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From data to phenomena: a Kantian stance

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Abstract This paper investigates some metaphysical and epistemological assumptions behind Bogen and Woodward's data-to-phenomena inferences. I raise a series of points and suggest an alternative possible Kantian stance about data-to-phenomena inferences. I clarify the nature of the suggested Kantian stance by contrasting it with McAllister's view about phenomena as patterns in data sets.

Keywords Data · Phenomena · Bogen · Woodward · McAllister · Kant

1 Introduction

Bogen and Woodward (1988) distinction between data and phenomena marks an important turning point in recent philosophy of science. From a historical point of view, their notion of phenomena as stable and repeatable features emerging out of different data marks the beginning of a new trend in which phenomena are regarded as robust entities that scientific theories explain and predict, against a 'thinner' notion of phenomena typical of the empiricist tradition. As such, they have provided a foil to re-assess van Fraassen's constructive empiricism. It is this particular aspect of Bogen and Woodward's position that originally hooked me and that I explored in connection with a criticism of van Fraassen's constructive empiricism in Massimi (2007).

In this paper, I do not want to discuss the merits of the data—phenomena distinction with respect to van Fraassen's view, nor do I want to present lengthy case studies taken from the history of physics. My starting point is instead a puzzle: how should we intend the data—phenomena distinction? Is it just descriptive of scientific practice?

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Or does it have a normative status? If descriptive, we risk loosing ourselves into a plurality of case studies, each of which may be right in its own terms, but it would no longer be clear what the exact role of the distinction is. If, on the other hand, we intend it as having a normative status, then we need to understand how exactly data-to-phenomena inferences *ought to* work. Although there may well not be a unique way in which data-to-phenomena inferences ought to work, I think the distinction *does* have a normative status, otherwise it could not bear on the epistemological implications against van Fraassen's constructive empiricism, for instance.

In this paper, I want to endorse an approach to the data—phenomena distinction which is *normative and naturalised* at the same time, following the Kantian tradition of epistemological naturalism. This is the tradition that claims that answers to the problem of knowledge (i.e., of how we know what we know) should be found by drawing on natural sciences. Or better, they should be found by taking the natural sciences as paradigmatic of scientific knowledge and by investigating the conditions under which we gain knowledge of nature.

My focus is on data-to-phenomena inference, as Woodward (1989, 1998) has characterised it in terms of *statistical reliability* and *manipulationist causation*. I introduce a Kantian stance on phenomena and compare it with James McAllister's (1997, 2004, 2007) alternative account of phenomena as patterns in data sets. I agree with Bogen and Woodward's bottom-up characterization of data-to-phenomena inference, but I draw a conclusion that Bogen and Woodward would resist: namely, that phenomena are (partially) constituted by us, rather than being ready-made in nature. On the other hand, I grant with McAllister that phenomena have features which are—in some relevant sense to be clarified here below—mind-dependent, but I resist McAllister's characterization of phenomena as patterns in data sets. Thus, a Kantian stance situates itself in between the externalist account of Bogen and Woodward, and McAllister's internalist one.

In Sect. 2, I raise three points to show that statistical reliability and manipulationist causation may not necessarily be sufficient to individuate phenomena unequivocally. In Sect. 3, I present a Kantian stance on phenomena intended as 'conceptualised appearances', and I discuss some main aspects of it by highlighting the points of convergence and divergence with respect to McAllister's view.

2 Metaphysics and epistemology in Bogen and Woodward's notion of phenomena

In this section I want to highlight some of the metaphysical and epistemological assumptions behind the distinction between data and phenomena.

From a metaphysical point of view, Bogen and Woodward share realist intuitions about phenomena: Phenomena exist 'out there' in nature, as stable and repeatable features emerging across a variety of experimental contexts and data. As Woodward nicely puts it: "Detecting a phenomenon is like looking for a needle in a haystack or (…) like fiddling with a malfunctioning radio until one's favourite station finally comes through clearly" (Woodward 1989, p. 438).

Despite the variety and idiosyncratic nature of the *causal factors* involved in data production, Woodward stresses the importance of controlling and screening off various causal influences that may undermine the reliability of phenomena detection. The usual procedure consists in (1) control of potential confounding factors; (2) elimination of background noise; (3) procedure for statistical analysis and data reduction. Woodward's analysis goes in a Cartwright–Hacking direction in claiming that (1)–(3) are relatively theory-free, and fall into the province of experimentalists' expertise, rather than of theoreticians'. Not surprisingly, most of the examples from the history of high-energy physics draw on the works of historians and sociologists such as Galison (1985); Franklin (1986); Collins (1981) and Pickering (1984).

Thus, the metaphysical framework is close to that of experimental realism, whereby (i) phenomena such as weak neutral currents *exist* in the world 'out there'; (ii) they manifest themselves by *causally producing* data such as bubble chamber photographs, which we then (iii) learn how to recognise from other data due to background noise via *reliable* procedures of data analysis and data reduction.

Strictly coupled with this experimental realist metaphysics is the epistemology of reliabilism, championed in recent times by Alvin Goldman (1986) among others. The key idea is that we are justified to believe in a phenomenon *p* if the process that confers justification is reliable, i.e. generates true beliefs with high frequency. In other words, we are justified to believe in a phenomenon *p* if the process of data production and data analysis is *reliable*, i.e. generates true beliefs about *p* with high frequency. Woodward (1998) has addressed the issue of reliability by explicitly drawing links with both Goldman's epistemology and with Deborah Mayo's (1996) error-statistical approach. The central idea is to identify patterns of counterfactual dependence between data and phenomena that take the following form (Woodward 1998, S166): "(1) if Dj is produced, conclude that Pi is true. Then the ideal at which one aims is (2) the overall detection or measurement procedure should be such that each of the conditionals of form (1) recommends that one accept Pi when and only when Pi is correct. Somewhat more succinctly: the detection and measurement procedure should be such that different sorts of data D1…Dm are produced in such a way that investigators can use such data to reliably track exactly which of the competing claims P1…Pn is true".

A distinctive feature of reliabilism is that it licenses theory-free data-to-phenomena inferences: "in order for data to be reliable evidence for the existence of some phenomenon (…), it is neither necessary nor sufficient that one possesses a detailed explanation of the data in terms of the causal process leading to it from the phenomenon" Woodward (1989, pp. 403–404):

- It is not sufficient because even if a phenomenon plays a causal role in the production of data, it may well be impossible to extract reliable information from the data about the phenomenon of interest. For example, although W and Z bosons are produced in bubble chambers, in 1960s and 70s no one used bubble chambers to detect W and Z because it meant to locate a very rare event among millions of pictures, "an impractical task" until Carlo Rubbia did it in the 1980s.
- It is not necessary either because data can provide reliable evidence for some phenomena even if one is ignorant of or mistaken about the character of the causal processes leading from the phenomena to the data. For instance, Donald Glaser,

> who invented the bubble chamber, was himself quite mistaken about the actual causal mechanism at work in bubble chambers (which he misinterpreted as electrostatic repulsion).

Thus, there are two main interrelated assumptions behind Bogen and Woodward's notion of phenomena:

I. *Metaphysics*: Phenomena are ready-made in nature and they manifest themselves by causally producing data.
II. *Epistemology*: Reliability is the epistemic criterion for knowing phenomena from data.

2.1 Three points about Bogen and Woodward's notion of phenomena

In this section, I want to raise three points concerning both the metaphysical and epistemological assumptions above.

2.1.1 On the metaphysics of ready-made phenomena: underdetermination of phenomena by data

The problem with the metaphysics of ready-made phenomena is that it is not always clear what signal we should be looking for in a sea of noise: in fact, there could well be more than one relevant signal compatible with the same data. To put it in a different way, the data from which we have to infer the phenomena can be *causally produced* by a variety of different phenomena under suitable experimental conditions, so that the choice of which phenomena the data provide evidence for may well be underdetermined by the data. I have explored this *entity*-version of the classical underdetermination problem in relation to Hacking's experimental realism (1983) in Massimi (2004), where I concluded that "whenever we have prima facie rival potential causes for the same phenomena, in order to distinguish between them and to determine which entity-with-causal-power has actually produced the observed effect, we must in the end rely on a description of what causal powers/ capacities /dispositions an entity *is* to have so as to produce the observed effects. This description is given by a scientific theory" (ibid., pp. 42–43). Let me clarify this point. This is not a re-statement of the theory-ladeness of observation. I am making a more radical claim. I claim that there can be more than one way in which we can carve data production, analysis, and reduction, given the various contextual factors in the sea of noise. And what sort of phenomena we infer depends on the way we have carved and 'massaged' those data. Of course, there are constraints, and in no way do I want to suggest that anything goes, as it will become clear when I present my Kantian stance.

But to go back to this first point, the same data may be compatible with more than one phenomenon, and simply manipulating / controlling data at the experimental level may not be sufficient to discriminate what entity-with-causal-powers has produced them. Woodward (1998, S167) acknowledges the problem:

> Consider a case in which the presence of neutral currents is in fact sufficient for (…) or has in fact caused certain bubble chamber photographs D1. If some other

> competing phenomena claim P2—e.g. that background neutrons are present—is also sufficient for D1 or if background neutrons would also cause D1 (…), then the overall detection procedure is very likely such that one would conclude P1 even if P2 is correct. In this case the pattern of counterfactual dependence described above will not be satisfied and D1 will not be good evidence for P1. The pattern of counterfactual dependence just described is an ideal, which for a variety of reasons is rarely fully satisfied in practice.

The conclusion he draws is that data-to-phenomena inferences are probabilistic rather than deterministic. Thus, one possible response to the problem of underdetermination of phenomena by data consists in offering a probabilistic version of reliability. In this way, the burden of supporting the metaphysics of ready-made phenomena is shifted to the epistemology of reliabilism, to which I now turn.

2.1.2 On reliability as an epistemic criterion independent of our knowledge of the causal mechanism leading from the phenomenon to the data

The main novelty of Bogen and Woodward's notion of phenomena is their use of reliabilism to back up their experimental realist metaphysics. In epistemology, reliabilism has been championed in recent times by Alvin Goldman and Fred Dretske (1981), among others. The distinctive feature of reliabilism is that it provides justification for beliefs in a way that is independent of our knowledge of the causal mechanism behind data production, as we saw above. So it is this claim that we have to assess here.

Consider the following counterexample. An experimenter may come to believe in a phenomenon *p* by a reliable process, i.e. a process that generates true beliefs about *p* from data with high frequency, although the experimenter's undergoing this process is caused in an unreliable way (for instance, the experimenter may have learnt a reliable data reduction process from an unreliable colleague), so that although the belief-forming process is reliable we would not count it as justified belief.

Goldman responds to this type of counterexample by introducing second-order processes (1986, p. 115): "A second-order process might be considered metareliable if, among the methods (or first-order processes) it outputs, the ratio of those that are reliable meets some specified level, presumably greater than .50". The idea is that over time the use of second-order processes improves calibration by managing to discard poorly calibrated processes.

However, there is a problem with this strategy of delegating the judgment of reliability for first-order processes to second-order processes (which is captured by the experimentalist notion of calibration—see Franklin 1997).[1] And it is a problem that affects more in general reliabilism as an externalist epistemology. The problem is that it engenders a bootstrapping mechanism, as Jonathan Vogel (2000) has argued against Goldman; namely, the use of reliabilism itself to sanction its own legitimacy.

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[1] Woodward (1998, p. S171) refers to Franklin's analysis of calibration within the context of empirical assessment of reliability: "One important example (…) is the strategy that Allan Franklin (1997) calls calibration. Here one assesses the error characteristics of a method by investigating its ability to detect or measure known phenomena and then assumes that the method will have similar error characteristics when used to investigate some novel phenomenon".

Suppose someone believes whatever a spectrometer says about the spectrum of a certain chemical substance, without having justification for believing that the spectrometer is reliable.[2] Suppose that the spectrometer happens to function very well. Then, an experimenter looks at it and forms the belief "In this case, the spectrometer reads 'X' for substance a, and X", where X is the proposition that substance a has infrared spectrum. Since the experimenter's perceptual process of reading the spectrometer is presumably reliable (she does not suffer from hallucinations, etc.), given the assumption that the spectrometer is functioning alright, we can say that the experimenter is justified to believe that the spectrum of substance a is infrared. Therefore, the experimenter can deduce that "On this occasion, the spectrometer is reading accurately". Suppose she iterates this procedure many times, without ever checking whether the spectrometer is reliable because it is properly wired, etc. By induction, the experimentalist infers that "The spectrometer is in general reliable", and hence goes on to use it in other cases to measure the spectrum of unknown substances. In this way, the experimentalist would fall prey of bootstrapping circularity.

Goldman (2008, p. 17) replies to Vogel's bootstrapping objection by saying that "the problem is not unique to reliabilism, but it is shared by many epistemologies. (…) Thus, if a theory like reliabilism—or any form of externalism—makes easy knowledge possible, this is not a terrible thing. Skepticism is a very unwelcome alternative".

If scepticism is certainly an unwelcome alternative, it is not however the only one. The counterexample shows, in my opinion, how despite the attractiveness, reliabilism is not itself exempt from difficulties. One of these difficulties resides in the attempt to secure knowledge by detaching reliability from the *causal knowledge* of the mechanism that generates true beliefs from data with high frequency. Woodward (1998, S176) stresses again this point: "The idea that one can often empirically establish that (4) a detection process is reliable without (5) deriving its reliability from some general theory of how that process works and/or why it is reliable is supported by a number of episodes in the history of science. (…) Galileo advanced a number of empirical arguments showing that his telescope was a reliable instrument in various astronomical applications even though he lacked a correct optical theory that could be used to explain how that instrument worked or why it was reliable".

I am not entirely convinced by this argument. I think that reliability cannot be entirely detached from the *causal knowledge* of the mechanism that generates true beliefs with high frequency. Take Galileo's telescope and the controversy it sparked about the actual size of fixed stars. True, Galileo may not have had a full-blown or the right optical theory to explain how the telescope worked or why it was reliable, but he did have a *causal explanation* connecting the size of the stars (based on his Copernican belief), to the function of the lens of the telescope in making the objects appear more similar to the way they are in nature. His opponents endorsed the opposite causal explanation about the actual size of the stars (which they believed were bright spots on a celestial sphere) and the function of the lens, which—they thought—distorted the actual size of the stars. Galileo famously resorted to some non-telescopic observations to avoid possible objections against the reliability of his telescope (see Frankel 1978).

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[2] The following example is patterned upon Vogel's (2000, pp. 612–615) reformulation of Michael Williams' "gas-gauge case".

Nonetheless, several people from Christopher Clavius to Lodovico delle Colombe and Cesare Cremonini objected to the use of the telescope and its reliability. One of the main objections was that the telescope was unreliable because it did not seem to magnify the stars, by contrast with other objects: the size of the stars observed with naked eye at night and with the telescope was approximately the same. At stake in this debate was the issue of whether or not the halos of the stars visible with naked eye should be taken or not into account in the estimate of the actual size of the stars: Aristotelians such as Horatio Grassi thought that it should, while Galileo thought that it should not because it was illusory. The debate sparked when Grassi published (under the pseudonym of Lothario Sarsi) this objection to the reliability of the telescope in his 1619 *Libra astronomica*, which Galileo rejected in *Il Saggiatore*.

The controversy that raged during Galileo's time around the use of the telescope and how to interpret the data, testifies in my view to the central importance that *causal explanation* plays in assessing reliability claims. The final verdict went to Galileo because the scientific community eventually embraced Galileo's causal explanation of how the telescope worked and why it was reliable.

Another example could be cathode rays. Originally discovered by Julius Plücker in 1859, they became a standard tool for different generations of scientists that used them in conjunction with different causal explanations of the fluorescence observed. The current causal explanation is that cathode rays are streams of electrons projected from the cathode by electrical repulsion. But this was not the causal explanation endorsed by nineteenth century physicists. Indeed, their different causal explanations of the mechanism led to very different data-to-phenomena inferences: from Arthur Crookes's 'particles' (ranging from molecules to an unknown state of matter); to J.J. Thomson's 'corpuscles' carriers of electricity; to Joseph Larmor's 'electrons' as permanent structural features of the ether. No wonder this historical episode has attracted much attention among historians and philosophers of science that are still trying to resolve the historiographic dispute about who really discovered the electron (see Achinstein 2001, and Arabatzis 2006). We see here another example of how reliability claims cannot be completely disentangled from discussions about the causal mechanism that generates data.

2.1.3 On whether the epistemology of reliabilism can support the metaphysics of ready-made phenomena

There is a third point I want to make. One may wonder to what extent reliabilism supports the metaphysics of ready-made phenomenena. Or better, whether that metaphysics finds back-up in the epistemology of reliabilism. For instance, how do we know that neutral currents are 'out there' in nature? In Sect. 2.1.1 we discussed the problem of underdetermination of phenomena by data. In Gargamelle bubble-chamber, the greatest challenge was to identify whether the observed data were the result of weak neutral currents (which scientists were looking for) or rather the result of neutron background which can produce exactly the same data. We concluded Sect. 2.1.1 by mentioning Woodward's probabilistic version of reliabilism to support the metaphysics of ready-made phenomena. Can the probabilistic version of reliabilism support the metaphysics of ready-made phenomena?

I suspect that the answer to this question is negative for the following reason. Until we have specified the class of contexts within which processes of data production, analysis and reduction are to operate, it is hard to see how these processes can generate true beliefs about phenomena with high frequency. The problem with reliabilism—even in the probabilistic version—is that we need to specify in advance what our epistemic goals are and need to give an exact account for appraising reliability ('reliable with respect to what?'). Unless we somehow know already how the phenomena that we are searching for should look like, how can we appraise whether data production and data reduction provide *reliable* evidence for them? In a way, this problem is a re-elaboration of what Harry Collins (1985/1992) has described as the *experimenter's regress*: in order to prove that an experimental process is reliable, we have to show that it identifies the phenomenon correctly. But in order to identify the phenomenon correctly, one has to rely on the experimental process whose reliability is precisely at stake. So reliability seems to fall back into a justificatory circle.

My claim is that unless we have a causal story, which normally comes from a scientific theory that helps us discriminate genuine phenomena from confounding factors, and enter in the way experiments are conceived, designed, and thought out, it may be very hard (and sometimes practically impossible) to discern phenomena on purely experimental grounds. To paraphrase an old slogan about causes, 'No phenomena in, no phenomena out'. In sum, reliabilism presupposes the same metaphysics of ready-made phenomena that is meant to back up.

One may respond at this point that the metaphysics of ready-made phenomena is ultimately supported by some sort of Kripkean essentialism, according to which there are natural kinds and they manifest themselves in a reliable way through a variety of data and experimental procedures, even if we either do not know or simply cannot practically identify the causal mechanism that goes from natural kinds to data production. This is not an option that Bogen and Woodward expressly discuss, as far as I am aware of, but it is a fairly standard move for realists to make when pressed on metaphysical issues.

But not even this move would solve the circularity between the metaphysics of ready-made phenomena and reliabilism. Indeed the circularity problem would crop up again this time in the bottom-up inductive nature of the data-to-phenomena inference. We may indeed ask how many 'positive instances' provide increasing evidential support for data-claims which would qualify as *reliable evidence* for a certain phenomenon. How can we guarantee that the next few instances are not going to be negative and hence are not going to have a knock-down effect on the data-claim we are trying to build for *reliably* inferring a certain phenomenon? Of course, this old problem of induction—applied to bottom-up data-to-phenomena inferences—has direct implications for how we look at the history of science and scientific revolutions. When did (inductively supported) data-claims about combustion stop being considered as reliable evidence for phlogiston, and begun to be regarded as reliable evidence for oxygen? As my colleague Hasok Chang (2008) is currently reconstructing in his work on the Chemical Revolution, answering this question (and similar ones about caloric and ether) is far from trivial. And it seems that unless we know already that phlogiston is not a natural kind, while oxygen is, reliability *per se* does not cut any ice in answering those questions. Thus, natural kinds can certainly back up the metaphysics of ready-made phenomena, but not the epistemology of reliabilism: unless one *knows already* that something is a natural kind (e.g. that oxygen is, but phlogiston is not), one cannot legitimately claim to infer it from data in a reliable way, i.e. in a way that generates *true* beliefs with high frequency.

3 A Kantian stance on phenomena

I turn now to a Kantian stance on phenomena and hence on data-to-phenomena inferences, building up on some intuitions originally presented in Massimi (2007), and highlight some of the metaphysical and epistemological features that make it worth pursuing it, to my eyes.

I said a "Kantian stance". It is a 'stance', because echoing van Fraassen (2002) there is an element of voluntarism in endorsing a Kantian perspective on phenomena. I do not aim to offer a scientific, quasi-scientific or metaphysical theory of phenomena-cognition, but only to describe the *human epistemic conditions* under which we gain knowledge of phenomena. In this sense, what I present here below is only a 'stance' that is non-committal about any specific matters of fact about human cognition. So much for clarifying why I call it a 'stance'.

It is 'Kantian' because it goes back to Immanuel Kant's insight about phenomena as "conceptualised appearances". Kant drew an important distinction between what he called 'appearances' and 'phenomena'. An appearance, for Kant, is "the undetermined object of an empirical intuition" Kant (1781/1787, A20/B34). Appearance refers then to an object as merely given in sensibility and conceptually still 'undetermined', not brought yet under the categories of the faculty of understanding. A phenomenon, on the other hand, is a *conceptually determined appearance*, namely an appearance that has been brought under the categories of the understanding: "appearances, to the extent that as objects they are thought in accordance with the unity of the categories, are called phenomena" (ibid., A249). We gain scientific knowledge of nature by subsuming appearances (i.e. spatiotemporal objects as given to our mind in empirical intuition) under a priori concepts of the understanding (via schemata).

In Massimi (2008, and forthcoming) I claimed that the special role Kant assigned to Galilean–Newtonian physics should be understood precisely in the light of the new conception of phenomena Kant was putting forward. Phenomena are not ready-made in nature, instead we have somehow to *make* them. And we make them by first ascribing certain *spatiotemporal properties* to appearances (for instance, for the phenomenon of free-fall investigated by Galileo, the property of acquiring the same speed over different inclined planes with the same height), and then by subsuming them under a *causal concept*, such as Newton's gravitational attraction. But to what extent can we extract Kant's conception of phenomena from its philosophical and historical context, and make it valuable for current discussions about data and phenomena? How would a Kantian stance on phenomena look like? And what good would it be?

Here is my suggestion, which is only tentative and does not claim to be exhaustive or all-encompassing:

(A) The inference from data to phenomena can be understood as the relation Kant envisaged between appearances and phenomena as follows:

i. Scientific investigation starts with *data*, observable records of occurrences which can take the form of conditional relative frequencies (e.g. how many times the 'green' light of a particle detector goes off, given a chosen experimental setting; how many times the same degree of speed is recorded, given a plane with a certain inclination and height).
ii. Data are then plugged into *salient experimental parameters* (e.g. scattering cross-section in particle collisions; acceleration for free-falling objects).
iii. The salient experimental parameters are then organised in such a way as to make possible the production of graphs (which can or cannot be computer-aided) showing how variation in one experimental parameter affects variation in another experimental parameter (e.g. how the scattering cross-section varies by varying the energy of collision;[3] or how the space-to-time relationship varies given a certain acceleration). It is at this level that a new phenomenon can make its debut in the form of an unexpected graph solicited by data plugged into the experimental parameters.
iv. We then have to find a model with new (unobservable and more removed from data) *parameters* (e.g. the parameter R of hadron-to-muon production in scattering cross-sections of particles; or Newton's gravitational constant g) that *maximise* the probability of the graph found. It is at this level that a *causal concept* is introduced to back up the new parameter in its job of maximising the probability of the found graph. So we say that the parameter R shows an unexpected value of 10/3 (matching the unexpected peak at 3.1. GeV in the graph), *because* of some anomalous hadron production *caused* by the presence of a fourth quark c with fractional charge 2/3. Or we say that free-falling objects obey Galileo's times-squared law *because* the gravitational acceleration g can be regarded as approximately constant on the surface of the Earth.

The phenomena scientists investigate are often the end product of these series of intermediate steps, at quite a distance from the original data. Not only then can they be unobservable, as Bogen and Woodward have rightly pointed out; they may also require a significant amount of *conceptual construction.* By making phenomena the serendipitous result of what Kant called the faculty of sensibility and the faculty of understanding, or—as we may prefer to say today—the result of both input from nature (in the form of data) and human contribution (in the form of causal concepts), a Kantian stance can capture the data-to-phenomena inference in a novel way.

Indeed, I think that a Kantian stance on phenomena can satisfy Patrick Suppes (1962) hierarchy of models and go in the direction of clarifying an important point about that hierarchy. Suppes (1962, p. 259) famously laid out an hierarchy of models, whereby the lowest layer is occupied by "*ceteris paribus* conditions", whose typical

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[3] I am referring here to Burton Richter's 1974 data model that led to the discovery of an unexpected peak at 3.1 GeV in the computer-aided graph about the scattering cross-section of electron–positron collisions obtained by varying the energy of the collisions (for details of this case study in relation to my analysis of data models, see Massimi 2007).

problems are noise control with no formal statistics; the next layer up is "experimental design", dealing with problems such as randomization, assignment of subjects and any relevant info about the design of the experiment. Then, we have the layer of "models of data" properly speaking, dealing with three main problems: (1) homogeneity, (2) stationarity, (3) fit of experimental parameters, followed up by "models of experiment" dealing with problems such as number of trials and choice of experimental parameters "far removed from the actual data" (ibid., p. 255). Finally, on top of the hierarchy, we find "linear response models" dealing with problems such as goodness of fit to models of data and estimation of parameters that may well be unobservable (e.g. in Suppes' example from learning theory, the unobservable parameter which "is not part of the recorded data" is the learning parameter θ taking as values real numbers).

First thing to note about this hierarchy is that noise control, data models and phenomena constitute distinct layers. In particular, noise control belongs to the lowest layer of *ceteris paribus* conditions, not even to the data models layer, even less so to the level of linear response models, which I take to be identifiable with Bogen and Woodward's 'phenomena', since it involves the estimation of parameters not observable and far removed from the recorded data.

The crucial point about 'data models', 'models of experiment' and 'linear response models'—to use Suppes' terminology—is to spell out the link between the experimental parameters, as given by relative frequencies of occurrences (raw data) *and* the estimation of unobservable parameters (like θ in Suppes' example from learning theory, or R in my (Massimi 2007) Richter's example from high-energy physics), which act at the interface between experiment and theory. This link is what a Kantian stance can take care of, in my view.

The advantage of a Kantian stance is that it gives the bottom-up, empiricist approach championed by Suppes, Bogen and Woodward its due, while also acknowledging that phenomena are in part the product of the way scientists carve nature at its joints. In the next section, I take a closer look at a Kantian stance on phenomena by highlighting two main metaphysical and epistemological aspects and by comparing and contrasting it with McAllister's similar view.

3.1 Metaphysical and epistemological aspects of a Kantian stance on phenomena

3.1.1 No ready-made phenomena

A Kantian stance does not commit to any metaphysics of ready-made phenomena. From a Kantian point of view, the operation of the scientific instruments and the ensuing production of data together with what Kant called "principles of reason" play a pivotal role in the *constitution of phenomena*.

In Massimi (2008, and forthcoming) I have investigated this distinctive aspect of a Kantian conception of phenomena in relation to Galileo's mathematization of nature. From a Kantian perspective, the goal of the inclined plane experiment was to extract from the appearance (motion of a bronze ball along an inclined plane) the property of uniform acceleration that Galileo had himself a priori inserted in the appearance for the sake of possible experience. Namely, for the sake of experiencing uniformly accelerated motion, we must *constitute* the properties of free-falling bodies according to Galileo's kinematical reasoning. Galileo did not arrive at his law of free fall by simple curve-fitting data about balls rolling down inclined planes. There was instead an element of construction, a "principle of reason" that guided Galileo in his experiments with inclined planes and led him to the phenomenon of uniformly accelerated free-falling bodies.

Similar analyses could of course be carried out in reference to many other examples and case studies. The reason why I am concentrating on Galileo is because of its direct link with Kant, of course, and also because it provides a springboard to contrast a Kantian stance with James McAllister's alternative analysis of Galileo along the lines of his account of phenomena as patterns in data sets. Galileo provides indeed a nice foil to compare the *mind-dependence* inherent a Kantian account with what I take to be the *mind-dependence* implicit in McAllister's account, to which I now turn.[4]

McAllister (1997) locates mind-dependence in the contingent, investigator-dependent noise control settings, which as a result engender different phenomena out of the same data set:

> any given data set can be described as the sum of any one of infinitely many distinct patterns and a corresponding incidence of noise. (…) 'Pattern A + noise at m percent', 'Pattern B + noise at n percent' and so on. (…) As a data set lends itself equally readily to being described as containing any amount of noise, this value will have to be fixed by investigators (…) this option yields an investigator-relative notion of phenomena. (ibid., pp. 219, 223)

While, my Kantian stance goes along the lines of McAllister's in rejecting an ontology of phenomena as being 'out there' in nature causing patterns in data sets, at the same time it disagrees with McAllister about where to locate the mind-dependent aspect of phenomena. I would not locate it at the level of noise control, and I envisage a more robust role for it. Take McAllister's (2004, pp. 1166–1168) analysis of Galileo's experiments:

> An example of a phenomenon, according to Galileo, is free fall. However, each instance of free fall is also partly determined by accidents (…) In the limiting case, if the influence of accidents could be reduced to zero, it would be possible to read off the properties of the phenomenon from an occurrence. (…) Galileo's polishing and smoothing of his experimental apparatus yielded the desired result. (…) Distinct performances of any concrete experiment with falling bodies that was technically feasible at Galileo's time would not have accorded on any clear-cut phenomenon of free-fall.

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[4] James McAllister (private communication) would disagree with my use of the expression 'mind-dependence' to refer to his position, which is meant to capture a 'thoroughgoing empiricism', whereby the expression 'phenomena' does not appear any longer and 'all evidence about the structure of the world is constituted by patterns in data sets' (see McAllister's paper in this volume). As the discussion here below will clarify, I use the expression 'mind-dependence' to refer to the specific 'investigator-dependent' choice of the noise level, which enters in McAllister's analysis of how some patterns in data sets are identified as phenomena.

I completely agree with McAllister's comment that no actual amount of experience would back up Galileo's phenomenon of free-fall. At the same time, I resist his conclusion that if Galileo had only been able to reduce the influence of accidents and noise to zero, he would have been able to read off the properties of the phenomenon of free fall from occurrences. No amount of polishing and smoothing the experimental apparatus would be tantamount to the phenomenon of free fall, unless we introduce some key assumptions or suppositions under which we construct certain kinematical properties, which is precisely what Galileo did, according to the Kantian line I am suggesting.

Thus, I share McAllister's ontological perspective in recognising that the world is a complex causal mechanism that produces data in which a variety of phenomena can be discerned, but I diverge from him when it comes to the identification of the mind-dependent feature of phenomena with noise control. I think instead that the latter is to be identified with the way in which kinematical data are organised and dynamical/causal concepts applied. Of course, this divergence is indicative of a more substantial divergence about the human contribution to the phenomena. While McAllister thinks that it is down to investigators to stipulate which patterns in data sets count as phenomena, I think there is more to this process than human 'stipulation'.

3.1.2 Phenomena are not stipulated by investigations

McAllister (1997, p. 217) defends the view that "each investigator may stipulate which patterns correspond to phenomena for him or her", by differently setting the noise level, and hence by identifying different patterns in the same data set. He challenges Bogen and Woodward "to pick out independently of the content of scientific theories which patterns in data sets correspond to phenomena" (ibid., p. 222). Consider, for example, the phenomenon of planetary motions (ibid., p. 226):

> Asked in what the phenomenon of planetary orbits consists, Kepler would have replied 'In the fact that, with such-and-such noise level, they are ellipses', while Newton would have replied 'In the fact that, with such-and-such noise level, they are particular curves that differ from ellipses, because of the gravitational pull of other bodies'. Thus, phenomena…vary from one investigator to another…. Which aspect of the occurrences is singled out for explanation varies from one investigator to another, notwithstanding the fixedness of physical occurrences themselves…there are no grounds for claiming that either Aristarchus, Kepler, Newton, Einstein, or anyone else has correctly identified the pattern to which the phenomenon of planetary orbits corresponds while the others have failed.

I think that McAllister's characterization of patterns in data sets plus noise is (1) neither a sufficient, (2) nor a necessary condition for phenomena.

(1) It is not a sufficient condition because it is *overpermissive*: it multiplies phenomena without necessity, unless some suitable provisos are introduced. It is not the case that all regular patterns in data sets (obtained by simply varying levels of noise at the investigator's discretion) qualify as phenomena. Take as an example William Prout's chemical theory in 1815. On the basis of tables of

atomic weights available at the time, Prout thought that the atomic weight of every element was an integer multiple of the hydrogen and hence identified the hydrogen atom as the basic constituent of all chemical elements. Although Prout identified a genuine pattern in data sets (plus noise level/accidents) which was indeed vindicated in modern chemistry with the discovery of isotopes, we would not qualify it as a phenomenon. Or take as another example eighteenth-century chemistry. In post-Lavoisier chemistry, under the influence of Lavoisier's idea of oxygen as the principle of acidity, compound substances were normally listed in a series according to the amount of oxygen they contained, and regular patterns—namely, series of binary combinations of oxygen with simple substances up to four degrees of "oxygenation"—were identified and became popular in chemistry textbooks of the time. There was however a problem with muriatic acid. Lavoisier assumed that it was an oxide of an unknown radical which he called the 'muriatic radical', and that it could be further oxidated, to form oxygenated muriatic acid (later, chlorine). This whole muriatic series was built on an assumed analogy with the most common acids, namely vitriolic and nitric. But the analogy was not borne out, and the series listed in chemistry textbooks of the early 19th century was revised after Davy determined that chlorine was an element, that muriatic acid was hydrochloric acid (HCl), which contains no oxygen, and hence that the hypothesised muriatic radical did not exist at all.[5] Again, not every identifiable pattern in a data set qualifies as a genuine phenomenon.

(2) McAllister's characterization is not a necessary condition either for phenomena because it *oversimplifies* the notion of phenomena by reducing it to a linear structure of the form $F(x) = a \sin \omega x + b \cos \omega x + R(x)$ (where $R(x)$ is the noise term—see McAllister 1997, p. 219). A physical phenomenon is a more complex entity that this linear structure. And indeed most phenomena do not obey this structure. Phenomena crucially involve *parameters* and hence *concepts* that shape data sets in a way rather than another. And typically different parameters and concepts used to carve data engender different phenomena in a way that vindicates the complexity of scientific revolutions.

McAllister's analysis of how Aristarchus, Kepler, Newton, and Einstein engendered different phenomena by setting the noise levels differently does not seem to capture the genuine conceptual revolution that took place in the passage from Aristotelianism to Copernicanism. Similarly, his analysis of Galileo's experiment with the inclined plane and the phenomenon of free fall does not seem to vindicate the genuine conceptual revolution that Galileo brought to mechanics, compared to the Aristotelians.

In this respect, I think that a Kantian stance can provide a better philosophical standpoint for the history of science: in the end, we do want to defend the view that the development of physics from Galileo to Newton was a scientific revolution that improved our understanding of nature, compared to Aristotelian physics. And one possible way to go about making this claim is to look at the immediately preceding past history (say, Medieval impetus theory), its transformation with Galileo's concept

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[5] I thank Hasok Chang and Georgette Taylor, to whom I owe this example.

of ‘impeto’ (which was at quite a distance from Buridan and Oresme and yet still somehow related to the Archimedean science of weights), and its paving the way to Newton’s new concept of ‘gravitational attraction’ (for details, see Massimi forthcoming). The specific Kantian story of how we construct phenomena (by ascribing spatio-temporal properties to appearances that we then have to prove via experiments, and subsume under suitable causal concepts) does justice to the idea of scientific progress. After all, justifying the progress achieved by Galilean–Newtonian physics was precisely what Kant’s transcendental philosophy aimed at.

4 Conclusion

As I hope to have clarified, a Kantian stance on phenomena can potentially offer a genuinely new perspective on the issue of how we infer phenomena from data, by taking the distance both from a metaphysics of ready-made phenomena and from conventionalist readings. Needless to say, spelling out the details of a Kantian stance and how exactly it translates into modern science (leaving aside most, if not all, the baggage of Kant’s transcendental philosophy) is a very challenging enterprise and much more work needs to be done. But my goal in this paper was to show that this is an enterprise worth pursuing for all the metaphysical, epistemological, and also historical reasons briefly canvassed.

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