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Reason versus Ian Hacking's styles of scientific reasoning

La razón frente a los estilos de razonamiento científico de Ian Hacking

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ABSTRACT

There is a view in contemporary philosophy of science according to which scientific methodology itself is subject to radical change as part of scientific progress. According to this view, change in science is not confined to accepted theories. The core principles of scientific theory appraisal, including the rules and categories used to rank and confer truth-values on theories, are also said to be subject to radical change as science develops. In this paper, I examine Ian Hacking's version of this *no-invariant-methodology* thesis. I argue that, just like Thomas Kuhn's "paradigms," Larry Laudan's "research traditions," and Imre Lakatos' "research programmes," Hacking's "styles of reasoning" fail to give an adequate account of scientific progress.

Keywords: Styles of reasoning; subjectivity; objectivity; relativism; methodology

RESUMEN

En la filosofía contemporánea de la ciencia existe una opinión según la cual la propia metodología científica está sujeta a cambios radicales como parte del progreso científico. Según este punto de vista, el cambio en la ciencia no se limita a las teorías aceptadas. También se dice que los principios básicos de la evaluación de las teorías científicas, incluidas las reglas y categorías utilizadas para clasificar y otorgar valores de verdad a las teorías, están sujetos a cambios radicales a medida que la ciencia se desarrolla. En este artículo, examino la versión de Ian Hacking de esta tesis de la *metodología no invariable*. Sostengo que, al igual que los "paradigmas" de Thomas Kuhn, las "tradiciones de investigación" de Larry Laudan y los "programas de investigación" de Imre Lakatos, los "estilos de razonamiento" de Hacking no dan cuenta adecuada del progreso científico.

Palabras clave: Estilos de razonamiento; subjetividad; objetividad; relativismo; metodología

1. INTRODUCTION

According to Hacking (1975; 1980; 1982; 1983; 1985; 1996; 1999; 2012), there are different "styles of reasoning" in science. Styles of reasoning are categories of thought that confer truth values on propositions. It is because styles of reasoning exist that we are able to entertain propositions that are validated or substantiated by reason. Propositions which are true and those which are false, (according to Hacking) have no existence that is independent of the style of reason that underpins them. In other words, the truth or falsehood of propositions are dependent upon their underlying styles of reasoning. Styles of reasoning are historical in the sense that they vary from epoch to epoch as science progresses. Hence, for Hacking, it follows that, "we cannot reason as to whether alternative systems of reasoning are better or worse than ours, because the propositions to which we reason get their sense only from the method of reasoning employed" (Hacking, 1982, p. 65). Although Hacking maintains that his view does not lead to subjectivism, he accepts that his view does lead to some sort of relativism, which he deems unavoidable and acceptable.

But is "relativism" better than "subjectivism?" In addressing this question, this paper presents a critical exposition of Hacking's argument for changes in styles of scientific reasoning. I will query whether "styles of reasoning" provide an adequate explanation of scientific progress. Furthermore, because Hacking maintains that his view is substantiated by the history of science, I will analyze his leading example, the emergence of probability, and argue that this example in fact fails to justify his position.

2. METHODS

In an essay titled "Objectivity, Value Judgment, and Theory Choice", Thomas Kuhn (1977) identified five methods for the evaluation and assessment of empirical research. These are: accuracy, internal and external consistency, broadness of scope, simplicity, and fruitfulness. In this article I make use of two of Kuhn's methods (accuracy and consistency) in my assessment of Hacking's Styles of Scientific Reasoning. More specifically, I argue that: (a) Hacking's account of the emergence of probability is inaccurate; (b) some of his arguments are not internally consistent; and (c) that there are alternate accounts of the history of probability that fit the facts better than Hacking's account.

3. WHAT ARE STYLES OF SCIENTIFIC REASONING?

Hacking's (1980; 1982; 1983; 1985; 1996; 1999; 2012) idea of styles of reasoning is based on A. C. Crombie's (1980) *contextual approach* to the study of science, which starts from the rather obvious point that scientific development is not entirely autonomous but has at any rate some dependence on the philosophical ideas, technical equipment, and the social context of the scientists involved. According to Crombie (1980), the study of science must examine the manner or style in which scientific enquiry is executed:

> Styles of scientific thinking in Western intellectual history have been dominated and progressively diversified by the interaction of philosophical and practical programmes ... The active promotion and diversification of scientific methods of late medieval and early modern Europe reflected the general growth of a research mentality in European society, a mentality conditioned and increasingly committed by its circumstances to expect and to look actively for problems to formulate and solve, rather than for an accepted consensus without argument. The varieties of scientific methods so brought into play may be distinguished as:
>
> 1. the simple postulation established in the mathematical sciences,
> 2. the experimental exploration measurement of more complex observable relations,
> 3. the hypothetical construction of analogical models,

> 4. the ordering of variety by comparison and taxonomy,
> 5. the statistical analysis of regularities of populations and the calculus of probabilities, and
> 6. the historical derivation of genetic development.
>
> The first three of these methods concern essentially the science of individual regularities, and the second three the science of the regularities of population ordered in space and time (pp. 283-284).

Hacking (1985), however, goes on to make the stronger claim that new cannons of evidence and new methods of argumentation come into being with the emergence of a new style of reasoning, and he gives characterization of scientific change in terms of change of styles of reasoning.

Just like Kuhn's (1962) *paradigms*, Laudan's (1984, 1990) *research traditions*, and Lakatos' (1970) *research programmes*, a *style of scientific reasoning* is, according to Hacking, global or general, because it is broader than those *axiomatized sets of hypotheses* referred to, by the logical positivists, as *theories*. Indeed, a style of reasoning, according to Hacking (1985), is still more general than paradigms, research traditions, or research programmes. This is because we can have two or more paradigms, research traditions, et cetera, within one style of reasoning. Paradigm change, for instance, can refer to small scale changes like the discovery of x-rays or the fluid theory of electricity. But these paradigms may belong to the same style of reasoning (in this case, the "empirical sciences" style of reasoning). Furthermore, within the statistical style of reasoning, according to Hacking (1985), we have different paradigms such as the *Copernican Revolution* and the Mathematical paradigm. The empirical tradition in the Physical Sciences is closer to the large-scale activities Hacking (1985) has in mind when he talks of styles of reasoning. In short, one key difference between styles of reasoning, on the one hand, and paradigms, research traditions, research programmes, et cetera, on the other hand, is in the scale of generality.

Another important difference is in the manner in which styles are used to explain scientific progress. Long before Thomas Kuhn's (1962) account of scientific revolutions, positivists like Auguste Comte (1858; 1880) had believed in the steady accumulating progress of theoretical knowledge. Of course, Comte (1858; 1880) never thought of accumulation in the simplistic manner of an ever-uniform accumulation of knowledge, and therefore he distinguished three stages of knowledge: (a) the theological, (b) metaphysical, and (c) positive stages. For Comte (1858; 1880), it is only when we have separated the murk of theology and metaphysics from our posited knowledge claims that we can have an ever-growing stock-pile of positive knowledge. Positivists, in short, believed in the accumulation of truths about the world, and they characterized progress in terms of the growth of theoretical knowledge. Indeed, post-positivists philosophers like Popper (1965), Lakatos (1970), Toulmin (1953), and Laudan (1984) were also interested in correctly identifying and characterizing the growth of theoretical knowledge.

The Euclidean style of reasoning, Hacking claims, brought into being the new class of sentences called Euclidean geometry:

> Let us take the hard case of Euclidean geometry. It is common philosophical doctrine that empirical propositions, to be established from case to case by measurement, are to be distinguished from the *a priori* and demonstratable propositions of geometry. I assert that those very sentences used to express the geometrical *a priori* propositions could not have that sense unless they were embedded in the practice of geometrical demonstration. In fact... *hardly any* [emphasis added] of the propositions and theorems of Euclid were even uttered or thought of before there was the practice of proof. The emergence of a style of reasoning brings into being a new class of sentences whose sense is in part determined by the way in which we can reason for their truth or falsehood. (Hacking, 1983, p. 457.)

Hacking's claim that, although the propositions of Euclidean geometry are "demonstratable *a priori,*" these propositions were not conceived of before the Euclidean practice of proof, is highly controversial. For if the Euclidean style of reasoning is historical in the sense that it came into being at a specific point in time, it must be a contingent or empirical event! But if the emergence of the Euclidean style is a *posteriori,* and the truth-values of the propositions of Euclidean geometry are inseparable from the Euclidean style of reasoning, how can those same propositions be *a priori if* they are inseparable from an *a posteriori* style? Hacking (1983), perhaps sensing this apparent inconsistency, in fact talks rather vaguely of "... a style of reasoning... ***in part*** [emphasis added] determining... the way in which we reason for [the] truth or falsehood [of sentence]" (p.457.); and that "... ***hardly any*** [emphasis added] of the propositions and theorems of Euclid were even uttered or thought of before there was the [Euclidean] practice of proof". (p.457.)

But if Hacking's view is to be of any use in theory choice and theory acceptance, we need to know those parts of true or false Euclidian propositions that are not determined by that style of reasoning. We would also need a criterion (or a set of criteria) based on which we can separate those parts of the senses of true or false propositions that are determined by styles from those parts that are not.

Of course, Hacking (1985) suggests that it is not only the availability of styles of reasoning that is important in conferring truth-values to sentences and propositions. He concedes that low-level observation statements also play important roles in determining truth-values. Therefore, he claims that: "It is not the case that nothing's either true or false but thinking makes it so. Plenty of things we say need no reason. That is the core of the discredited philosophical doctrine of observation sentences..." (p. 49). Low-level observation statements, however, play a very trivial role in determining the truth-value of propositions; and Hacking (1985) does not give a detailed account of the role of low-level observation statements in determining the truth-values of propositions and sentences. In any case, the importance of low-level observation statements in conferring truth-values does not resolve the apparent inconsistency in Hacking's account of Euclidean geometry. For the role of low-level observation statements in conferring truth-values still does not explain how a set of propositions can be *a priori* if "they do not have an independent or prior existence" (Hacking, 1982 p. 457) from an historical or a *posterior* style.

Although the term reason and/or argument is often used to refer to deductive and inductive logic, Hacking's (1985) style of reasoning or styles of argumentation are completely different from logic—deductive or inductive. This is because (according to Hacking, 1985) logic deals with the preservation and transference of truth, while styles introduce *the possibilities of truth*. Deductive logic, for instance, describes how truth is transferred from the premises to the conclusions of valid arguments. Deduction does not create the truth; it only transfers the truth that is/are already in-built into the premises. One major approach to inductive logic also deals with how truth can be transferred from premises to conclusion most of the time. But styles of reasoning do not describe how truth is transmitted between sentences; they introduce new candidates for truth or falsehood. Simply put, Hacking's claim is that the use of deduction and induction in human reasoning require the availability of underpinning styles.

Hacking (1985) also distinguishes his styles of reasoning from Quine's (1948) "Conceptual schemes". Although Quine's "conceptual schemes" are schemes of sentences held to be true and Hacking describes his styles in terms of *possibilities of truth*, Quine's (1948) schemes are schemes of true sentences; but Hacking's schemes of possibilities are not. For unlike Quine's (1948) conceptual schemes which are merely schemes of sentences held to be true, Hacking's styles determine classes of sentences that have *possible* truth-values. Styles of reasoning deal, therefore, with truth-or-falsehood. They do not determine the definite truth-values of sentences; they only create the possibility to reason or think in the direction of certain kinds of propositions.

The implication of this idea is that two different conceptual schemes, in Quine's sense of the term, may belong to the same style of reasoning. For in Quine's view, two schemes differ when a substantial number of the core sentences of one scheme are not held to be true in the other scheme. But this does not need to be so in styles. People agreeing to the same style may disagree on the truth-value of sentences; one person may hold as true what another person rejects. This is because styles merely determine possible (or conceivable) truth-values; they do not determine the specific truth-values (T or F) of sentences.

Hacking (1982) further claims that a proposition which is a candidate for being true-on-false in one era may be a proposition which cannot even be entertained in another:

> ...many categories of possibilities, of what may be true or false, are contingent upon historical events, namely the development of certain styles of reasoning...we cannot reason as to whether alternative systems of reasoning are better or worse than ours because the propositions to which we reason get their sense only from the method of reasoning employed. The propositions have no existence independently of the ways of reasoning towards them (p. 65).

Since styles of reasoning are historical in the sense that "they emerge at definite points and ...die out," (Hacking, 1983, p. 162). Hacking claims that new methods of reasoning are brought into play by the emergence of new style, and that discontinuities occur in our criteria of scientific merit, or in the methodology of science. One of Hacking's most developed examples of the emergence of a style of reasoning (and of a new method of validation in science) is the emergence of probability.

4. HACKING ON THE EMERGENCE OF PROBABILITY

Although gambling had existed for hundreds of years, Hacking (1983; 1985) claims that the idea of probability as a mathematical conceptualization of chance occurrence did not emerge until mid-seventeenth century. The emergence of probability, according to Hacking, was the result of a *mutation* which occurred to the concepts of *sign and evidence* around 1660. Hacking's account of the emergence of probability goes as follows:

There are two fundamental aspects of the modern ideas of probability. On the other hand, there is the epistemological aspect concerned with the degree of belief warranted by evidence, and, on the other hand, there is the statistical aspect which is concerned with the stable frequency of chance events. According to Hacking (1983; 1985; 2012), there was no real concept of probability in use until around 1660 when scholars suddenly began to develop systematic theories about random phenomena. First, in 1654 Pascal solved a problem put to him by a gambler. The problem was the following: in a throw of three dice, although the numbers 9 and 12 can be obtained in as many combinations as the numbers 10 and 11, the experienced gambler knows that 10 and 11 are more advantageous than 9 and 12. Why? Pascal was able to solve this aleatoric or statistical problem of probability.

It was also around this time that Pascal first applied probabilistic reasoning to decision making in the famous Pascal's (1852) wager, thereby addressing the epistemological aspect of probability. (Although this problem was solved around 1654, Pascal's writings were published posthumously in 1852.) Furthermore, a book on probability was published in 1660 by Christian Huygens, titled, *De Ratiociniis in Aleae Ludo*, which discussed dice games. Also, it was during this period that Leibniz (see Schneider, 1981) began applying metrical probabilities to legal issues. In London as well, John Graunt (1662) published a set of statistical inferences on the basis of mortality records. Simply put, according to Hacking (1975), around 1650, there was a sudden surge in the conceptualization of probability as both epistemological and statistical. Scholars suddenly began to analyze and examine various types of chance phenomena, and to analyze and examine the degrees of

evidential support we can have for beliefs and actions in situations of uncertainty. Pascal (1852) addressed both aleatoric and epistemological problems. Leibniz (see Schneider 1981) was primarily interested in epistemological issues but, immediately, saw the similarity between his own work and the statistical/mathematical issues addressed by Pascal (1852) and Huygens (1660).

Hacking (1975) claims that it is a conceptual archaeological approach to the history of probability that can fully account for the sudden emergence of the Janus-faced ideas of probability. Conceptual archaeology upholds the autonomy of concepts, and maintains that current conceptual frameworks are preconditioned by previous ones. In his words:

> We should imagine that concepts are less subject to our decisions than a positivist would think, and that they play out their lives in, as it were, a space of their own. If a concept is introduced by some striking mutation, as is the case with probability, there may be some specific preconditions for the event that determine the possible future courses of development for the concept. All those who subsequently employ the concept use it within this matrix of possibilities. Whatever the overall value of this *strange model* [emphasis added] in the history of ideas, we can at least agree that since 1660 the concept of probability has been curiously autonomous and steadfast to its origins. (p. 15.)

In short, for Hacking, new concepts evolve from old ones in a mysterious way, which he describes as a *striking mutation*. The strange model that gives rise to this mutation has a lot in common with the philosophy of Popper (1965), who like Hacking, maintains that theories, concepts, (or if you like, forms), have some sort of objective metaphysical existence once invented. This strange model also has an acknowledged Hegelian heritage, which is located in the thesis that *our current conceptual spaces are preconditioned by previous ones*. But, since the previous concepts that condition or present ones may have been long forgotten, Hacking (1975) "suggests that we were in the grip of darker powers than are admitted into the positivist ontology," (p. 15).

Hacking's (1983) claim is that although gambling and dicing are indeed very old, and although there might have been some systematic theoretical exercises in both gambling and dicing (for instance, in India as he notes), these exercises were in fact not genuinely probabilistic. For probability, as we understand it, did not come into being until around 1660 when various thinkers hit upon the concept of probability as epistemic and statistical. For Hacking, the new method of probabilistic reasoning that emerged was the result of the advent of new concept of evidence.

Thinkers all throughout the Middle Ages made a sharp distinction between knowledge and *opinio*. Knowledge was about universal truths, and it was arrived at by demonstration. The set of doctrines and beliefs that were not derived from demonstration, or those propositions that were indemonstrable, were regarded as *opinio*. The word probability was initially associated with *opinio*. In those pre-1660 years, probability (and probabilism) connoted approval by authority in general, and celestial authority. *Opinio* was probable neither because of good reasons nor because of the degree of evidential support; it was the approval or acceptability of intelligent people, or the testimony of respected judges that conferred probability. Just after the Renaissance, however, the idea of inductive evidence evolved from the concept of signs used in the low sciences of alchemy and medicine. Because these low sciences aimed at *opinion* rather than demonstrable and universal knowledge, they relied on signs rather than ecclesiastical authority in accrediting the degree of acceptability of *opinio*. Signs, therefore, furnished *opinion* its testimony. But signs do not in themselves determine the degree of credibility of opinion. The credibility or probability of *opinio* was judged by the relative frequency of the sign in indicating or suggesting the *opinion* in questions. In short:

> It is here that we find the old notion of probability as testimony conjoined with that of frequency...to call something probable was...to invite the recitation of authority. But: since the authority was founded on natural signs, it is usually of a sort that was only 'often to be trusted'... Thus, the connection of probability, namely testimony, with stable law-like frequencies is a result of the way in which the new concept of internal evidence came into being. (Hacking, 1975, pp. 43-44)

Schneider (1980), argues that an ancient idea of probability had been in existence since the time of Aristotle. This ancient idea of probability is to be found in the Aristotelian term, "endoxos." The use of the term endoxos as probability can be found in some well-known Aristotelian texts. Endoxos pertained to a belief in relation to the state of information possessed by the believer. For the Aristotelians, endoxos played a very important role in dialectical reasoning, and in political and juristic debate. According to Schneider (1980), the probabilistic use of this concept is more clearly exhibited in the skeptical traditions of Sextus Empircus (see Floridi, 2002) and Carneades (see Obdrzalek, 2006). For these skeptics distinguished between three levels of the probable in practical everyday life affairs, and they assigned different levels of skepticism to these practical life decisions based on their probabilities. In short, Schneider's (1980) claim is that although the concept of probability had been in existence since the time of Aristotle, the concept was not applied to games of chance until the 17th century. During the time of Aristotle and the skeptics, the concept was applied into practical everyday life affairs; such as in dialectical debate, politics, and judicial decision making. According to Schneider (1980), it was the need for trading and training in commercial and economic affairs that engendered the connection between *endoxos* (or probability) and games of chance.

But in his response to Schneider's (1980) argument, Hacking argues that he never claimed that there was no concept of probability in existence before the 17th century. He maintains that his claim is that it was around the 17th century that our modern-day idea of probability as both epistemological and statistical emerged. But this does not genuinely describe the story Hacking (1975) tells in the *Emergence*. For *Emergence* as Hacking (1975) uses it does not mean "coming into being" as one would have expected. In fact, what Hacking's claim amounts to is the claim that probability, as we now understand it, became a public or common idea among intellectuals; intellectuals and scholars suddenly begun to study and write about probability. As Hacking (1980) puts it:

> My claim... [is] that there was a sudden eruption of probability lore around 1660, and no such lore in the preceding epoch, and that the way that probability erupted has to do with a transition of Renaissance ideas, chiefly connected with...the low sciences... it is...consistent with that thesis that there should have been some talk a millennium earlier, talk which was noted by the more scholarly seventeenth century authors (p. 107-108)

But this seems to give the rhetoric away! If Hacking's claim of emergence is merely the claim that probability suddenly became a subject of public discourse, then the claim that a new style of reasoning was brought into being becomes unfounded. Surely no one (except Hacking?) can seriously suppose that the possibility of the truth of a sentence that is true and that the possibility of the falsehood of a sentence that is false, depends on whether there is widespread discussion on these sentences! It is one thing to claim that different groups of people order their experiences by different concepts, and that the truth or falsehood of these experiences differ from group to group (that is conceptual relativism); but it is another thing to claim that until the ordering of a set of concepts become an object of public discourse in the intellectual realm, these concepts have no possible truth value. The latter seems to be Hacking's claim. Truth and

reason, on Hacking's account, are no longer relative to conceptual schemes; their relativity is now determined by whether they are objects of public discourse. This seems to be the implication of Hacking's thesis.

Indeed Hacking (1980) seems to imply that his account of the emergence is a tale which he expects no one to take seriously:

> No one will suppose that my simplistic picture [of the emergence of probability] can possibly be correct: human knowledge is too complex a web for that. One point of enunciating a simplistic sketch is to make assertions that might be false. Thus I claim that a certain idea of probability occurs in any number of sentences written after 1650, or so, but not before. (p. 107)

This jokey attitude to the arch example of the emergence of a style of reasoning suggests that one should rather place his bets on an alternative account of probability.

Hacking has placed the birth of probability at around 1650. F.N. David (1970) disagrees with this date. She puts the date back a century-and-a-half. According to David, it was in 1526 that "Cardano did...as far as we know, calculate the first probability by theoretical argument, and in so doing he is the real begetter of modern probability theory" (p. 13).

In short, Hacking's (1975) dating of the birth of probability at around 1660 is controversial. Just as Hacking can see the makings of probability in the mid-seventeenth century, so can others find legitimate grounds for setting the birth of probability theory at an earlier date. But as Howson (1978) rightly observed:

> If we are prepared to put back the birth of the theory from 1660 to the early sixteenth century [David does]...and ask...the...question 'why then?' the answer becomes obvious: those first exercises in probability theory are just part of the European Scientific and mathematical renaissance, developing in *pari passu* with the exploitation and augmentation of the legacy of Greek and Arab mathematics (p. 275).

Simply put, the fact that "there was a sudden eruption of probability lore around 1660, and no such lore in the preceding epoch" (as Hacking, 1975 maintains) is not enough reason to show that a new mode of reasoning came into being. For all the necessary ingredients for making calculations on chance phenomena were there long before 1660. This is because probabilistic reasoning (which directs thought at chance occurrence with the aid of numbers, calculations, and logic), is nothing more than an augmentation of the European mathematical tradition.

To claim that probabilistic reasoning is a result of an augmentation of European Mathematics is more plausible than Hacking's claim that the emergence of probability is the coming into being of a new style of reasoning. On this alternative account, "emergence" is nothing more than that: the exposition or revealing of that which had hitherto been concealed (or undiscovered). All the ingredients for the making of the first correct calculations in probability had been there all along, but as far as we know, no one latched onto the correct calculations until the sixteenth or seventeenth century.

This appears to fit the situation of the development of probability like a glove. For as Hacking himself observes, before the 17the century, division, and combinatorial problems about games of chance were addressed by thinkers. Thus Hacking (1975) quotes Luca Pacioli witting in 1494 as follows:

> A team plays ball in such a way that a total of 60 points is required to win the game, and each goal counts 10 points. The stakes are 10 ducats. By some incident they cannot finish the game, and one side has 50 points and the other 20. One wants to know what share of the money belongs to either side (p. 50).

It is merely because no one was able to solve problems of this kind that Hacking claims that probability came into being only around 1660 when Pascal was first able to supply the right answers to problems of this kind in mathematical fashion. But if one is prepared to admit that these earlier efforts at solving combinatorial and division problems about games of chance employed mathematical reasoning then it becomes obvious that probability theory itself is an aspect of the development and extension of the European scientific and mathematical tradition.

Hence, the "emergence" or probability does not show that there was new change in methodology. What occurred around 1660 was merely an extension of the European mathematically tradition to a new class of things; namely, to chance phenomena.

5. REASON AND THE RATIONALITY OF SCIENTIFIC CHANGE

Hacking's (1982) account of the emergence of probability is an attempt to illustrate how a "variety of scientific method" (p.445) came into being. The term methodology is very elastic, and he seems to have stretched it further by claiming that the methods of science change in as much as styles of reasoning evolve. We normally distinguish between two senses of methodology. In one sense of the word, methodology refers to the formal and basic principles of rationality (Worrall 1988; Worrall 1990). In this sense of the word, methodology includes the principles of deductive and inductive logic.

The other sense of *methodology* is that in which it refers to those descriptive/heuristics principles of theory appraisal; especially those more specific principles of how to do better science which are upshots of the actual practice of science. For instance, principles like clinical trials should be performed double blind. In these two senses of the term methodology, the overriding importance of deduction and induction is accepted, although Hacking (1982) denies the overriding importance of deduction and induction in scientific method. His claim is that the operations we are capable of performing with deduction and induction follow upon the emergence of styles of reasoning. Therefore, he insists that styles of reasoning are not to be identified with logic-deductive or inductive. Thus, he claims that:

> Deduction and induction were important human discoveries. But they play little role in scientific methods, no more than the once revered syllogism. They are devices for jumping from truth to truth. Not only will they give us no original truth from which to jump, but they take for granted the class of sentences that assert possibilities of truth or falsehood (p. 57).

Hacking is not just advancing the claim that proponents of two different styles cannot understand each other. What he is claiming is that to grasp the sense of a proposition, we would have to grasp the styles of reasoning relating to it. For a 15th century Parcelsian to understand the propositions of a 20th century Einsteinian, and *vice versa*, they would both have to grasps the senses of the adequacy of either of these styles independently of the individual styles. We cannot legitimately say that one style is better than the other, because "the styles of reasoning that we employ determine what counts as objective...the very candidates for truth of falsehood have no existence independent of the styles of reasoning that settle what is to be true or false in their domain" (Hackings, 1982, pp. 49). Additionally,

> ...the propositions that are objectively found to be true are determined as true by styles of reasoning for which in principle there can be no external justification. A justification can be an independent way of showing that the styles get at the truth, but there is no characterization of the true over and above what is reached by the style of reasoning itself (pp. 49-65).

For Hacking (1982), the rationality of a style or method of reasoning is relative to that style or method. No style or method is objectively better than another; and neither can we evaluate changes from style to style as rational because there is no characterization of rationality and truth independently of the stipulation of particular styles. The only judgement we can offer is about our present styles or methods:

> The sense of a proposition *p*, the way in which it points to truth or falsehood, hinges on the style of reasoning appropriate to *p*. Hence, we cannot criticize that style of reasoning, as a way of getting to *p*, or to not –*p*, because *p* simply is that proposition whose truth value is determined in this way (p. 49).

Obviously, this is relativism.

Hacking (1982) implies that this form of relativism is acceptable, and supposes that if we, carefully, distinguish between subjectivism and relativism, then the sort of relativism he advocates is acceptable. Thus, he claims that although his view of scientific change accepts relativism it, nonetheless, belongs to the objectivist's tradition. In traditional philosophy of science, objectivists are those who maintain that the development of science has been a predominantly rational affair. This is because on the objectivists' view, most scientific judgements about the merits of competing theories are said to be dictated by *reason*. On this view, change in science is regarded as a rational affair, because considerations such as simplicity, consistency, empirical success, etc., as opposed to personal idiosyncratic considerations, are those that are said to govern scientific development.

Those who reject the objectivist-rationalist position are regarded as subjectivists. Since choice, for the subjectivists, is not dictated by objective considerations, but by personal idiosyncratic factors, objectivist regard them as relativists.

But Ian Hacking (1982) maintains that although his view of scientific change does not lead to subjectivism, it is both objectivist and relativist:

> This chain of reflections does not lead to subjectivism. It does not imply that some propositions, with a content independent of reasoning, could be held to be true, or to be false, according to the mode of reasoning we adopt. Yet this defeat of subjectivism seems hollow because the propositions that are found to be true are determined as true by styles of reasoning for which in principle there can be no external justification. A justification would be an independent way of showing that the style gets at the truth, but there is no characterization of the truth over and above what is reached by the style of reason itself (p. 65).

As with most philosophical terms, the terms objectivism and subjectivism have many uses. In one of their uses, "subjective" refers to matters of taste and emotions. In this sense of the term, to characterize something as subjective is to refer to states that are internal and personal to individuals. For instance, after taking certain medications, I may claim to have sensations of sweetness, which upon taking the same medications another person claims to have a bitter sensation. Subjectively, our different characterizations of our internal states are *sui generic*. The contents of both claims merely express sensations concerning respective internal states. What it

shows is that we must distinguish the sensations we experience in our individual internal states from the judgments we make concerning these states. "Subjectivity", in this sense of the term, is not judgmental; it merely picks our descriptions of internal states. Let us call this subjectivity (1)**.**

The sense of *objectivity* that is incommensurable to this sense of *subjectivity (1),* refers to things that are external to the mind in the sense that they can be empirically (or inter-subjectively) verified. Let us call these uses of these terms' objectivity **(1)** and subjectivity (1), respectively.

The second use of the objective-subjective distinction is, essentially, judgmental. *Subjectivity*, in this sense, refers to judgements that are colored by bias and personal likes or dislikes. To be *subjective* in this sense, is to allow prejudice to dictate or influence choice. Choices that are objectively dictated are those that are based on good reasons; reasons such as empirical adequacy, weight of evidence, consistency, and empirical success. Let us call these two senses of the distinction objectivity **(2)** and subjectivity **(2)**.

In traditional philosophy of science, the dispute between those who maintain that the development of science has been a predominantly rational affair, (hence, objective), and those who maintain that it is not, has been about the objectivity **(2)**–subjectivity **(2)** distinction. The issue concerning the rationality of scientific change is not about objectivity **(1)**–subjectivity (1). The dispute between rationalists and relativists is about the extent to which *good reasons* dictate choice in the development of science.

Hacking (1982) seems to have overlooked these distinctions, and in fact, when he claims that he is not a subjectivist, he is, indeed, talking about what I call subjectivity (1)**.** This is what he says "This chain does not lead to subjectivity. *It does not imply that some propositions, with a content independent of reasoning could be held to be true, or to be false*..." (p. 65.)

It is in the objectivity **(1)**–subjectivity (1) distinction that subjectivity is independent of reasoning. It is because Hacking's view does not reduce scientific claims to claims of subjectivity (1), that it is objective in the sense of objectivity **(1).**

But the issue concerning the rationality of revolutionary scientific change, as I understand it, was never about objectivity **(1)** –subjectivity (1). The rationalist never claimed that reasons in the form of arguments and evidence had no roles to play in the subjectivists **(2)**-relativists position. Rather, the claim is that such reasons are not good reasons; hence, they cannot provide adequate explanations of the development of science. The objectivists **(2)** (rationalists) do not deny the claim that, as purely empirical fact, there are no scientific judgements about the merit of competing theories that were governed by subjective **(1)** and subjective **(2)** considerations! What the rationalist claims is that such situations in the history of science do not constitute *scientific progress*. When theory choice is not governed by good reasons, change is not based on the idea of "scientific progress." Furthermore, the objectivist **(2)**-rationalist does not deny that some of the idiosyncratic considerations that blind choice in such situations cannot (or could not) have been (erroneously) regarded as prima facie good reasons. The objectivist **(2)**-rationalist, however, insists that careful analysis will reveal those considerations as *unfounded* and *unacceptable*.

The objectivist **(2)**-rationalist relies heavily on logic. But Hacking denies the overriding importance of deductive and inductive reasoning in scientific method. Therefore, he insists that the operations we can perform with deduction and induction follow upon the emergence of styles of reasoning. Hacking's claim seems to be that styles are more important, (or more fundamental), than deduction and induction in the sense that styles must be established before we can apply deduction and induction in reasoning. I suggest that this puts the cart before the horse. It is

impossible to understand how we are to direct reason in the establishment (or emergence) of candidates for truth-or-falsehood if the principles of deduction and induction are not yet established.

Put differently, any "style of reasoning" in its capacity as a style (i.e. that which creates the possibilities of truth-or-falsehood), has to be consistent, coherent, well-grounded (in some sense), and must as such have a discernable chain of reasoning connection that makes it cognitively usable to start with. In which case, every "style of reasoning" must itself be founded upon a logical reasoning criterion of well-foundedness. Consider again, "probability," Hacking's pet example of a "new" and "emerging" style. Duality, opinion, evidence, signs, calculations, ratio, etc., are all reasoning tools that have meaning independently of "probabilities." Even though these concepts are integral to the new style of probabilistic reasoning, they are also elements of human cognitive reasoning that are usable and appreciable in any style of reasoning. I challenge Ian Hacking to give an example of one "style" in any field of human cognitive "reasoning" in which these elements are totally redundant! Different fields may emphasize different cognitive properties, but they all require these categories of human thought processing.

Hacking may have been misled into supposing that we can formulate styles of reasoning (i.e. that we can conceive of categories for the possibility of true or false propositions) independently of deduction and induction because he takes these two modes of reasoning (deduction and induction) in their formal logic definitions only. This, perhaps, is why he claims that: "We... understand deduction as that mode of inference that preserves truth. It cannot pass from true premises to a false conclusion...induction is that mode of argument that preserves truth most of the time" (Hacking, 1982, p.57).

Of course, Hacking's (1983) description of deduction and induction as modes for the transference of truth in inference is correct. What he overlooks, however, is that above all, they are the core aspects of human cognition, rationality, and reasoning. Even if we are not in the business of characterizing or identifying what is "true" or the "false", we still must employ these modes of reasoning. This is because deduction and induction as modes of thought contain within themselves, certain principles which must be followed if we are to be reasonable. Hence, in as much as *styles* are forms of reasoning, they must employ some of the principles of logic—principles such as those of coherence and non-contradiction.

6. CONCLUSION

The objective-rationalist tries to explain developments in science by showing how changes are governed by objective factors. As the objectivist-rationalist uses objectivity in its objectivist (**2**) sense, Hacking's changing styles of scientific reasoning cannot give an adequate explanation of scientific change. On his view, we cannot say that one style of reasoning is better than the other; and neither can we evaluate changes from style to style as rational. The point then is that his view of scientific change is not objective in any interesting way; for what he ends up with is an irrational form of relativism, which plays no helpful part in issues concerning the development science.

Of course, Hacking could object that objectivity (**2**) and subjectivity (**2**) are deeply embedded into my own "style of reasoning" and that they do not function in his own "style of reasoning." As such, I cannot use the standards of my own "style" to critique his own style. This may very well be true! We may have an incommensurability of styles.

At any rate, Hacking's changing styles of scientific reasoning provides no viable challenges to the traditional view that the methodological rules used in science have remained invariant throughout its development.

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