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Theory-Ladenness and Scientific Instruments in Experimentation

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Since the late 1950s one of the most important and influential views of post-positivist philosophy of science has been the theory-ladenness of observation. It comes in at least two forms: either as a psychological law pertaining to human perception (whether scientific or not) or as conceptual insight concerning the nature and functioning of scientific language and its meaning. According to its psychological form, perceptions of scientists, as perceptions of humans generally, are guided by prior beliefs and expectations, and perception has a peculiar holist character. In its conceptual form it maintains that scientists' observations rest on the theories they accept and that the meaning of the observational terms involved depends upon the theoretical context in which they occur. Frequently, these two versions are combined with each other and give rise to a constructivist view of scientific knowledge (I shall use the term "constructivism" roughly in the same way as Golinski [1998, chap. 1]). According to this outlook, our experience is categorized and preconditioned by prior belief since the process of gaining knowledge through science always involves the use of concepts from some theory or other. This view can easily be strengthened to serve as the cornerstone of a constructivist and anti-empiricist account of science: The categories in terms of which we carve up our experience are not read off from the external world but follow from prior theoretical commitments.

The implications of theory-ladenness for a view of scientific experimentation are straightforward: If observations are theory-laden and if experimentation involves observation, then experimentation has to be theory-laden too. Since experiments, according to this view, make sense only in relation to some theoretical background they cannot play a role that is theory-independent. That means that an experiment can make sense only on the basis of some prior theory.

In the first part of this paper I shall discuss the view of theory-ladenness as it appeared in the work of its originators and draw a distinction between three different meanings of the term. In the second part, I shall develop a classification of instruments that reflects these different meanings and specifies the different roles instruments come to play in experiments. Before instruments can be employed in a theoretical framework, they have first to be employed causally. I shall illustrate my view of instruments, and thereby of experiment, by drawing on Kuhn's discussion of Wilhelm Roentgen's discovery of the X-rays and by referring to the way Georg Simon Ohm developed the law governing the flow of electricity in conductors that bears his name.

1. Three Conceptions Of Theory-Ladenness

In order to develop the causal view of experiment and to investigate how it fares in relation to the theory-ladenness of observation, we first have to get an overview of the different meanings of the latter notion. In discussing theory-ladenness the average postpositivist is likely to refer to Norwood Russell Hanson's book *Patterns of Discovery*. Strangely enough, the reader will almost never bother to go beyond the first chapter of this book which is entitled "Observation." This chapter provides ample material for quotations which can be used in defending theory-ladenness against stubborn positivists. "[S]eeing is a 'theory-laden' undertaking," we read. "Observation of *x* is shaped by prior knowledge of *x*. Another influence on observations rests in the language or notation used to express what we know, and without which there would be little we could recognize as knowledge." (Hanson 1958, 19)

It should be noticed, however, that in chapter 3, entitled "Causality," Hanson's view receives a peculiar twist which must not be overlooked. He tells us there that theory-laden talk in science is mainly causal talk, talk in which causes and effects and the connections between them are identified. Hanson further maintains that this way of talking is to be contrasted with the use of sense-datum language, which is devoid of any causal meaning. The only way science fulfills its major goal, explanation, is by invoking causality: "Notice the dissimilarity between 'theory-loaded' nouns and verbs, without which no causal account could be given, and those of a phenomenal variety, such as 'solaroid disc', 'horizoid patch', 'from left to right', 'disappearing', 'bitter'. In a pure sense-datum language causal connexions could not be expressed. All words would be on the same logical level: no one of them would have explanatory power sufficient to serve in a causal account of neighbour-events." (Hanson 1958, 59) This quotations shows two things: First of all, against all claims to the contrary, there *can* be perceptual accounts according to Hanson that are free from theory. It is another matter that he attributes to them no great use in science since their phenomenal nature prevents them from having any explanatory content. Second, theory-ladenness in science primarily means “causality-ladenness” for Hanson, being loaded with causal meaning: “The notions behind ‘the cause x’ and ‘the effect y’ are intelligible only against a pattern of theory, namely one which puts guarantees on inferences from x to y. Such guarantees distinguish truly causal sequences from mere coincidence.” (Hanson 1958, 64)

Before we can use this insight for a workable account of the nature of experiment, let us consider the next “founding fathers” of theory-ladenness: Pierre Duhem, and his work *La théorie physique--Son objet et sa structure*. Duhem discriminates between a fact and its theoretical interpretation, or, as he says, between a “concrete” and a “theoretical fact.” He tells us that an experiment in physics involves two parts:

> In the first place, it consists in the observation of certain facts; in order to make this observation it suffices for you to be attentive and alert enough with your senses. It is not necessary to know physics; the director of the laboratory may be less skillful in this matter of observation than the assistant. In the second place, it consists in the interpretation of the observed facts; in order to make this interpretation it does not suffice to have an alert attention and practiced eye; it is necessary to know the accepted theories and to know how to apply them, in short, to be a physicist. (Duhem 1974 [1906], 145)

> An experiment in physics is the precise observation of phenomena accompanied by an *interpretation* of these phenomena; this interpretation substitutes for the concrete data really gathered by observation abstract and symbolic representations which correspond to them by virtue of the theories admitted by the observer....The result of an experiment in physics is an abstract and symbolic judgment. (Duhem 1974 [1906], 147)

This quotation demonstrates that Duhem’s conception of theory-ladenness is clearly different from Hanson’s. For him, experiments in physics are done on a level of the scientific enterprise that is not explanatory. As becomes evident, it would not be sufficient for Duhem simply to place practical facts into a web of causal relations in order to make them theoretical: “The result of common experience is the perception of a relation between diverse concrete facts. Such a fact having been artificially produced some other fact has resulted from it. For instance, a frog has been decapitated, and the left leg has been pricked with a needle, the right leg has been set into motion and has tried to move away from the needle: there you have the result of an experiment in physiology. It is a recital of concrete and obvious facts, and in order to understand it, not a word of physiology need be known.”

(Duhem 1974 [1906], 147) This description is as causal as it could be, but its causal nature is obviously not enough for Duhem to make it a theoretical fact. For Duhem, theory-ladenness has therefore little to do with causality, but with inscribing phenomena in the terms of an abstract and symbolic structure: "The result of the operations in which an experimental physicist is engaged is by no means the perception of a group of concrete facts; it is the formulation of a judgment interrelating certain abstract and symbolic ideas which theories alone correlate with the facts really observed." (Duhem 1974 [1906], 147)

In order to understand what Duhem has in mind here we need to clarify his distinction between experimentation at an advanced level of theory and experimentation at the level of "common experience," which is not theoretical at all. A theory is advanced, according to Duhem, when it provides an interpretation of experimental laws by substituting abstract and symbolic representations for them. In less advanced sciences like physiology or certain branches of chemistry "where mathematical theory has not yet introduced its symbolic representations," the experimenter can reason "directly on the facts by a method which is only common sense brought to greater attentiveness." (Duhem 1974 [1906], 180; remember the example of the frog above!) In order to specify the rules that are in operation in this common sense reasoning Duhem quotes at length from his countryman, the physiologist Claude Bernard. Duhem thus clearly admits the possibility of observations and experiments that are free from theory, although only at a less advanced level of science. Note, however, that being theory-free means something different for him than for Hanson.

To record our results so far: Hanson and Duhem have different conceptions of theory-ladenness: Whereas for Hanson any injection of causality into the mere registering of facts is bound to render them theoretical, it is, for Duhem, with the representation of (causal) relations in an abstract, noncausal structure that theory begins.

Let us have a look now at the third advocate of theory-ladenness, Thomas Kuhn. For Kuhn, theory-ladenness is first of all "paradigm-ladenness": The normal-scientific tradition in which one has been trained, and the experiences that this has brought about determine how the scientist sees his world:

> [S]omething like a paradigm is prerequisite to perception itself. What a man sees depends both upon what he looks at and also upon what his previous visual-conceptual experience has taught him to see. (Kuhn 1970, 113)

> [P]aradigm changes do cause scientists to see the world of their research-engagement differently. (Kuhn 1970, 111)

> Within the new paradigm, old terms, concepts and experiments fall into new relationships one with the other. (Kuhn 1970, 149)

> The proponents of different theories are like the members of different language-culture communities. (Kuhn 1970, 205)

Here we have the fusion I referred to at the beginning of this chapter between a psychological law or laws pertaining to perception and a particular philosophical view of the functioning of scientific language, which holds that scientific terms derive their meanings from prior experiences, beliefs, or theories and possess meaning only in their context.

Or so it seems. If we look closer, we find that even Kuhn admits the possibility of "fundamental novelties of fact,"—that is, of genuine discovery that goes *against* a well-established paradigm. Without this possibility, as he himself realizes, science could only develop theoretically and never by adjustment to facts. "Discovery commences with the awareness of anomaly, i.e., with the recognition that nature has somehow violated the paradigm-induced expectations that govern normal science." (Kuhn 1970, 52-53)

We now have to specify exactly where, according to Kuhn, these "paradigm-induced expectations" come from that are violated in discovery: Are they induced by theoretical and abstract structure (à la Duhem) or by the causal properties (à la Hanson) of those elements in question that can be manipulated in experiment? A natural answer for Kuhn would be to say: "both!" "[B]oth observation and conceptualization, fact and assimilation to theory, are inseparably linked to discovery." (Kuhn 1970, 55) If we look closer, however, at Kuhn's own examples, we notice that it is almost always the *theoretical interpretation*, the assimilation to theory, that is taken as decisive for discovery and hardly ever any causal experience. We are primarily shown cases where someone identifies a well-known experiment or entrenched phenomenon in a new way. Lavoisier, we are told, for example, was enabled through his new paradigm "to see in experiments like Priestley's a gas that Priestley had been unable to see there himself" and was "to the end of his life" unable to see. (Kuhn 1970, 56) Assertions to the contrary notwithstanding, novelty in discovery seems for Kuhn to be the result of a paradigm-induced change in "seeing as" and not in a novel experience or recasting of a causal process.

The only case where Kuhn admits that discovery has been effected by a genuinely novel causal experience seems to be the case of the X-rays. "Its story opens on the day that the physicist Roentgen interrupted a normal investigation of cathode rays because he had noticed that a barium platino-cyanide screen at some distance from his shielded apparatus glowed when the discharge was in process." (Kuhn 1970, 57) Although Kuhn seems to consider this observation theory-laden, I maintain that, in Duhem's sense, it is not. If it *were*, Roentgen, by definition of theory-ladenness, would have been able to interpret it in light of the theories of physics he had at his disposal. But here it is exactly the point that his theories deserted him and he could *not* find a place for this new experience in his customary theoretical structure. For this reason he interrupted his investigation and asked himself why the screen had come to glow. Yet it goes without saying that the novel observation is theory-laden in the sense of Hanson, because Roentgen immediately looked for a causal relationship between his apparatus and the glowing of the screen, although this went completely against all his expectations!

A follower of Kuhn might now say that Roentgen would never have paid attention to the glowing screen if he had not disposed of deeply entrenched theories of physics that *prohibited* such a phenomenon. This shows again, as Kuhn's advocate could continue, that observation is governed by expectation—as it happens a conflicting one this time—and that therefore, at least in this sense, Roentgen's observation was also theory-laden. This might be true, but note that this is now a *third* sense in which the notion of theory-ladenness is used. It says something about the likelihood with which an observation occurs, the ease with which a phenomenon is detected or paid attention to in the light of a paradigm: An observation is theory-laden in this sense if it were improbable that an observer would have made it (that an observer would have noticed it or would have attributed any importance to it) without her holding a particular theory beforehand.

This is *not*, however, a claim about the nature of observation and its relation to theory, as the earlier discussed view of theory-ladenness would require, but about the disposition of a subject to perceptually detect or discriminate a phenomenon in relation to her prior experiences and theoretical belief or disbelief. It is certainly true that we tend to notice or overlook phenomena depending on certain expectations and beliefs. Whatever, though, the relation between the expectations of an observer and her perceptual abilities might actually be, it cannot by itself establish any relevance of an observer's belief to the *meaning* of the observational terms involved. If this were the case we would not be able to detect any anomalies—that is, observations that *contradict* our theoretical expectations. (Since anomalies are prerequisite to scientific revolution in Kuhn's sense, Kuhn cannot renounce them for his own theory.)

In order, therefore, to distinguish this third type of "theory-ladenness" (if this label is still appropriate at all) from the two types associated before with Hanson and Duhem, respectively, let us call it henceforth "theory-guidance." It refers to how the disposition to make a particular observation depends on the theoretical background of the observer, and it should primarily be associated with Kuhn.[1] As we have seen, however, theory-guidance cannot be taken as genuine theory-ladenness because of its irrelevance to the *meaning* of observation sentences.

Let us step back into Roentgen's laboratory for a moment. What did he do after he noticed the anomaly? He conducted various experiments in order to explore the *cause* of the incident: "Further investigations--they required seven hectic weeks during which Roentgen rarely left the laboratory--indicated that the cause of the glow came in straight lines from the cathode ray tube, that the radiation cast shadows, could not be deflected by a magnet, and much else besides. Before announcing his discovery, Roentgen had convinced himself that his effect was not due to cathode rays but to an agent with at least some similarity to light." (Kuhn 1970, 57) This is perhaps the only place in his book where Kuhn uses the term "cause" (or an equivalent expression) in relation to an experimental investigation. The quotation shows vividly that Roentgen's experiments were not conducted to test a theory but to expand our knowledge of causal connections in relation to the scientific instruments and devices involved. (Steinle [1998, 284-292] investigated this type of experimentation more closely and called it "exploratory.")

What does our discussion suggest as the most adequate description of Roentgen's early investigations? They were certainly theory-guided in the sense of Kuhn and they were causality-laden in the sense of Hanson, but not (or not yet), I claim, theory-laden in the sense of Duhem. The experiments Roentgen conducted during his seven hectic weeks were in the same way a "recital of concrete and obvious facts" as the above mentioned experiment of decapitating the frog was. To draw an analogy to Duhem's case, we can say that Roentgen could have understood these facts even if he had not known a word of physics. The only knowledge he had to have for conducting his initial experiments was about the *causal power of the instruments* he used. (It goes without saying that looking at an X-ray tube became gradually theory-laden the more the X-ray tube became embedded in a new theory.)

Roentgen's early series of experimentation has to be (and it *can* be!) systematically distinguished and separated from the Kuhnian process of "assimilation to theory." Such an assimilation can of course also proceed by experimentation. Kuhn is right when he says that only after this assimilation has been achieved and the phenomena have received an abstract and symbolic representation can we speak of a "discovery" of X-rays. Yet before this interpretation has taken place, we can only say that an anomaly has occurred.

The case of the X-rays shows, however, that in an important sense experimentation itself can be, and very often is, autonomous and free from theory.[2] It is wrong to see experiment as nothing more than a test of preconceived ideas gained by a theoretical interpretation. We should therefore learn a lesson from the X-ray case and distinguish between two kinds of experiments: those that are causal, but not embedded in a theoretical structure and those that presuppose the knowledge of such a framework. I think that Kuhn's discussion caused much damage by blurring and dissolving this difference and by identifying the concept of a paradigm too much with the Duhemian conception of theoretical interpretation. Sometimes Kuhn seems to realize this when he stresses that "[a]t a level lower or more concrete than that of laws and theories, there is, for example, a multitude of commitments to preferred types of instrumentation and to the ways in which accepted instruments may legitimately be employed." (Kuhn 1970, 40). And he explicitly refers to the discovery of X-rays as a case in point. So even in Kuhn a sense turns up in which experiment can be independent of the theoretical commitments of paradigm and dependent only on an entrenched tradition of instrumentation, although Kuhn does not pursue this idea further.

Is my emphasis of an autonomous "lower level" in experimentation a relapse into a positivist spirit? Definitely not. Nowhere in my argument appears an appeal to a neutral experiential authority like "immediate experience," "bare sense-data," "the given" or "pure observation-language" that is to decide conclusively for or against a theory. All that is claimed here is that two types of experimentation should be kept conceptually apart: experimentation at the causal level, where instrumental manipulation is distinguished, and experimentation taking place at the theoretical level, where the results at the causal level are represented in a theoretical superstructure (that can itself also have causal meaning). In this way, all those claims to theory-independence that for Kuhn and Hanson were typical of positivism can be avoided.

Our discussion so far suggests two things: one should first of all distinguish "theory-guidance" from the notion of "theory-ladenness." Even if all experimentation were guided by theory, in the sense found in Kuhn, this alone would not be enough to prove that observations of experimental results are theory-laden. The reason for this is that theory-guidance alone is not able to establish a relation between the *meaning* of observational terms and the *meaning* of the theory that guided it. Second, one should distinguish between “theory-ladenness through appeal to causal understanding” and “theory-ladenness through theoretical interpretation,” or, in Kuhn’s words, “assimilation to theory.” The latter has to reflect the former, but not necessarily the other way around, as Kuhn and Hanson (and many others) usually seem to suggest. It makes much more sense to regard causal understanding and theoretical understanding moving toward each other from separate and independent starting-points until they meet at a stable state of equilibrium than to mingle them beforehand.

In order to avoid misunderstandings it is then even better to reserve the term “theory-ladenness" for the cases Duhem had in mind—that is, for “theory-ladenness through theoretical interpretation.” When Hanson introduced the term in the course of his own discussion (actually he coined the expression “theory-loaded”), his primary intention was not to claim that all observation is interpreted in the light of a theory, but to stress that, contrary to the positivists, observation always presupposes some causal notion that transcends direct experience. One can of course maintain that all causal talk is theoretical talk, and Hanson was perhaps an advocate of this opinion. I think, however, that this goes too far, because there are many cases in our everyday communication where we use causally loaded terms in an explanatory fashion without referring to any theory or theoretical entities. When someone asks me why the light went on, I can use causal words in my answer (“I turned the switch”) without invoking any theory about the nature of electricity and electric action whatsoever. The term “theory-laden” in its original sense refers to genuine theoretical interpretation that transcends causal understanding of “common experience.” It should be reserved for these cases and not be diluted in the sense advocated by Hanson.

2. Instruments And Their Use In Experimentation

In the light of the forgoing discussion, it seems advantageous to identify as the central and primary constituent of scientific experimentation the causal agents involved—that is, *scientific instruments*. In this way, experimentation can be investigated more closely in its two basic forms: as improvement and expansion of causal knowledge and as adjustment to a theoretical context. In addition, this way of putting things makes it possible to envisage a genuine history of experimentation that is driven by the instruments involved.

If we look at experiment in its *first* form, as causal manipulation by means of instruments, we can distinguish between instruments that are used in order to fulfil a *productive* and those that have a *constructive* function.[3] The goal of productive instruments is to produce phenomena that normally do not appear in the realm of human experience. Roentgen was using his apparatus productively when he tried to accomplish other, hitherto unknown, effects with it besides the glowing of a barium platinocyanide screen. As we have seen, he found out that under certain conditions he could make it cast a shadow. Roentgen's apparatus was, as we might say, *unconditionally productive*, but there are other productive instruments that produce *known* phenomena—although in circumstances where they have not appeared before. I am thinking of instruments, like microscopes or telescopes, used in order to improve human perception. Still another type of a *conditionally productive* instrument is one that tries to analyze or to split a phenomenon into different, previously unknown components. A case in point is the spectroscope.

Roentgen also used his instruments *constructively* when he tried to influence phenomena in order to make them behave in a certain way. The goal of such experiments is to produce an effect in its "pure form," without any complications or additions that could spoil it or that are otherwise alien to it. Another goal is to tame the phenomena in order to be able to manipulate them in a certain desired way. Ernan McMullin spoke of the "causal idealization" of Galileo's experiments in this respect. (McMullin 1985, 247-73) We can also refer to another of Kuhn's favorite examples, the Leyden jar, invented around 1745. This instrument was not used at the time to *uncover* the phenomenon of electricity, so to speak, but to collect and store electricity in it. It was developed in order to produce a desired effect in a desired way.

Still another type of experimentation in its first form is experimentation by means of *imitative* instruments. They are used to produce effects in the same way as they appear in nature without human intervention. In biology, for example, we find experiments in which an apparatus is used that closely simulates the production of an enzyme in an organism.

If we look at experiment in its *second* form, as adjustment to a theoretical context or assimilation to a theoretical interpretation with the help of instruments, we see other functions of instruments step to the foreground—above all, what I call the *representative* role of instruments. In this case, the goal is to represent symbolically in an instrument the relations between natural phenomena and thus to better understand how phenomena are ordered and related to each other. Examples of instruments that fulfil such a function are clocks, balances, electrometers, galvanometers, thermometers etc. These are "information-transforming instruments," as Davis Baird once called them; they transform the input information into a more useful output format while preserving the order of the phenomena vis-à-vis the intensity of the attribute in question. (Baird 1987, 328) In a thermometer, for example, the different states of heat accessible to our sense of heat are transformed into different states of the instrument itself (that is, different heights of the mercury column) that are accessible to sight. The order of the heat states is more or less preserved in the order of the heights of the column (cf. Mach 1896). The changes the instrument undergoes can be taken as representative of the changes of the measured phenomena.

The difference between the use of productive and constructive instruments on the first level of experimentation and the use of representative instruments on the theoretical level mirrors to some extent other distinctions that have been proposed: there is the old difference of the seventeenth century between "philosophical" and "mathematical instruments." This is taken up in our time by Jed Buchwald when he suggests to distinguish "discovering experiments" from "measuring experiments." (Buchwald 1993, 200) In a similar way, Willem D. Hackmann makes a difference between "active instruments" that intervene in nature and "passive" ones that try to minimize any effect on the relevant object. (Hackmann 1989, 39-40)

In order to illustrate my claim, I would like to have a look at the experiments that led to Georg Simon Ohm's law as a case in point and ask how Ohm's law of electricity theory was discovered and what role instruments played in this discovery.[4] In the series of experiments he conducted between 1825 and 1827, Ohm relied mainly on two instruments: the electroscope to measure what he called the "electroscopic force" or "tension" in the electric circuit, later identified as "potential difference" by Kirchhoff in 1849, and the galvanometer in order to measure the "exciting force" of the current or the "strength of the magnetic effect on the conductor"—that is, the intensity of the current. Both instruments served constructive and productive functions in Ohm's experiments. The electroscope was first of all used as a constructive device to identify electricity in its pure form, in abstraction from any specific situations in which it arises. But, later, Ohm transposed it from the context of static electricity to the (dynamic) case of electric flow, using it as a conditionally productive device to yield hitherto unknown effects. Consequently, the ensuing usage of "tension" for the dynamic case proved to be very difficult for many of Ohm's contemporaries, and many of them rejected it as unfounded.

The galvanometer had its origin in Oersted's "fundamental experiment," of course, which was conducted in order to produce the magnetic effect of a current carrying wire. In the hands of Ohm it also served as a constructive device when its different states were used as the only aspects relevant to the strength of the dynamic action. This also did not find the acclaim of many of Ohm's contemporaries, because they were convinced that dynamic action of electricity is different in the case where there is some chemical action present. Ohm, however, was a follower of Volta's theory, according to which the electric action depended on the contact of two metals and was not the result of a chemical activity. (The action of the Voltaic pile was explained with the so-called contact theory, which did not see any chemical action present.) Indeed, Ohm thought chemical activities in a "galvanic chain" should be avoided, because they detract from the "natural purity" of the galvanic effect. Following a suggestion of the editor of the *Annalen der Physik*, Ohm used, from 1826 on, a thermoelectric source for his experiments. This thermoelectric apparatus played a double role, both as a productive and a constructive device: a productive role because all other sources of dynamic electricity available at the time were highly instable, vacillating highly in their electric action; and a constructive role because it produced the action in its pure or idealized form, as Ohm thought, without any chemical "contamination," so to speak.

The constructive and productive usage Ohm made of his instruments as described takes place on a causal level that is theory-free and guided by the causal possibilities available with the instruments in question. The *representative* or *symbolic* level is now superimposed on this causal level. Ohm attains for his experiments a representative and symbolic significance by three means: First, he used his instruments not only as productive and constructive devices but also as representing ones—that is, as measuring instruments—arriving thereby at a "symbolic generalization," as Kuhn called it, which functions as a unifying formula. Second, his approach enabled Ohm to create and define a new ing formula. Second, his approach enabled Ohm to create and define a new theoretical concept, the concept of electric "resistance" or "conductivity." And third, he was able to give a theory of the instruments involved—that is, to "substitute for the concrete objects composing these instruments an abstract and schematic representation," as Duhem (1974 [1906], 153) once formulated it.

As already noted, Ohm finally arrived through his measurements at the formula that is known as Ohm's law and which can be written as: $I = V/ R$. The road to this formula was very winding and tortuous indeed, and Ohm had to make many attempts, both in a practical as well as in a theoretical respect, to obtain his result. It is highly significant that his first theoretical conception of electric activity in a closed circuit was guided by the Coulomb paradigm of static electricity and that he was able to describe this already with some version of his law. This implied a concept of "resistance" similar to the mechanical resistance in friction phenomena. Later Ohm conceived of electric conduction in a new way by establishing an analogy between heat conduction as developed in Fourier's theory of heat and the conduction of electricity. In this sense, "resistance" becomes a truly theoretical or theory-laden term that is not yet present at the causal level of Ohm's experiments; it is reached and formulated only at the symbolic level.[5] Ohm could avoid measuring resistance directly in his first-level experiments by the following reasoning: Let I_n be the intensity of an electric circuit where instead of the outer part of the circuit a short and thick "Standard Conductor" is introduced. ("I_n" stands for "normal" or "standard intensity.") If R_i is the inner resistance of the circuit and R_o the outer one, we can put $I_n = V/ R_i$. For a circuit other than with the Standard Conductor, we can thus write $I = (R_i) \; f I_n / R_i + R_o$. I put the first "$R_i$" in parentheses because Ohm overlooked that this is not a constant factor of proportion but depends on the inner resistance of the circuit. It was only at a later stage that he substituted V for $(R_i) \; f I_n$.

It should be obvious that Ohm could also apply his new mathematical formula to the galvanometer and the electrometer and predict their behavior in many different cases. Ultimately it was the practical usability of Ohm's law for the arrangement of all kinds of measurements in the circuit and especially its technical applications, such as in electrical telegraphy, that in the end led to its acceptance. It was soon recognized that Ohm's law was completely neutral with regard to the exact theory of the origin of the electromotive force of electricity; it holds irrespective of "whether that force is regarded as being derived from the contact of dissimilar metals [as its founder himself believed] or as referable to chemical agency," as the Royal Society wrote when it dedicated the Copley medal to Ohm in 1841. (Royal Society 1841, 336)

3. Conclusion

The received view of theory-ladenness in observation and especially in experimentation is too coarse-grained. First of all, we should not mix up theory-ladenness with the concept of "theory guidance" as it appears in Kuhn's work. Second, we should distinguish between theory-ladenness on a primary causal level of scientific experimentation (if it is still appropriate to call it this way) and experimentation on a supervening, secondary level when theory takes possession of the direct causal experience with scientific instruments and when the adjustment of a causal picture to a theoretical and symbolic context is called for. There are many cases where first level experimentation is and can be pursued without taking into account the secondary level.

It is true that in advanced and mature theories the two levels form an inseparable amalgam, as Duhem and Kuhn have amply demonstrated. Yet it is clear that when a new domain is explored, experimentation is conducted in a theory-free way, only constrained by considerations of the causal power of the instruments used. As I have tried to show, the distinction between a causal and a theoretical level of experimentation also sheds new light on the different roles instruments play in scientific experiment. Last but not least, this way of viewing things enables us to give back to experiment some of the epistemic dignity it used to have when empiricism was still in more esteem. This view also liberates us from extreme modes of constructivism without falling back into naive forms of experientialism.

Notes

I am grateful to John Michael for his help in improving my English.

[1] I note in passing that theory-guidance as reconstructed here is not the only claim Kuhn takes over from the psychology of perception. We also encounter the *contrary claim* in his book that the more entrenched a paradigm is the more one neglects anomalies and that this disposition weakens only in periods when a paradigm enters a crisis state.

[2] This claim was of course first raised by Hacking (1983a, esp. chap. 11). My argument for it as presented here, however, differs from his.

[3] The classification of instruments in experimentation as developed in this chapter was first suggested in (Heidelberger 1998).

[4] For a fuller story, see Heidelberger (1980), or—in German translation, with less misprints—Heidelberger (1983).

[5] I have explored in Heidelberger (1979) how Ohm defined "conductivity" in his theory by presupposing the validity of Ohm's law. I also tried to show that this method is *not* circular and that it is followed not only in Ohm's case but that it constitutes a frequently used way to introduce theoretical terms.

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