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CONTEXT OF DISCOVERY VERSUS CONTEXT OF JUSTIFICATION AND THOMAS KUHN

1. INTRODUCTION

Let me begin with a convention. I will refer to the distinction between the context of discovery and the context of justification as "the DJ distinction" (where I may note, for potentially misled younger readers, that this "DJ" has nothing to do with the music business). This paper is based on an older paper of mine (Hoyningen-Huene 1987). In the present paper, I will first recapitulate some of the topics of the older paper, and will contribute further considerations. Subsequently, I will discuss Thomas Kuhn's ideas about justification in science. Thus will be clarified, in which sense precisely Kuhn opposed the DJ distinction. This is noteworthy, because in the 1960s and 1970s, many philosophers concluded from Kuhn's opposition to the context distinction that he just did not understand what it was all about (and they inferred from this that he was just too uneducated as a philosopher to be taken seriously).

My general line will be this: The DJ distinction, as it was used in the 1960s and 1970s, is not just one distinction, but a set of intermingled distinctions. Due to the conflation of various distinctions, the assertion of the DJ distinction contains hidden identity statements among these distinctions. This identification results in massive philosophical assumptions that are highly problematic. As a consequence, much of the discussion of the DJ distinction in the 1960s and 1970s is fairly muddled, because it is not clear what exactly is stated by its defenders and what exactly is attacked by its critics. Eventually, all parties, growing frustrated, turned away from the discussion. Earlier historical details of the DJ distinction will be provided in other chapters of this book.

In section 2, I will discuss the varieties of the DJ distinction. Then, I will demonstrate how these distinctions incorporate several hidden assumptions (section 3). In section 4, I will present Kuhn's somewhat opaque criticism of the DJ distinction as it was formulated in his 1962 *The Structure of Scientific Revolutions*. Section 5 is devoted to Kuhn's positive views about justification in science. In the final section, I shall present a rejuvenated DJ distinction that might be acceptable to all parties.

2. THE VARIETIES OF THE DJ DISTINCTIONS

In this section, I shall distinguish five versions of the DJ distinction that can all be found in the literature. In order to make the DJ distinction as plausible as possible, it is

J. Schickore and F. Steinle (eds.), Revisiting Discovery and Justification, 119–131.

useful to have one of the standard examples that guide the proponents, in mind. Very often, Kekulé's discovery of the ring structure of benzene serves as such an example. According to the standard story, Kekulé who had pondered on the benzene structure for some time, dozed off in front of a fire place. While resting, the image of six carbon atoms forming a cycle appeared in his mind. Hence, the idea of a ring structure for a number of organic molecules was conceived. Subsequently, the community of organic chemists critically discussed whether the idea was right or wrong. In consideration of this example, the following apparently clear and plausible version of the DJ distinction emerges.

Version 1

Discovery and justification are *temporally distinct processes*: At the beginning, something is discovered. Subsequently, it is justified (Mowry 1985, 79 calls this version of the DJ distinction the "standard formulation").

It may seem that this version of the DJ distinction does not have any empirical content, because it appears to be a conceptual consequence of the meaning of "justification" (see, for instance, Popper 1959 [1934], 31, as a clear example of this tendency). Whatever the concrete process of justification may consist of, it presupposes that there is something that has to be justified. Therefore, before the process of justification can begin, the thing to be justified has to be somehow present. Now, it is plausible that in science, anything that is in need of a justification has to be *discovered*; it is not simply given. At least, this approach is plausible if "discovery" is understood in a wide sense that includes "invention". Claims in science that are in need of justification typically comprise new hypotheses, new theories, new models with certain properties, new classifications, new forms of representation, or new phenomena. It is obvious that in this particular version, the DJ distinction relies on a supposed difference between discovery processes and justification processes. If a discovery process could not be differentiated from a justification process, the distinction would collapse.

There are two main objections to this distinction. The first states that phases of discovery and of justification may alternate, that is that the history of science is not a straightforward sequence of discovery and justification, but more complex (see, e.g., Feyerabend 1970, p. 70; Mowry 1985, p. 79). This presumed historical fact has been granted by some of the proponents of the DJ distinction (see, e.g., Salmon 1970, p. 71). In that, the proposed distinction is not challenged, but only a refinement is wanted. It is probable that establishing a complex item, such as a theory, happens as will be outlined as follows. First, a part of the theory is discovered and justified subsequently. Then, another part is discovered and justified subsequently, and so on, until the theory has been discovered and justified in full in this stepwise manner.

The second objection is much more serious. It is doubtful that it is really possible to identify discovery and justification processes in the history of an item that is an unquestionable candidate for having been discovered and having been justified. As an example, let us take an empirical law (for more sophisticated and realistic examples, see specifically Arabatzis (this volume) and Steinle (this volume)). Clearly, empirical laws have to be discovered (they often bear the names of their presumed discoverers), and clearly, they are in need of justification. Now, let us assume that the most recent history of an empirical law consists in the establishment of a higher degree of its quantitative accuracy due to new methods of measurement. How should these more accurate measurements be classified? Are they part of the discovery process of the quantitative refinement of the law? Or are they part of the justification process for the quantitative refinement of the law? It seems impossible to attribute these measurements uniquely to one or the other category. Thus, the identification of discovery processes as opposed to justification processes in the history of science is—at least in some cases—not possible.

A typical defense of the DJ distinction against this objection allows for overlapping contexts, or even that "the process of discovery and the process of justification may be nearly identical" (Salmon 1970, p. 72). Although this may be a defense of some *other* version of the DJ distinction, it does not defend the version discussed here. In fact, it admits that the DJ distinction is insufficient as a distinction between processes of discovery and justification. Whatever the distinction, it is not a distinction between processes. Much of the critical discussion on the DJ distinction has focused on this version (e.g., Feyerabend), and other chapters of this book deal with it as well. As it is clear that this variant is not tenable, the DJ distinction can only be upheld in other versions than the present one.

Version 2

The distinction concerns the *process* of discovery versus the *methods* (in a wide sense) of justification (or testing).

Here, we have a contrast between the factual historical process and methods, considerations, procedures, etc. that are relevant to justify or to test knowledge claims (see, e.g., Feigl 1970, p. 4; Popper 1959 [1934], p. 31; Salmon 1970, pp. 68, 72; Scheffler 1967, pp. 69–73; Siegel 1980a, pp. 299–304; Siegel 1980b, pp. 369–372). Again, this distinction appears to be fairly clear. The part about the methods of justification or testing, however, is ambiguous. On the one hand, it may refer to methods of justification that were used at the time. This is probably the preferred reading by historians. These methods need to be discovered empirically, by historical work. On the other hand, the distinction may refer to methods of justification that "really" establish knowledge claims, independently of the beliefs of the historical actors. Most probably, philosophers committed to a normative philosophy of science will prefer this reading. Methods that "really" establish knowledge claims need to be justified philosophically, whatever that may mean. It is obvious that they cannot be established by any kind of historical work alone. There are problems with both readings.

On the first reading, a similar concern to the main problem with version 1 of the DJ distinction presents itself. How can we distinguish historically used methods of justification from a supposed process of discovery, when such a process separate from justification often does not exist? The distinction presumes the possibility of sorting out scientific activities as belonging to either discovery or justification, and this is often impossible. This reading of the distinction does not work, therefore.

On the second reading, justification or testing is understood in a *normative* (or perhaps more precisely: in an *evaluative*) sense. Something can count as a justification or testing procedure only when its goal is attained, i.e., only if it really establishes justification or a test. On this reading, we must be in command of procedures that tell us how justification and testing must be done. At that, the DJ distinction turns into a special case of the distinction between the descriptive and the normative: historical processes (of discovery) are described, whereas claims of justification or testing are normatively evaluated.

Certainly, the latter distinction is a clear one. However, important questions remain. How does one attain the norms for proper justification or testing? On what basis are the norms themselves justified? Are the norms invoked really timeless? Or are they subject to historical change?

The following version of the DJ distinction is a methodological specification of the version just discussed.

Version 3

The analysis of discovery is *empirical,* whereas the analysis of justification or testing is *logical*.

In this version, the DJ distinction states a methodological difference on the meta-level, relative to an object-level of historical processes or justification procedures. All authors previously cited supporting version 2, also support version 3. The essence of version 3 is that descriptions have to be found empirically, whereas normative evaluations, i.e., whether or not some epistemic claims are justified, have to be carried out logically. As logic is a time-independent discipline, the justificatory procedures in this version are probably specified as not being subject to historical change; they are presumed to be timeless (unless the logical means employed for justification change over time).

The next version of the DJ distinction does not introduce substantial novelty, but maps the present distinction in academic fields.

Version 4

Within this version, the difference of history, psychology and sociology of science from philosophy of science is methodological: the former are empirical, the latter is logical. Empirical disciplines deal with the process of discovery, philosophy of science deals with the logical analysis of justification (testing) and is normative. Again, the quoted defenders of versions 2 and 3 also defend version 4. Although the methodological characterization of history, psychology and sociology of science as empirical is more or less unproblematic, it remains unclear, whether discovery processes (where they exist) can only be investigated empirically or not. Furthermore, the persuasiveness of the methodological characterization of philosophy of science as logical has waned substantially. Clearly, this characterization belongs to logical positivism and logical empiricism. By now, many philosophers consider these programs to be too restricted.

Version 5

Various authors introduce the DJ distinction as a distinction between different questions. However, they do not pay explicit attention to this fact most of the time. In order to promote the DJ distinction, they ask questions such as "What has happened historically during this discovery?" versus "Can a statement be justified? Is it testable?", insinuating that the reader realizes the difference between these questions (for a clear example of this way of introduction see, e.g., Popper 1959 [1934], p. 31).

In this version, the DJ distinction notes a difference between questions asked from the point of view of the meta-level. I shall discuss later whether or not the embodiment of the distinction into questions is significant.

Looking back at the different versions we note that version 1 of the DJ distinction operates on the object-level: it distinguishes different kinds of historical processes. Version 2, first reading, also operates on the object-level: it distinguishes discovery processes and historically used methods of justification. Version 2, second reading, mixes object-level and meta-level: it contrasts discovery processes with normative reconstructions of justification. Versions 3–5 operate fully on the meta-level: they distinguish different kinds of analyses, meta-disciplines, or questions.

3. SOME HIDDEN ASSUMPTIONS

In the literature, versions 1–4 are typically conflated, as if we were dealing with one homogeneous DJ distinction only. What is implied in the conflation of the versions 1–4 of the distinction? My assertion is that the conflation implicitly encloses substantial theories about discovery and about justification that the proponents of the conflated DJ distinction took more or less for granted. Let us look at the discovery and the justification sides in turn.

Discovery side: The characterization of the process of discovery as subject to empirical investigation only (by psychology, history, etc.), and thereby excluding philosophy from its analysis, implies that the process of discovery has no features that can be subjected to any sort of non-empirical analysis. In other words, there cannot be a "logic of discovery" or a "rational heuristics". This assumption was attacked especially by the protagonists of a so-called "logic of discovery" (Hanson 1971; Nickles 1980, pp. 22–25). The main argument is that there may indeed be structures of discovery that can be subjected to logical analysis; any assumption to the contrary is unfounded.

Justification side: The conflation of versions 1–4 implies that there are justificatory processes in science, and that the only admissible methods of justification (testing) are logical. Philosophy of science, as a discipline, investigates this sort of justification. This position clearly reflects the program of some representatives of logical positivism (or logical empiricism) that conceived philosophy as logical analysis of language. The justification (testing) of some propositions becomes the analysis of the logical relations between this proposition and other propositions, i.e., mainly basic (or protocol) sentences.

As much as this assumption has been taken for granted in (some parts of analytic) philosophy, it is by no means philosophically innocent. One of the important implications is the following. A disagreement about the justification of an item can arise (1) out of a disagreement about basic sentences, or (2) a disagreement about conventions, or (3) by error. A disagreement over basic sentences can either be settled or emerges from diverging conventions, depending on the supposed nature of basic sentences. Thus, all disagreements arise either out of error or by adoption of differing conventions. Disagreements that are rooted in different conventions are not epistemically substantial. All epistemically substantial disagreements, therefore, are caused by error: At least one of the disagreeing parties commits a mistake, because it is impossible for both parties to be right in the same instance. In other words, *a rational disagreement about justification* is conceptually impossible. Put in a different idiom, the justificatory part of science is a one-person-game. This implies that in this part of science, there is no fundamental epistemic role for scientific communities, as opposed to individually working scientists.

4. KUHN'S CRITICISM OF THE DJ DISTINCTION

Towards the end of the introduction to his *The Structure of Scientific Revolutions*, Thomas Kuhn reflects upon the content of his book that he had just summarized in the preceding paragraphs.

> "History [. . .] is a purely descriptive discipline. The theses [of *Structure*] are, however, often interpretive and sometimes normative. Again, many of my generalizations are about the sociology [. . .] of scientists; yet at least a few of my conclusions belong traditionally to logic or epistemology. [. . .] I may even seem to have violated the very influential contemporary distinction between the "context of discovery" and "the context of justification". Can anything more than profound confusion be indicated by this admixture of diverse fields and concerns?" (Kuhn 1970, pp. 8–9)

In this paragraph, Kuhn articulates what some readers may have felt, with increasing uneasiness, when following Kuhn's summary of *Structure*. Like many other paragraphs, he ends it by asking a rhetorical question that may indeed be some readers' real question. And of course, in the next paragraph Kuhn provides a negative answer to this question:

> "For many years, I took [this distinction and others] to be about the nature of knowledge [. . .]. Yet my attempts to apply them [. . .] to the actual situations in which knowledge is gained, accepted, and assimilated have made them seem extraordinarily problematic. Rather than being elementary logical or methodological distinctions, which would thus be prior to the analysis of scientific knowledge, they now seem integral parts of a traditional set of substantial answers to the very questions upon which they have been deployed. That circularity does not at all invalidate them. But it does make them parts of a theory and, by doing so, subjects them to the same scrutiny regularly applied to theories in other fields." (Kuhn 1970, p. 9)

This passage does not appear terribly clear to me.[1] But before going into details of analysis, I want to insert a somewhat personal note. When I started writing the first draft of my book on Kuhn's philosophy of science (Hoyningen-Huene 1993), my plan was to make this passage a central piece of my reconstruction of his theory. The reason was straightforward. Here, at the end of his introduction, at a very prominent place, Kuhn describes a central difference of his work to the preceding epistemological tradition. This tradition lasted at least from Kant right to his days; both the logical empiricists and Popper made fundamental use of the DJ distinction. It defined the working area of philosophy of science. Thus, Kuhn seemed to be helpful enough to tell his readers what his most profound deviation from the philosophical tradition was. Therefore, this appeared to be an ideal entry point to an understanding of the philosophical underpinnings of his much discussed but—at least it seemed to me—poorly understood theory.

When I told Kuhn about this plan in 1984, I was immensely surprised when he recommended that I should not focus on this passage, because it only constituted a "throw away remark". He told me that its insertion was suggested to him by his then Berkeley colleague Stanley Cavell, in order to deal with anticipated criticism by philosophers of science. So, in a sense he did not take these remarks very seriously himself, as not being very illuminative for what he was really after. As Kuhn's memories about details of the composition of *Structure* were, as he himself confessed repeatedly, not very reliable, it is worthwhile to look for confirming or disconfirming evidence for his story.

Kuhn had finished a first draft of *Structure* that may be called "*Proto-Structure*" in the fall or early winter of 1960 (Kuhn 1960).[2] It was mimeographed (the technical predecessor of Xeroxing) and distributed to some people, including Stanley Cavell, James Conant and Paul Feyerabend. The two paragraphs from which I quoted above are entirely missing from *Proto-Structure*. In their stead, there is the following note:

> [The final version will require one or two additional paragraphs at this point. They will describe footnote and bibliography policy, indicate the relation of this form of the monograph to its fuller version, justify the restriction to physical sciences, and attempt at least the most essential acknowledgments.] (Kuhn 1960, p. 10)

Obviously, Kuhn changed his mind about what should be inserted at this place, because what he mentions in this paragraph as required he finally dealt with in the Preface to *Structure* (there is no preface to *Proto-Structure*). In the preface, there is also an acknowledgment to Stanley Cavell that is noteworthy in our context. After gratefully noting the parallels of their views and their special mode of communication, he closes by stating that the latter "attests an understanding that has enabled him [Cavell] to point me the way through or around several major barriers encountered while preparing my first manuscript" (Kuhn 1970, xi). It is quite plausible that one of the barriers was the DJ distinction that appeared to forbid what Kuhn did in *Structure*, and that Cavell pointed him the way how to dismiss it.

Now let us see what Kuhn's criticism of the DJ distinction in *Structure* consists in. What does he mean by saying that the DJ distinction (and other similar distinctions) "[r]ather than being elementary logical or methodological distinctions [. . .] seem integral parts of a traditional set of substantial answers to the very questions upon which they have been deployed" (Kuhn 1970, p. 9)? This is a rather convoluted sentence that provokes the following queries. In which way can the distinctions mentioned be "deployed" upon questions? How can something that is part of an answer to a question be deployed to the very same question? What questions exactly is Kuhn talking about? These unanswered queries make the quoted sentence rather confusing. As there is no direct hint in the text on how to answer these queries, one has to make assumptions. My guess is that among the questions Kuhn mentions is the question "How does innovation occur in science?" When the standard DJ distinction (that conflates versions 1–4) is deployed to this question, the result looks as follows. First, the DJ distinction postulates that one should distinguish between a discovery part and a justification part of innovation. Then, the discovery part should be delegated to the empirical disciplines, whereas the justification part belongs to the business of philosophy of science that investigates it by logical analysis (remember the Kekulé case where all this seems perfectly plausible). And indeed, Kuhn's statement that this is not an application of "elementary logical or methodological distinctions", but rather an "integral part of a traditional set of substantial answers" is right because this procedure is a part and parcel of logical positivism/empiricism. Furthermore, he is right in claiming that this substantial answer is "part of a theory", and that this theory should be scrutinized. The theory he mentions is what I called, in the last section, the hidden assumptions built into the traditional (conflated) DJ distinction. This theory assumes that innovation is a two-step process containing discovery and justification phases, that discoveries have no structures that can be subjected to logical analysis, and that justifications can be fully understood by formal logical analysis.

As is often the case with the Kuhn of 1962, philosophically he is on the right track (or rather: on an interesting track!). However, to put it mildly, he is not very explicit (because he himself rather feels his way instead of having fully analyzed it). His pertinent sentences flow nicely and seem uncomplicated, but in truth they are opaque and, ironically, by their very stylistic qualities they are easy to misunderstand.

5. KUHN'S VIEW OF JUSTIFICATION IN SCIENCE

Kuhn's own view of justification is different from the logical positivist/empiricist picture. For them, justification ultimately uses as exclusive means formal logics and basic/protocol sentences. Before discussing Kuhn's view, we should note that Kuhn's explicit discussion of the DJ distinction and his deviating view takes place exclusively in the context of theory choice. Of course, there are other occasions in science were talk of justification is appropriate. Quite certainly, Kuhn would have similar views about some of them as the logical positivist/empiricists, say with respect to the justification of a certain mathematical approximation procedures. But in the case of the choice of general hypotheses, especially of theories, both the DJ distinction is most plausible and the contrast between Kuhn and the alternative philosophies is sharpest. For Kuhn, justification of theory choice in science uses means that, according to the standard DJ distinction, qualify as belonging to the context of discovery. Thus, for him the distinction must be invalid. I will not, however, pause on discussing and refuting the standard clichés about Kuhn, namely his alleged irrationalism, subjectivism, relativism, and so forth. Instead, I will directly present his position regarding theory choice justification.

What are those means, usually counted as belonging to the context of discovery that, according to Kuhn, are relevant in the context of justification? In the end, Kuhn claims, it is a set of communal cognitive values that determines the outcome of the theory choice situation (Hoyningen-Huene 1993, pp. 147–154, 239–245). Two things are particularly remarkable about this set of values. First, this set of values changes over time and it is specific for the community in question. Thus, one may characterize a specific scientific community by the epistemic values it is committed to. Indeed, it is one of the principal sociological means to characterize any communities or groups by the specific values (or norms) that hold for them. In other words, these characteristics of scientific communities are traditionally counted as sociological and thus, cannot belong, according to the standard DJ distinction, to the context of justification. Second, although the community as a whole may be characterized by these values, each individual member of this community will specifically shape these values, both with respect to their precise content and their mutual weight. But the important fact about these individual variations of the communal epistemic values is that these differences become unimportant when the theory choice situation comes to a close. This is, in fact, analytically true. The theory choice situation only comes to a close when a consensus of the community about the best theory is reached. But each individual member of the community evaluates the candidates according to his or her individually shaped value system. So, a consensus can only be reached if *in spite of these individual value differences*, agreement emerges about which theory is the best and should therefore be accepted (whatever "acceptance" really means for the individual researcher in terms of commitment). In spite of their judging from slightly different viewpoints, i.e., from slightly different value systems, almost all community members come to the same conclusion regarding the winning theory. This underscores that Kuhn's theory of justification is by no means psychological, but sociological.

But there is an obvious objection to this account. The objection states that Kuhn's account fails to really address the context of justification and instead, addresses a "context of decision", describing a factual decision process about theory acceptance by a scientific community (Siegel 1980b, pp. 370–371; Siegel 1980a, pp. 310–312). This objection, however, underestimates the thrust of Kuhn's account. Kuhn does not only intend to neutrally describe the actual decision procedures in science, but he also argues that this is a *justified* decision procedure, that this is the way that science *should* actually be done, or in other words, that the procedure is a *rational* one because there are *good reasons* for it. I have italicized some words that are often used by the proponents of the DJ distinction when arguing that something does indeed belong to the context of justification.

Why does Kuhn think that this sort of decision procedure is actually a rational one? What sort of rationality is pertinent here? We are dealing here with simple means-ends rationality. It is claimed that the decision procedure is rational because it is directed at the cognitive goals of science, that is roughly the invention and improvement of explanatory and often predictive theories. First, the epistemic values do indeed represent the goals of science. According to these values, theories should be accurate, internally and externally consistent, as simple as possible, fruitful, and they should have a broad scope of application. These virtues of theories may be condensed into the master value of high problem solving capacity. It is also possible to see these values as an operationalization of science's quest for truth, where the historical and communal specificity of these values reflects the particular epistemic situation of that community (Hoyningen-Huene 1992, pp. 496–499). Second, it must be shown that also the individual value differences that lead to disagreement in the phase of extraordinary science but disappear from the result of a communal theory choice, are rational means towards the cognitive goals of science. The main idea here is that these differences make a rational disagreement during the phase of extraordinary science possible. This disagreement is vital for the distribution of risk in a situation of epistemic uncertainty as no one knows, which candidate for paradigmatic theory will be successful. Thus, it is reasonable that different scientists try out a variety of possibilities in order for the community to have a wide spectrum of competing alternatives to explore (Hoyningen-Huene 1992, pp. 493–496). This closes the argument for the rationality of the theory choice decision procedure.

As it was demonstrated, Kuhn does indeed engage in questions of justification of theory choice. He does not simply describe the history of science, either particularities or generalizations about its course. But then the question arises in which sense exactly Kuhn opposes to the DJ distinction because he does engage in the discussion of the rationality of justification procedures as opposed to purely historical work. In fact, this question does not only belong to Kuhn philology but raises a broader and more important issue. What remains of the DJ distinction if one removes the conflations discussed in section 3 to which also Kuhn opposed? Is there some core of the distinction that has in fact survived the attacks of historically minded philosophers and that should survive them because its philosophical substance is important?

6. THE DJ DISTINCTION, REJUVENATED

Actually, I do believe that there is a core of the DJ distinction that has, to the best of my knowledge, never been attacked in the discussion about it. This core is distributed among the versions 2 and 5 that I discussed in section 2. What I have in mind is an abstract distinction between the factual on the one hand, and the normative or evaluative on the other hand. This is a distinction of two *perspectives* that can both be taken regarding scientific knowledge, especially epistemic claims (but also about claims of differing characteristics such as legal, moral or aesthetic claims). From the descriptive perspective, I am interested in facts that have happened, and their description. Among these facts may be, among other things, epistemic claims that were put forward in the history of science, that I may wish to describe. From the normative or evaluative perspective, I am interested in an evaluation of particular claims. In our case, epistemic claims, for instance for truth, or reproducibility, or intersubjective acceptability, or plausibility, and the like are pertinent. Epistemic norms (in contrast to, say, moral or aesthetic norms) govern this evaluation. By using epistemic norms we can evaluate particular epistemic claims according to their being justified or not.

A perspective is a particular way of looking at an object. A perspective is actively chosen by an epistemic subject; it is not simply given by the object in question. A given perspective singles out certain aspects of the object in question as important, at the expense of others. The choice of a perspective is well expressed by a question because questions underscore the activity on the part of the questioner. Thus, it is not accidental that the introduction of the two perspectives underlying the DJ distinction is often accomplished by posing the respective questions. As I have noted in section 2, version 5, many authors do indeed introduce the DJ distinction by posing questions that make aware of the factual and the normative or evaluative perspective and their difference.

In his arguments against the DJ distinction, Kuhn has never challenged the difference between the factual and the normative or evaluative perspectives; neither has Feyerabend nor any other critic. It also seems that the distinction between describing a claim and evaluating it is indeed very solid. But I should add immediately three remarks in order to prevent misunderstanding. First, the statement of the mere contrast between the descriptive and the normative or evaluative perspectives does not commit to any assumptions about the nature of facts or of descriptions, nor about the nature of norms or justification. It is only the difference between these perspectives that is abstractly stated, and not yet any content about what this difference more concretely consists in. Hence, by distinguishing the two perspectives we are not committed, for instance, to the position that norms and facts are absolutely separated and that they have nothing in common whatsoever. More to the point, it is emphatically not excluded that epistemic norms have factual presuppositions or implications, nor that facts have normative presuppositions or implications. Second, the question approach to the DJ distinction makes particularly clear that the overlap problem of the process distinction (version 1) is in fact a pseudo-problem. The reason is that different questions may receive the same answer without any danger of the two questions being confused with one another. In spite of getting the same answer, the questions "What is 5 plus 4?" and "What is 3 times 3?" remain distinct from one another. The same applies to a question that seeks a description and one that seeks a justification for some claim. Answers to these questions may be similar, even identical in some cases without blurring the difference between the questions at all. Third, the distinction between descriptive and normative or evaluative perspectives is not exhaustive. Other perspectives are also possible. In principle, there is space for those critics of the DJ distinction who claimed that it should be expanded to be threefold or even fourfold (see, e.g., Blackwell 1980; Curd 1980; Kordig 1978, pp. 114–116; Laudan 1977, pp. 108–114; McLaughlin 1982; Nickles 1980, pp. 18–22; Schaffner 1980, pp. 178–200).

If the DJ distinction is rejuvenated in the proposed way, one gets a distinction that is very lean. It is not loaded with potentially controversial philosophical theories about discovery or about justification, as the traditional conflated DJ distinction is. It is neither biased towards the program of logical empiricism, nor does it presuppose or imply controversial theories about the relationship between facts and norms. The lean distinction, thus, appears to be neutral and acceptable to everyone, at least to those who raised their voices in the extended discussion in the 1960s and 1970s. On the one hand, it should be acceptable to the critics of the traditional DJ distinction as they either attacked the process version of the DJ distinction or the conflation of different other versions of it. But the critics never doubted the difference between the descriptive and the normative or evaluative perspectives. On the other hand, it should be acceptable to the defenders of the DJ distinction because the rejuvenated version preserves the core of what they fought for: that there is a distinct normative perspective that aims at the evaluation of scientific claims. Perhaps an acceptance of the rejuvenated DJ distinction will put an end to statements of despair like the one by Herbert Feigl: "I confess I am dismayed by the amount of—it seems almost deliberate—misunderstanding and opposition to which this distinction has been subjected in recent years" (Feigl 1970, p. 4).

ACKNOWLEDGMENTS

I wish to thank all the other contributors to this volume, because they all helped improve the quality of this chapter—as far as this was possible. With respect to the English writing, I would like to thank Dr. Maya Shaha cordially for substantial improvements.

NOTES

1. This passage is not only not terribly clear; it was also shocking to some logical empiricists. For instance, Wesley Salmon described his reaction to this passage as follows: "On my first reading of Thomas S. Kuhn's *The Structure of Scientific Revolutions* (1962) I was so deeply shocked at his repudiation of the distinction between the context of discovery and the context of justification that I put the book down without finishing it" (Salmon 1991, p. 325). (I rediscovered this reference in chapter 11 a book manuscript by Hal Brown entitled *Conceptual Systems*).
2. This is what Kuhn wrote to me in a letter of 26 May 1994: a "draft that I had typed up, I should guess, in the fall or early winter of 1960". But now it seems improbable to me that Kuhn had completed *Proto-Structure* before Spring 1961. According to recent archival studies in the Harvard Archives (Driver-Linn 2003, pp. 272 fn. 2), on April 22, 1961, Kuhn had sent "a draft of the *Structure* manuscript", i.e., *Proto-Structure*, to James Conant, then President of Harvard University "with a letter inviting criticism and making an appeal for Conant's endorsement to a publisher". Why should Kuhn have waited for several months before sending *Proto-Structure* to Conant to invite his criticism, possibly to be incorporated into the final version of the book? In addition, the two letters that Feyerabend sent to Kuhn in response to receiving *Proto-Structure* were most probably written in Spring (or Summer) 1961 (Hoyningen-Huene 1995, pp. 353–354). Given Feyerabend's style of work and temperament, I consider it unlikely that he delayed his reaction to *Proto-Structure* for several months.

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