Making Contact with Observations

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1. Introduction

Jim Bogen and James Woodward's 'Saving the Phenomena', published only twenty years ago, has become a modern classic. Their centrepiece idea is a distinction between data and phenomena. Data are typically the kind of things that are publicly observable or measurable like "bubble chamber photographs, patterns of discharge in electronic particle detectors and records of reaction times and error rates in various psychological experiments" (p. 306). Phenomena are "relatively stable and general features of the world which are potential objects of explanation and prediction by general theory" and are typically unobservable (Woodward 1989, p. 393). Examples of the latter category include "weak neutral currents, the decay of the proton, and chunking and recency effects in human memory" (Bogen and Woodward 1988, p. 306). Theories, in Bogen and Woodward's view, are utilised to systematically explain and predict phenomena, not data (pp. 305-306). The relationship between theories and data is rather indirect. Data count as evidence for phenomena and the latter in turn count as evidence for theories. This view has been further elaborated in subsequent papers (Bogen and Woodward 1992, 2005 and Woodward 1989) and is becoming increasingly influential (e.g. Prajit K. Basu 2003, Stathis Psillos 2004 and Mauricio Suárez 2005).

In this paper I argue contrary to Bogen and Woodward that data serve as evidence for theories, not only for phenomena. Bogen and Woodward seem to forget the old Duhemian dictum that 'theories cannot be tested in isolation'. That is, they seem to forget that theories require the help of auxiliary hypotheses to make contact with data. When augmented with suitable auxiliaries, theories do entail, predict and potentially explain the data. I say 'potentially explain the data' because my focus in this paper is only on the inferential and predictive relations between theories, phenomena and data. To demonstrate my claim I examine four cases from physics, chemistry and astronomy: (i) a controversy between Lavoisier and Priestley, (ii) the calculation of lead's melting point, (iii) the prediction of the Poisson spot and (iv) the discovery of Neptune. The first of these is discussed in Basu (op. cit.) and the second in Bogen and Woodward (1988). The last two have not yet been discussed in the context of Bogen and Woodward's work but they are widely discussed in confirmation theory as paradigmatic examples of novel predictions. The choice of cases reflects my desire to assess Bogen and Woodward's view (1) under the best light by considering one of their principal examples as well as a meticulously discussed example from one of their devotees and (2) under the most stringent confirmation criteria by considering two exemplary cases of novel prediction.

2. The Lavoisier-Priestley Controversy

Basu (op. cit.) argues that for observations to be of use in theory testing, they first need to be transformed into evidence. Since the transformation, according to him, involves the introduction of theoretical vocabulary, the end-product is theory-laden. Basu motivates his claims using a distinction between raw (observational) data and evidence that is explicitly modelled on Bogen and Woodward's distinction.[1] Following Bogen and Woodward, he claims that theories do not entail, predict or explain observation statements or data, not even with the help of suitable theoretical auxiliaries. This prevents any direct observational

[1] Although Basu agrees with much of what Bogen and Woodward have to say, he thinks that their distinction "is inadequate in handling cases of 'revolutions' in science" (p. 354).

assessment of theories (plus auxiliaries).[2] To support his claims, Basu considers in detail a rather well-known controversy between Antoine Lavoisier and Joseph Priestley.

The controversy concerns two conflicting results emanating from what appear to be the same experiments independently carried out by the two scientists. Both scientists were in agreement that the observable result of the experiments was the production of a black powder with certain properties.[3] Since their respective theories of oxygen and of phlogiston do not speak of (or indeed entail) the presence of black powder, the observable result cannot immediately be used for theory adjudication. The raw observational data first has to be theoretically treated. This is where the disagreement arose. For Priestley, who advocated the phlogiston theory, when iron is heated in dephlogisticated air it leads to the production of iron calx. For Lavoisier, an advocate of the oxygen theory, the heating of iron in oxygen leads to the production of iron oxide. Yet, the presence of iron calx is only entailed by the phlogiston theory and the presence of iron oxide is only entailed by the oxygen theory. In other words, the same observation (i.e. the presence of a particular kind of black powder) is theoretically interpreted – out of necessity, for on its own, Basu claims, it is not evidentially potent – as two different evidential statements, each only confirming its respective theory.

Although Basu takes theoretical auxiliaries as necessary for the transformation of observations into evidence, he insists that they cannot help infer the relevant observation statements from the given theory. In the case at hand, this means that the presence of that particular kind of black powder cannot be inferred from either of the two theories. To see this point, let's formalise the aforementioned statements. Let O_1: Iron is heated in oxygen, O_2: Iron is heated in dephlogisticated air, E_1: Iron oxide is produced, E_2: Iron calx is produced, B: Black powder with certain observable properties is present, L: $O_1 \rightarrow E_1$, P: $O_2 \rightarrow E_2$, A_1: $B \rightarrow E_1$ and A_2: $B \rightarrow E_2$. L is a central theoretical claim in Lavoisier's theory and P the one in Priestley's theory. A_1 and A_2 are theoretical auxiliaries that respectively allow each scientist to go from observation to evidence.[4] Consider Lavoisier's theory. From O_1 and L, we can infer E_1 but not B. To confirm Lavoisier's theory we must assume A_1 which together with B entail E_1. Thus, to confirm Lavoisier's theory (or at least one of its parts, i.e. L), we must first transform B into an evidentially relevant statement (i.e. E_1) using theoretical auxiliary A_1. Notice that if we add A_1 to the set of statements $\{O_1, L\}$ we still cannot infer B. This seems to vindicate Basu's point that even with the help of theoretical auxiliaries we cannot infer the observational statement. In his own words, "…the construction of E_1 in (1) [i.e. the proposition that B and A_1 imply E_1] is asymmetrical. The fact that iron oxide is produced does not entail (along with [A_1]) that a black powder is produced" (p. 361). The same asymmetry afflicts Priestley's evidential inferences. Note that we cannot judge Priestley's theory on E_1 and Lavoisier's theory on E_2. Each evidential statement is at best irrelevant to the other theory, at worst it disconfirms it.

Basu does ponder at one point "whether it is possible to predict the (raw) data from the hypothesis by employing suitable auxiliary assumptions" (p. 362). He dismisses this

[2] Observations, Basu claims, need not be theory-laden but they cannot play a direct role in confirmation: "…although one could legitimately hold that there are observations that are not theory infected, such observations *cannot* be employed for theory resolution" (p. 356) [my emphasis].

[3] Priestley and Lavoisier agreed on various other observable results such as balance readings. They disagreed on whether the reaction only led to the production of black powder. Priestley thought that carbon dioxide was also produced. This disagreement is not important for our current discussion - Basu similarly sidelines it - as we are only interested in the inferential links between evidence and (commonly shared) observation statements.

[4] A_1 and A_2 have a more complicated structure that for the sake of simplicity I leave out. This should not affect the conclusion of my argument since both auxiliaries appeal to the same Stahlian hypotheses to determine the purity of samples.

possibility two pages later, roundly asserting that "(raw) data *never* have any evidential bearing" (p. 364) [my emphasis]. In what follows, I contest this assertion by finding the requisite auxiliary assumptions that let us derive, predict and potentially explain observational report B. I do so by presenting a general strategy for constructing suitable auxiliaries that has applicability to a broad range of cases. This, as we shall shortly see, requires a detour via set-theory. If my strategy is compelling it undermines not only Basu's particular project but more generally Bogen and Woodward's on which the former is firmly grounded in.

Sets can be partitioned into various disjoint parts. More formally we say that a set P is a partition of a set S if and only if (1) all of P's members are non-empty subsets of S, (2) the union of P's members is co-extensional to S and (3) the intersection of any two members of P is empty.[5] A peculiar aspect of this standard definition is that any set S (that can be partitioned) will have {S} among its partitions. For those interested in splitting the original set into two or more disjoint parts, a partition containing the original set as a member will of course be unwanted. To overcome this problem, let's define another notion that prohibits such partitions, call it 'partition*'. A set P is a partition* of a set S if and only if P fulfils the above three conditions (i.e. it is a partition of S) and P does not contain S as a member. Let's denote such a set as Part*(S). Sets with less than two members cannot be partitioned*. For a set S with *n* members, the number of partitions* is given by the bell number of that set minus one.

Predicates denote properties. Extensionally understood, properties are sets. That means that for any set there is one and only corresponding (natural or artificial) property, and vice-versa. This allows us to partition* properties by partitioning* their corresponding sets. Thus a partition* of a set S will have as members non-empty non-intersecting sets, each of which can be assigned a different property. Indeed, any property applicable to more than one object can be partitioned* into two or more properties each of which is distinct from one another and applicable to at least one object. Take the property of being a mammal. It can be partitioned* into a great number of properties, some of them corresponding to natural, others to artificial properties. Examples of (presumably) natural properties are the properties of primate, rodent, bat and dolphin. Examples of artificial properties are the properties of being a mammal half a meter long, being a mammal named 'Alexa' and weighing more than 500kg.[6]

To remove any lingering unclarity, let us take a closer look at an example of a set being partitioned*. Suppose S = {1, 2, 3}. We know that this set has four partitions*, i.e. $Part_1$*(S) = { {1}, {2}, {3} }, $Part_2$*(S) ={ {1, 2}, {3}}, $Part_3$*(S) ={ {1, 3}, {2}}, $Part_4$*(S) ={ {2, 3}, {1}}. Observe that each partition* contains as members sets that are mutually disjoint and whose union is set S. Qua sets, each member of a partition* of S can be assigned a property. Take for example $Part^*_1(S)$. It contains three members, namely sets {1}, {2}, {3}. Each of these can be assigned a different property; we can use the predicates R_1, R_2 and R_3 to denote these properties. Now if R is the predicate denoting the property corresponding to set S, then $(x)\ (Rx \equiv (R_1x \oplus R_2x \oplus R_3x))$ where $\oplus$ stands for exclusive disjunction. All the partitions* of S can be given the same treatment. What is more, since partitioning* decomposes properties into mutually exclusive and exhaustive parts, inclusive disjunction formulations of such biconditionals – in the case at hand $(x)\ (Rx \equiv (R_1x \vee R_2x \vee R_3x))$ – are logically equivalent to their exclusive disjunction counterparts.

[5] An alternative first condition does not exclude non-empty subsets of S, thereby allowing for partitions such as {S, ∅}.

[6] Overlapping properties such as being a mammal half a meter long and being a mammal named 'Alexa' do not of course belong to the same partitions* of the property mammals.

With these tools and results in mind, let us turn to the problem at hand. Given our move to predicate logic, atomic propositions O_1, O_2, E_1, E_2 and B are now taken to be predicates while complex propositions L, P, A_1 and A_2 are now quantified propositions. For example, theoretical auxiliary A_1 now reads: $(x)\ (Bx \rightarrow E_1x)$. Crucially, this universal generalisation implies that either E_1 is co-extensional to B or B is a non-empty proper subset of E_1.[7] In the former case, this amounts to the bi-conditional statement A_3: $(x)\ (Bx \equiv E_1x)$. If we add A_3 as an auxiliary to our original set of propositions $\{O_1a, L: (x)\ (O_1x \rightarrow E_1x)\}$ we can derive the desired sentence B*a*, where *a* denotes the particular object that bears these properties. In the latter case, we can turn to the concept of partition* to derive an equally suitable statement. We know that B, qua a non-empty proper subset of E_1, belongs to at least one partition* of E_1.[8] Take such a partition*, let's call it 'C'. C is co-extensional to E_1. It contains B as a member but also one or more other sets that are disjoint from B. We can assign a property and hence a predicate to each of them. Let us call these 'C_1', … , 'C_m', where *m* is determined by the number of disjoint sets in C other than B. The following auxiliary can now be formulated A_4: $(x)\ (E_1x \equiv (Bx \oplus C_1x \oplus \ldots \oplus C_mx))$. The properties on the right side of the biconditional are jointly co-extensional to the property on the left side. If we add A_4 to our original set of propositions we can derive the following statement $Ba \oplus C_1a \ldots \oplus C_ma$.[9] Since B*a* is one of the exclusive disjuncts, the observation that *a* has property B can confirm the theory and auxiliaries used in the derivation.[10] Contra Basu, B*a* need not first be transformed into theory-laden evidence.

Technicalities aside, the conclusion is supported by a very simple logical point. Suppose we are faced with the sort of asymmetry Basu talks about, i.e. we have a statement of the form 'All F's are G's' but we really want a statement of the form 'All G's are F's' or at least some statement that allows us to go from G's to F's. If we know that all objects with property F have property G, we can infer that either some objects with property G have property F or all of them do. The latter case plays straight into our hands. The former needs a little spelling out. That's where the partition* notion comes in, as it facilitates the spelling out by letting us decompose properties like G into F and non-F parts. Doing so allows us to conclude that an object with property G will also possess a property from a finite selection of mutually disjoint properties (partitioned* from G) that includes F. Thus finding an object with property F can confirm a theory which predicts the existence of objects with property G. To put things in perspective, suppose 'G' is an unobservable property and 'F' an observable one. Theories supplemented with the auxiliary 'All F's are G's' can be confirmed by observational reports of objects possessing property F.

In a sense what I have argued for is unsurprising. An auxiliary of the form 'evidence or phenomenon x implies observation y' or something weaker like 'evidence or phenomenon x implies (or raises the probability of) an exclusive disjunction one of whose disjuncts is an observation y' is implicit in the scientists' thoughts when they employ an inverse conditional,

[7] For simplicity, I use the same letters to denote predicates and their corresponding properties and sets. Context will determine which one I have in mind.

[8] Although some partitions* of E_1 might not have B as a member, their members' union will contain all the objects that are contained in B. From these we can reconstruct B, e.g. by further partitioning* the members of a given partition* and then taking the relevant union of the resulting partitions*. That means that the partition* choice does not really matter for the purposes of inferring something about B from E_1. Choosing a partition* that includes B as a member just makes the point easier to communicate.

[9] The complex proposition $Ba \oplus C_1a \ldots \oplus C_ma$ need not be thoroughly observational, but at least one of its atomic components, i.e. B*a*, will be.

[10] I say 'can confirm' instead of 'confirms' to avoid a controversial issue in confirmation theory, i.e. whether or not derived observational statements *always* have confirmational power. The received view has been that they do always have such power but Larry Laudan and Jarrett Leplin (1991), amongst others, have challenged this view.

i.e. when they infer from their observations some evidential report. Indeed, on pain of inconsistency, the scientists must have a biconditional or even an identity relation in mind. They take it that one of the manifestations of iron oxide (or iron calx) is black powder, hence they are in effect accepting a statement like 'An object is iron oxide (or iron calx) iff/= it is black powder with certain observable reactions to other substances xor it is a red-brownish solid with certain observable reactions to other substances xor …'. The availability of such auxiliaries and the inferential relations they engender undermines Bogen and Woodward's view that theories do not entail, predict or even potentially explain observation statements.

Theories can it seems make direct contact with observation reports. It should be obvious that by 'direct contact' I do not mean anything that violates Duhem's thesis that theories can never be tested in isolation. Rather, I mean that theories plus suitable theoretical auxiliaries can entail, predict and potentially explain observation statements or data. In short, the view developed in this section is perfectly compatible with various forms of holism.[11]

It is worth noting that auxiliaries A_3 and A_4 are not merely stipulated but derived from the existing auxiliary A_1. We can similarly derive auxiliaries A_5, $(x)\ (Bx \equiv E_2x)$, and A_6, $(x)\ (E_2x \equiv (Bx \oplus D_1x \oplus \ldots \oplus D_kx))$, from A_2 to allow Priestley's theory to be tested by observations. Indeed, with the help of A_5 and A_6, Priestley's theory can be confirmed by Ba. Since Ba can confirm both theories it cannot be used to discriminate between them. This problem is of no concern to us here since we are frying an altogether different fish. The aim was to show that theories plus suitable auxiliaries can be tested by observations, i.e. it was not to show that the presence of black powder discriminates between Lavoisier's and Priestley's theories. At any rate, in terms of theory testing we are not worse off than when we started since E_1 and E_2 are also unable to discriminate between the two theories. Moreover, the fact that one observation report cannot adjudicate between two theories (plus associated auxiliaries) does not entail that (1) it cannot adjudicate between those theories and others and (2) all observation reports are similarly impotent.

Alas, things are even more complicated than I have let on so far. Auxiliaries A_1 and A_2 seem to have been ad-hoc stipulations since no independent reasons were given to support the claims that one of iron oxide's or iron calx's manifestations is black powder with certain observable properties. It is always preferable to have independent confirmation for a hypothesis prior to its utilisation but it is not absolutely necessary. Nowadays we can independently confirm Lavoisier's auxiliary since we have distinct methods of analysing the chemical structure of the black powder residue. If no independent confirmation existed, the relevant auxiliary would be confirmationally impotent. Put differently, either the original auxiliaries that go from data to phenomena enjoy independent support and then so do the derived auxiliaries that go from phenomena to data or the original auxiliaries are ad-hoc postulations that play no genuine confirmational role but then they are of no interest to any party in the debate.

3. Calculating the Melting Point of Lead

We turn now to one of Bogen and Woodward's most prominent examples. The sentence 'Lead melts at 327.5 °C' can presumably be explained, derived and predicted from theories of molecular structure. In Bogen and Woodward's view the sentence is not an observation report but rather a report about the phenomenon of the melting point of lead. The relevant observations or data come in the form of scatter points of temperature readings generated by a series of measurements. Provided various experimental conditions hold, e.g. that there is no

[11] In my view, some form of partial holism is highly plausible.

systematic error, that small uncontrolled causes of variation "operate independently, are roughly equal in magnitude, are as likely to be positive as negative, and have a cumulative effect which is additive" the mean of the data can be considered to be a good estimate of lead's true melting point (1988, p. 308). The data thus serve as evidence for the phenomenon but they cannot be explained by, derived or predicted from the relevant theories of molecular structure because the mean of a given distribution "does not represent a property of any particular data point" and "it will not, unless we are lucky, coincide exactly with that value [i.e. the true value of the melting point]" (1988, pp. 308-9). On the basis of these two reasons, Bogen and Woodward conclude that the data in this and similar cases cannot serve as evidence for the corresponding theories.

Let us consider more closely the two reasons Bogen and Woodward cite to prop up their conclusion. As I understand it the first holds that we cannot explain, derive or predict a datum from a mean because the latter represents a property of a set of data but not of any one of its members. The second reason holds that we cannot explain, derive or predict a given mean from the theoretically predicted value of the melting point – which in the example above Bogen and Woodward suppose to also be its true value – since the mean and theoretically predicted values need not be equivalent. Although the two reasons are strictly speaking correct, closely analogous derivations can be pulled off with the help of suitable auxiliaries. To wit, we can derive exclusive disjunctions whose disjuncts include the desired mean and datum.

Take the second claim first. Suppose we want to derive a particular mean m_1, which we assume for the sake of simplicity to satisfy the aforementioned experimental conditions, from a particular theoretically predicted value p_1. We know that every mean m with some standard error ε corresponds to a different range r of theoretically predicted values of the melting point of lead such that $r = \{x: m-\varepsilon \leq x \leq m+\varepsilon\}$. Now take only those pairs of m and ε that fulfil the aforementioned experimental conditions, i.e. the pairs that are typically good estimates of lead's true melting point. Let us call these 'the selected pairs' and their corresponding ranges 'the selected ranges'. Since Bogen and Woodward assume that the theories of molecular structure determine lead's true melting point – this is not an essential assumption but it simplifies the derivation – we can infer that the selected pairs are typically good estimates of the theoretically predicted value of lead's melting point p_1. This means that the majority of selected ranges contain p_1 as one of their members. Let us denote that set of selected ranges by R and the corresponding set of selected pairs by M. We can obviously derive M from p_1. Provided m_1 and standard error ε_1 are good estimates of lead's true melting point, as we have assumed above, $(m_1, \varepsilon_1) \in M$. To express this in a more familiar format, the pair (m_1, ε_1) will be one of several disjuncts in an exclusive disjunction that, contrary to Bogen and Woodward, we can derive from the theories of molecular structure plus the foregoing auxiliaries.

The first claim can be handled similarly. Suppose we want to derive a datum d_1 from a mean m_1 which is determined by a particular data set one of whose members is d_1. Like before suppose for simplicity's sake that m_1 satisfies the stated experimental conditions. We know that every mean m with some standard error ε corresponds to a unique range q of data sets of temperature readings of lead's melting point. Obviously different data sets can have the same mean. That's why the relevant auxiliary assigns to each mean a range of data sets, i.e. a set of data sets. Take those pairs of m and ε in M. Each of these has a corresponding range of data sets. Let us denote the set of all such ranges by D. We can obviously derive D from p_1 and the other auxiliaries. We know already that the pair (m_1, ε_1) has a corresponding range of data sets, at least one of which contains d_1. Since $(m_1, \varepsilon_1) \in M$ we can infer that d_1 is contained in at least one of the data sets contained in D. In other words, d_1 will be one of several disjuncts in an exclusive disjunction that, against Bogen and Woodward's view, can be derived from the theories of molecular structure plus some suitable auxiliaries.

4. Novel Predictions

A significant gap exists in the writings of Bogen and Woodward. Nowhere do they systematically and explicitly discuss the role of novel predictions, considered by many as the Holy Grail in confirmation, in the relationship between data, phenomena and theories.[12] In this section, I will argue that novel predictions are particularly damaging to Bogen and Woodward's claim that data cannot serve as evidence for theories. To make this point I will look into two paradigmatic cases of novel prediction.

The notion of novel prediction can be understood in a handful of competing ways. These can roughly be classified under two broad categories: temporal and use. *Temporal novelty* requires that what is predicted be in some sense unknown prior to a theory's prediction of it.[13] The sense of unknown depends on the particular temporal restrictions advocated. For instance, one may require that what is predicted must not be widely known or that it must be unknown to the theoretician who makes the prediction. Examples that satisfy both stringent and liberal criteria of temporal novelty include the two cases that I will shortly be examining, namely the prediction of the Poisson spot and the prediction of the existence and properties of Neptune. These two cases can also be accounted for by the notion of *use novelty* which requires that what is predicted is not in some sense used in the construction of the theory that makes the prediction.[14] As before, the sense of used depends on the particular restrictions advocated. For instance, some require that what is predicted must not be the explanatory target of the individual who designed the theory, while others that it must merely not be used to fix the value of one or more of the theory's parameters.[15] An example that perhaps satisfies both stringent and liberal criteria of use novelty is Newton's prediction of the rate of precession of the equinoxes. This example does not qualify under any temporal novelty account since the rate of precession of the equinoxes was not only widely known to scientists at the time but also known to Newton himself.

Some scholars have questioned the idea that theories can be confirmed at all. Of those who accept that theories can be confirmed, however, none denies that at least some of the examples cited as cases of novel prediction have sharp confirmational power.[16] The Poisson spot and Neptune cases were chosen precisely because they are generally acknowledged to have acute confirmational power. As I already alluded, both cases satisfy stringent and liberal criteria of temporal and use novelty. For this reason, they present a first-rate test of Bogen and Woodward's view in the arena of novel predictions.

Let us first consider the Poisson spot case. In 1819 Augustin Fresnel entered his wave theory of light in the French Academy of Science competition on the diffraction of light. The panellists consisted mostly of supporters of the particle theory of light, which was dominant at the time. One such panellist, Siméon-Denis Poisson, attempted to disprove Fresnel's theory by deriving from it what he and others considered to be an absurd consequence. If Fresnel's theory was right, a bright spot should appear in the middle of a disk's circular shadow when

[12] Woodward (1989) makes some cursory remarks about novel predictions.

[13] Pierre Duhem ([1914]1991, p. 28) can be interpreted as being an advocate of temporal novelty.

[14] Deborah G. Mayo states the relationship between the two notions clearly when she says "most scientific cases are equally accommodated by (and hence fail to discriminate between) temporal and use-novelty, unsurprising since temporal novelty is sufficient, though not necessary, for use-novelty" (1991, p. 525).

[15] The first suggestion can be found in Elie Zahar (1973) while the second in John Worrall (2002).

[16] Mayo (op. cit.), for example, criticises the notion of novel prediction but does not deny that many of the cases that qualify as novel predictions have sharp confirmational power.

illuminated by a narrow beam of light. François Arago, one of the other panellists, performed the experiment and to everyone's disbelief observed the bright spot. As a result Fresnel's wave theory received a hard-earned confirmational boost. To make sense of this confirmational boost it is necessary that theories and data are more proximal than what Bogen and Woodward would have us believe. After all, without some guidance from suitable auxiliaries Poisson and Arago would not have known what to look for in order to judge whether Fresnel's theory was right. This guidance came in the form of an auxiliary hypothesis that connects the theoretical prediction of constructive interference in the centre of the disk's circular shadow to the observation of a bright spot. In other words, it was acceptable to both parties in the debate that constructive interference implies brighter regions. Without this assumption, which incidentally still stands today, Poisson would not have been able to predict the bright spot that he thought would undo Fresnel's theory.

The same point can be raised in the context of the discovery of Neptune. Urbain Jean Joseph Le Verrier and John Couch Adams worked independently on explaining Uranus' irregular orbit. Both men hypothesised the existence of a planet with enough mass to gravitationally perturb Uranus's orbit and employed Newtonian calculations to identify its properties and whereabouts. Le Verrier sent his predictions to J.G. Galle at the Berlin Observatory, who detected the planet on September 23 1846 at approximately the exact location forecasted by Le Verrier – the predicted true longitude was at 326º0´ whereas the observed one was at 326º57´ (see C. J. Brookes 1970). Soon after the discovery, but not before some wrangling, the planet was named 'Neptune'. Once again to make sense of the prediction it is necessary that theories and data enjoy a close relationship. Without some guidance from suitable auxiliaries Le Verrier, Adams and Galle would not have known what to look for in the telescopic observations that led to Neptune's discovery. The requisite auxiliary connects the theoretical prediction of a massive object with a specific orbit to the telescopic observation of a bright dot that appears in a particular part of the sky at a particular time at night. Without this assumption, which also stands today, Galle and others would not have been able to detect the planet via telescopic observations.

It ought to be painfully obvious that almost without exception suitable auxiliaries connecting theories, phenomena and data need to be at hand in the cases of novel prediction. Such auxiliaries play the crucial role of informing scientists about the observable manifestations of physical phenomena. To put the point about novel predictions in the realist's vocabulary: The view that suitable auxiliaries are required in the case of novel predictions is the only (or at least the best) view that does not make our knowledge of what observations to make in order to confirm or disconfirm a theory a miracle. Even well entrenched theories can be undone when the right data comes along. Within a few years of Poisson's prediction and Arago's observation the wave theory became the dominant theory of light.

5. Conclusion

It has not been argued here that theories can always make contact with the observational ground. Instead, it has been argued that in those cases where the phenomena are inferred from the theories and the data do indeed serve as evidence for the phenomena, the data also serve as evidence for the theories. Four cases were analysed in support of this claim. In each of these cases suitable auxiliaries were available to effect the derivation and prediction of data from the theories. This lends more credence to the view that observations and theories enjoy much more direct contact than Bogen and Woodward are willing to admit.

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