstein, P. (2001). *The Book of Evidence*, (New York: Oxford University Press).

Bernays, P. (1935). 'Sur le platonisme dans les mathématiques,' *L'Ensignement Mathématique* **34**: 52 – 69, translated by C. Parsons as 'On platonism in mathematics,' and reprinted in Putnam and Benacerraf (1983): 258 – 271.

Boltzmann, L. (1964). *Lectures on Gas Theory*, translated by S. G. Brush, (Berkeley: University of California Press).

Bostock, D. (2012). *Russell's Logical Atomism*, (Oxford: Oxford University Press).

Braithwaite, R. B. (1953). *Scientific Explanation: A Study of the Function of Theory, Probability and Law in Science*, (Cambridge: Cambridge University Press).

Buchdahl, G. (1964). 'Theory construction: The work of Norman Robert Campbell,' *Isis* **55** :151-162.

Campbell, N. R. (1920). *Physics. The Elements*. (Cambridge: Cambridge University Press).

Carnap, R. (1934). *Logical Syntax of Language*, translated by A. Smeaton, (London: Routledge and Kegan Paul, 1937).

Carnap, R. (1939). *Foundations of Logic and Mathematics*, in *International Encyclopedia of Unified Science, Vol. I, Part 1*, (Chicago: University of Chicago Press): 139 – 213.

Carnap, R. (1950). 'Empiricism, semantics and ontology,' *Revue Internationale de Philosophie* **4**: 20 – 40, revised and reprinted in Carnap (1956): 204 – 221. Page references are to the 1956 reprinting.

Carnap, R. (1956a). 'The methodological character of theoretical concepts,' in H. Feigl and M. Scriven (eds.), *The Foundations of Science and the Concepts of Psychology and Psychoanalysis*, *Minnesota Studies in the Philosophy of Science Vol. 1*, (Minneapolis: University of Minnesota Press): 38 – 76.

Carnap, R. (1956b). *Meaning and Necessity: A Study in Semantics and Modal Logic*, (Chicago: University of Chicago Press, 1956, enlarged edition).

Carnap, R. (1961). 'On the use of Hilbert's ε-operator in scientific theories,' in Y. Bar-Hillel et al. (eds.), *Essays in the Foundations of Mathematics Dedicated to A. A. Fraenkel*, (Jerusalem: The Magnus Press of Hebrew University): 156 – 164.

Carnap, R. (1963). 'Replies and systematic expositions,' in Schilpp (1963): 859 – 1013.

Carnap, R. (1995). *An Introduction to the Philosophy of Science*, edited by Martin Gardner, (New York: Dover Books, reprint of the Second (1974) edition of Carnap (1966)).

Chalmers, A. (2011). 'Drawing philosophical lessons from Perrin's experiments on Brownian motion: A response to van Fraassen,' *British Journal for the Philosophy of Science*, **62**: 711 – 732.

Cohen I. B. and R. E. Schofield. (1978). *Isaac Newton's Letters and Papers on Natural*

Philosophy, revised edition, (Cambridge: Harvard University Press).

Demopoulos, W. (2003a). 'On the rational reconstruction of our theoretical knowledge,' *British Journal for the Philosophy of Science* **54**: 371 – 403.

Demopoulos, W. (2003b). 'Russell's structuralism and the absolute description of the world,' in N. Griffin (ed.), *The Cambridge Companion to Russell*, (Cambridge: Cambridge University Press): 392 – 419.

Demopoulos, W. (2007). 'Carnap on the rational reconstruction of scientific theories,' in M. Friedman and R. Creath (eds.), *The Cambridge Companion to Carnap*, (Cambridge: Cambridge University Press): 248 – 272.

Demopoulos, W. (2011a). 'Three views of theoretical knowledge,' *British Journal for the Philosophy of Science* **62**: 177 – 205.

Demopoulos, W. (2011b). 'On extending "Empiricism, semantics and ontology" to the realism-instrumentalism controversy,' *Journal of Philosophy*.

Demopoulos, W., M. Frappier, and J. Bub. (2012). 'Poincaré's "Les conceptions nouvelles de la matiere,"' *Studies in History and Philosophy of Modern Physics* **43**: 221–225.

Demopoulos, W. (2013). *Logicism and Its Philosophical Legacy*, (Cambridge: Cambridge University Press).

Demopoulos, W. and M. Friedman (1985). 'Bertrand Russell's *The Analysis of Matter*: Its historical context and contemporary interest,' *Philosophy of Science* **52**: 621 – 639.

Einstein A. (1905). 'On the movement of small particles suspended in a stationary liquid demanded by the molecular-kinetic theory of heat,' in *Investigations on the Theory of the Brownian Movement*, edited with notes by R. Fürth and translated by A.D. Cowper, (New York: Dover, 1956): 1 – 18.

Einstein A. (1906). 'On the theory of Brownian movement,' in *Investigations on the Theory of the Brownian Movement*, edited with notes by R. Fürth and translated by A.D. Cowper, (New York: Dover, 1956): 19 – 35.

Einstein, A. (1921). *Address to the Prussian Academy*, translated by S. Bargmann as 'Geometry and experience,' and reprinted in A. Einstein, *Sidelights on Relativity*, (New York: E. P. Dutton and Co., 1923): 27 – 45.

Feigl, H. (1950). 'Existential hypotheses,' *Philosophy of Science* **17**: 192 – 223.

Freudenthal, H. (1962). 'The main trends in the foundations of geometry in the 19^{th} Century,' in E. Nagel *et al*. (eds.), *Logic, Methodology and Philosophy of Science: Proceedings of the 1960 International Congress*, (Stanford: Stanford University Press): 613 – 621.

Friedman, M. (2012). 'Carnap's philosophical neutrality between realism and instrumentalism,' in M. Frappier, D. Brown and R. DiSalle (eds.). *Analysis and Interpretation in the Exact Sciences: Essays in Honour of William Demopoulos,* (Dordrecht: Springer): 95 – 114.

Galavotti, M. (ed.) (1992). *Frank Plumpton Ramsey: Notes on Philosophy, Probability and Mathematics*, (Bibliopolis: Naples).

Glymour, C. (1980). *Theory and Evidence*. (Princeton: Princeton University Press).

Glymour, C. (2013). 'Theoretical equivalence and the semantic view of theories,' *Philosophy of Science* **80**: 286 – 297.

Gupta, A. (2006). *Empiricism and Experience*, (Oxford: Oxford University Press).

Gupta, A. (2009). 'Definition,' in Edward N. Zalta, ed., *Stanford Encyclopedia of Philosophy* (Spring 2009 Edition), URL: plato.stanford.edu.

Gupta, A. (2013). 'The relationship of experience to thought,' *The Monist* **96**: 252 – 294.

Hacking, I. (1983). *Representing and Intervening*, (Cambridge: Cambridge University Press).

Hallett, M. (2008). 'Reflections on the purity of method in Hilbert's *Grundlagen der Geometrie*,' in P. Mancosu (ed.) *The Philosophy of Mathematical Practice* (Oxford: 2008): 198 – 255.

Halvorson, H. (2012). 'What theories could not be,' *Philosophy of Science* **77**: 183 – 206.

Halvorson, H. (2013). 'The semantic view, if plausible, is syntactic,' *Philosophy of Science* **77**: 475 – 478.

Hempel, C. G. (1965). 'The theoretician's dilemma: a study in the logic of theory construction,' in C. G. Hempel, *Aspects of Scientific Explanation*, (New York: The Free Press, 1965): 173 – 226.

Hempel, C. G. (1963). 'Implications of Carnap's work for the philosophy of science,' in Schilpp (1963): 685 – 710.

Hempel, C. G. (1970). 'On the "standard conception" of scientific theories, in M. Radner and S. Winokur (eds.), *Analyses of Theories and Methods of Physics and Psychology*, *Minnesota Studies in the Philosophy of Science, Vol. 4*. (Minneapolis: University of Minnesota Press): 142 – 163.

Krips, H. (1986). 'Atomism, Poincaré and Planck,' *Studies in History and Philosophy of Science* **17**: 43 – 63.

Lewis, D. (1970). 'How to define theoretical terms,' *Journal of Philosophy*, **67**: 427 – 445.

Lewis, D. (1983). 'New work for a theory of universals,' *Australasian Journal of Philosophy* 61: 343 – 377.

Lewis, D. (1984). 'Putnam's Paradox,' *Australasian Journal of Philosophy* **62**: 221 – 236.

Laudan, L. (1981). *Science and Hypothesis*, (Dordrecht: Reidel).

Maddy, P. (2007). *Second Philosophy: A Naturalistic Method*, (Oxford: Oxford University Press).

Maxwell, J. C. (1875). 'Atom,' *Encyclopedia Britannica, Ninth Edition Vol. 3*.

Merrill, G. (1980). 'The model-theoretic argument against realism,' *Philosophy of Science* **47**: 69 – 81.

Millikan, R. (1917). *The Electron, Its Isolation and Measurement and the Determination of Some of its Properties*. (Chicago: University of Chicago Press)

Millikan, R. (1924). 'The electron and the light quant from the experimental point of view,' *Nobel Lecture,* May 23, 1924.

Nagel, E. (1961). *Structure of Science*, (New York: Harcourt, Brace and World, Inc.).

Newman, M. H. A. (1928). 'Mr Russell's causal theory of perception,' *Mind* **37**: 137 – 148.

Norton, J. (2000). 'How we know about electrons,' in R. Nola and H. Sankey (eds.), *After Popper, Kuhn and Feyerabend*, (Dordrecht: Kluwer): 67 – 97.

Perrin, J. (1910). *Brownian Movement and Molecular Reality*, translated from the *Annales de Chime et de Physique, 8th Series*, September 1909, by F. Soddy, (London: Taylor and Francis).

Perrin, J. (1916). *Atoms*, authorized translation by D. Ll. Hammock, (New York: Van Nostrand).

Perrin, J. (1926). 'Discontinuous structure of matter,' *Nobel Lecture,* December 11, 1926.

Poincaré, H. (1902). *Science and Hypothesis*, in *The Foundations of Science*, (Lancaster PA: The Science Press, authorized 1913 translation of George Bruce Halsted); also the alternative translation which is available as a Dover (New York: 1952) reprint.

Poincaré, H. (1905). *The Value of Science*, authorized 1913 translation of George Bruce Halsted. (New York: Dover, 1958).

Poincaré, H. (1906). 'Letter to the Editor,' *Mind* **15**: 141-143.

Poincaré, H. (1912a). 'Les conceptions nouvelles de la matiere,' *Foi et Vie* **15**: 185 – 191. Citations of this essay are to the translation that appears in Demopoulos, Frappier and Bub (2012).

Poincaré, H. (1912b). 'The relations between matter and ether,' in Henri Poincaré, *Mathematics and Science: Last Essays*, translated by John W. Bolduc (New York: Dover Publications, 1963): 89 – 101.

Psillos, S. (1999). *Scientific Realism: How Science Tracks Truth*, (London: Routledge).

Psillos, S. (2006). "Ramsey's Ramsey sentence," in M. Galavotti (ed.), *Cambridge and Vienna: Frank P. Ramsey and the Vienna Circle*, (Springer: Dordrecht): 77 – 100.

Psillos, S. (2011). 'Moving molecules above the scientific horizon: On Perrin's case for realism,' *Journal of General Philosophy of Science*. Published on-line, September 18, 2011.

Putnam, H. (1962). 'What theories are not,' in E. Nagel *et al*. (eds.), *Logic, Methodology and Philosophy of Science: Proceedings of the 1960 International Congress*, (Stanford: Stanford University Press): 240 – 251.

Putnam, H. (1976). 'Realism and reason,' *Proceedings and Addresses of the American Philosophical Association* **50**: 483 – 498.

Putnam, H. (2012). 'A theorem of Craig's about Ramsey sentences,' in M. DeCaro and D. Macarthur (eds.), *Philosophy in an Age of Science*, (Cambridge MA: Harvard University Press): 277 – 279.

Quine, W. V. O. (1951). 'Two dogmas of empiricism,' reprinted with abridgements and expansions in Quine's *From a Logical Point of View: Logico-Philosophical Essays*, (Cambridge MA: Harvard University Press, Second edition, 1980): 20 – 46.

Ramsey, F. P. (1929). 'Theories,' in *The Foundations of Mathematics and other Logical Essays by Frank Plumpton Ramsey*, (Paterson NJ: Littlefield and Adams, 1931): 212 – 236.

Reichenbach, H. (1920). *The Theory of Relativity and A Priori Knowledge*, translated and edited with an introduction by M. Reichenbach, (Berkeley and Los Angeles: University of California Press, 1965).

Renn, J. (2005). 'Einstein's invention of Brownian motion,' *Annalen der Physik* (Leipzig) **14**, Supplement: 23 – 37.

Russell, B. (1905). 'Review of Science and Hypothesis,' in his *Philosophical Essays*, (New York: Simon and Shuster, 1966): pp. 70 – 78.

Russell, B. (1927). *The Analysis of Matter*, (New York: Dover, 1954).

Schilpp, P. A. (ed.) (1963) *The Philosophy of Rudolf Carnap*, (La Salle IL: Open Court).

Schlick, M. (1918/1925). *General Theory of Knowledge*, translated by A. E. Blumberg, (La Salle IL: Open Court, 1985).

Seth, R. and G. Smith (forthcoming). *Brownian Motion and Molecular Reality: A Study in Theory-Mediated Measurement*.

Smith, G. (2001). 'J. J. Thomson and the Electron, 1897-1899,' in J. Z. Buchwald and A. Warwick (eds.), *Histories of the Electron: The Birth of Microphysics*, (Cambridge: MIT Press, 2001): 21 – 76.

Smith, G. (2002). 'The Methodology of the *Principia*,' in I. B. Cohen and G. E. Smith (eds.), *The Cambridge Companion to Newton*, (Cambridge: Cambridge University Press, 2002): 138 – 173.

Stanford, P. K. (2009). "Scientific realism, the atomic theory, and the catch-all hypothesis: can we test fundamental theories against all serious alternatives?," *British Journal for the Philosophy of Science* **60**: 253 – 269

Stein, H. (1989). 'Yes, but… . Some skeptical remarks on realism and anti-realism,' *Dialectica* **43**: 47 – 65.

Thomson, J. J. (1906). 'Carriers of negative electricity,' *Nobel Lecture,* December 11, 1906.

Thomson, J. J. (1897). 'Cathode rays,' Discourse in Physical Science, Royal lnstitution. Friday April 30, 1897, in W. L. Bragg and G. Porter, (eds.), *Royal Institution Library of Science, Physical Sciences, Vol. 5*, (London: Applied Sciences Publishers): 36 – 49.

Townsend, J. (1900). 'On the diffusion of ions into gases,' *Philosophical Transactions of the Royal Society*, **A 193**: 129 – 158.

van Benthem, J. (1978). 'Ramsey eliminability,' *Studia Logica* **37**: 321 – 336.

van Fraassen B. C. (1980). *The Scientific Image*, (Oxford: Oxford University Press).

van Fraassen, B. C. (2009). 'The perils of Perrin, in the hands of philosophers,' *Philosophical Studies* **143**: 5 – 24.

von Plato, J. (1994). *Creating Modern Probability*, (Cambridge: Cambridge University Press).

von Smoluchowski, M. (1906). 'Zur kinetischen Theorie der Brownschen Molekularbewegung und der Suspensionen,' *Annalen der Physik* **21**:756 – 780.

van't Hoff, J. (1901). *Nobel Lecture*, 'Osmotic pressure and chemical equilibrium,' December 13, 1901

Winnie, J. (1967). 'The implicit definition of theoretical terms,' *British Journal for the Philosophy of Science* **18**: 223 – 229.

Winnie, J. (1970). 'Theoretical analyticity,' in R. Cohen and M. Wartofsky (eds.), *Boston Studies in the Philosophy of Science, Vol. VIII*, (Dordrecht and Boston: Reidel): 289 – 305.

Worrall J. (2012). 'Miracles and structural realism,' in E. Landry and D. Rickles (eds.), *Structural Realism*, (Springer): 77 – 95.