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## Rescuing Hempel From His World

### ABSTRACT

This paper makes the case for the relevance of C. G. Hempel's 1942 proposal of the usage of «covering laws» in History. To do so, it argues that such a proposal reflects how 18 and 19<sup>th</sup> centuries «philosophy of History» became methods or epistemology of History. This carried a change in meaning of «History»: no longer a succession of past events but the study of documented human action (including of scientific kind in general), its distinction *vis-à-vis* philosophy, sociology etc., becomes a minor matter as far as logic of research is concerned. Also present in this paper is the conception of theory as a conceptual mode of narrative, and the defense of a development of theories alongside their practice, not apart from them. Authors considered besides Hempel range from Max Weber to Sigmund Freud, from Arthur C. Danto to Albert O. Hirschmann.

Except for a few desultory efforts to account for paradigm shifts, or in what Foucault designates *enoncés*, I can think of very little in the philosophy of History from the middle-1960's to the present. Somewhere someone in the last decade must have written about explanation, even about historical explanation – but I cannot think of an example offhand. (It should be noted that, as editor of the *Journal of Philosophy*, I see a fair sample each year of what philosophers offer as their most advanced work: my estimate is that a contribution on any aspect of the philosophy of history occurs at a rate of one per thousand submissions.) It is not just that the topic is under extreme neglect. It is, rather, that there is hardly room in the present scene of philosophy for discussion of its issues. So to find someone actively working at them would be almost as encounter a historically displaced person, like someone doing abstract expressionist canvasses as if the whole subsequent history of art had not taken place. Or like encountering Japanese soldiers on some obscure atoll who never found out that the war had ended. Arthur C. Danto (in Ankersmit and Kellner 1995: 72/3)

### 0. Introduction

I indulged myself in such a long quote at the beginning of this paper because I do agree with Danto in what concerns the historical displacement of philosophy of history, although for different reasons which I'll try to make clear at once (ck. 1). However, and disregarding a resurgence of the topic in recent years because I think its due mostly to unscientific or un-philosophical viewpoints (on the 11<sup>th</sup> of September, 2001, for instance; of course I admit that re-editing Collongwood and others is also relevant), I will contend that Danto's dismissal of Hempel's argument in «The Function of General laws in History» (Hempel 1942) is erroneous. I will take that essay in consideration and argue that the conception of history in it addresses the whole of social sciences (and, from a logical viewpoint the whole of science) and that, therefore, the lost of relevance of philosophy of history is due to its transformation in

epistemology or theory of history, a change to which Hempel's model can be very useful despite its philosophical outlook (the analytic philosophy of history and its language, so much in focus in Danto's perspective) are abandoned; to do so, I will call upon essays by Max Weber (ck. 1) and Sigmund Freud (ck. 2).

I will also consider another, more recent, essay of Hempel related to the same subject and emphasize my point, recurring to a work by Hirschmann and presenting my own example of the kind of research in many different areas of knowledge that one can develop using this theoretical model (ck. 3). This will lead to my conclusion that is not so much the language deployed in the research as the plasticity of theoretical model used in it, its own narrative logic and its closeness to research that justify a rereading of Hempel.

### **1. Hempel in his world: language and social sciences**

In his paper on «The Decline and Fall of the Analytical Philosophy of History» (in Ankersit and Kellner 1995: 70-85) Arthur Danto concludes by saying that «Hempel's theory in fact strikes me still as true. It just stopped being relevant, the way the whole philosophy of history it defined stopped being. It was replaced with a different set of questions, a world in effect, into which it no longer fit. As with the questions of scholasticism which were never answered but merely abandoned, it belongs to the history of philosophy to summon up enough of the world of 1942 philosophy to see what Hempel's theory meant.» (p. 85) A few lines below, Danto does recognize that «a lot of course goes unaffected by the change, and there are many beliefs invariant to the two worlds», but that does not stop him from maintaining that Kuhn's world and its language did replace Hempel's one, i.e., that the analytical philosophy of history has been replaced by an historical narrative of scientific knowledge. Therefore, narrative replaced Hempel's conceptual, or logical, philosophical outlook. Thus, Danto ends his essay as follows:

In any case, since points of view are essential to worlds *of* individuals but have no place in the natural sciences (henceforward defined by that fact), historical explanation – dealing as it must with points of view – will be different from explanations in the natural sciences, not in terms that the latter entail covering laws and former do not, but in the kinds of law they respectively entail. But I as yet have too shallow a philosophical understanding of points of view to go further than this here. What I can say is that since points of view are historically indexed, since, that is, the worlds of historical beings are penetrated by their historical locations, the *new* philosophy of history is in effect a new understanding of ourselves as through and through historical. (Idem)

So, Kuhn did replace Hempel<sup>1</sup>, and despite Danto's recognition of the validity of referring to covering laws this change brings with – to Danto at least – a return to the 19<sup>th</sup> century: Geisteswissenschaft vs. Naturwissenschaft, Dilthey-style.

I want to draw attention to two points: one, that such a strict separation is not necessary and a consideration of Hempel's 1942 model on its own merits rather than in the merits of the analytical language of its world will show that, and that «a philosophical understanding of points of view» must not be reduced to the history of philosophy (specially in the 19<sup>th</sup> and 20<sup>th</sup> centuries), under penalty of losing many valuable sources, in this case Max Weber's essays in methodology of science.

Danto's reference to the idea of «Verstehen» (ck. pp. 79-81) is in fact appropriate when addressing Hempel's 1942 essay. In fact, it is somewhat strange that Hempel does not mention it at all despite the resemblance of his theoretical model of explanation with Max Weber's famous essay on «'Objectivity' on Social Science and Social Policy» (Weber 1949). In that masterpiece of Weber's «Verstehen» sociology (ultimately a philosophy of culture as Weber himself stated over and over), not only the «ideal-typus» model is defined in terms that preceded (in a more elaborate form) Hempel's model but the shortcoming of understanding of Danto about laws and points of view is answered in clarifying terms:

The establishment of such regularities is not the *end* but rather the *means* of knowledge. It is entirely a question of expediency, to be settled separately for each individual case, whether a regularly recurrent causal relationship of everyday experience should be formulated into a "law". Laws are important and valuable in the exact natural sciences, in the measure that those sciences are *universally valid*. For the knowledge of historical phenomena in their concreteness the most general laws, because they are most devoid of content are also the least valuable. The more comprehensive the validity, - or scope - of a term, the more it leads us away from the richness of reality since in order to include phenomena, it must necessarily be as abstract as possible and hence *devoid* of content. In cultural sciences, the knowledge of the universal or general is never valuable in itself. (p. 80)

In both cases, natural sciences and historical ones, it is of individuals we are talking. Weber's insight is that the value individuals attribute to laws depends upon their field of research, not to any alleged difference between laws. That is to say that, with Weber against Dilthey, and with Hempel against Danto's strictly philosophical reading of his essay, the covering law model is in fact relevant (useful) today, only its relevance changes according to its subject of application. This is not so much a matter of a specific (analytic) language connected to an understanding through and through historical of ourselves as a matter of a very wide set of technical languages (sociology, history, etc.) able to deploy the same sort of practical procedure in their use as tools for scientific research. That practical procedure is Hempel's model of covering-laws, albeit today is much more openly narrative than it was to Hempel's eyes.<sup>2</sup>

## 2. Hempel's conception of History, social sciences and science

From the very outset of his essay (as soon as the first paragraph of Hempel 1942), Hempel draws the reader's attention to the fact that there is an analogy of the function of laws in research in history (identified with the social sciences) and in natural sciences. To try to make clear and credible this point his whole essay admits a difference between a proper «general law» and a ««universal hypothesis» but stresses that they carry out the same functions in the scientific endeavour. As section 2.2 argues in both natural sciences and historical-social ones it is impossible to carry out a complete description of the object under scrutiny and so proper scientific explanations are those that can be checked objectively (criteria indicated in 3.3.) and that allows us to predict and explain events by the use of the same method. This method consists of establishing a covering law applicable to every instance of the event we are studying and testing that law, or, conversely, in predicting an event by referring its occurrence to the effect of a law we enunciate as its cause (I am being simplistic, but for more on this, ck. 3). As historical affairs are related to concrete events, such laws are hard to come by, a general hypothesis with the same methodological features takes their place (Hempel 1942, ck. 5.3) and produce what can be named «explanation sketch». These kinds of sketches, as Hempel notices *en passant*, are very common in

History and are the basis for explanations in psychoanalysis (ck. 5.4); this is a remarkable openness of Hempel, considering that psychoanalysis remains until today marred by its alleged unscientific nature. I will now take this hint from Hempel and comment on an essay of Freud that illustrates Hempel's remark (and my thesis in this papers) very aptly, and return later to Hempel's essay.

## 2.1. A note on Freud's «Constructions in Analysis»

Freud's 1937 paper «Constructions in Analysis» (I'll quote from a recent edition, Freud 2002) is very pertinent to the correct understanding of the point Hempel is making. Not only does it address problems of method, it also addresses the problem of the relation between method and practical research (clinical practice, in this instance). And it starts with a reply to a very common criticism of psychoanalysis, one that could easily be attributed to Hempel's contender on the authorship of the deductive-nomological model, Karl Popper. According to such criticism, psychoanalysis deals with patients in a «heads I win, tails you lose» mind frame (Freud 2002: 211). Freud tackles this problem seriously as he recognizes that in analytic practice a “yes” or a “no” of the analysand should not be taken by their face-value.

He proceeds by an analogy between the work carried out during analysis (by analyst and analysand, notice) and the one carried out by an archaeologist excavating in the field. As Freud puts it, the analyst has to reconstruct from the scattered conscious remnant of dreams, memories, etc., what the analysand has forgotten (what became the cause of his present neurosis), just like an archaeologist constructs from ruins the plan and design of entire cities (Freud even suggests «reconstruction» as a more suitable term, ck. p. 213). In this advanced stage of his practice he no longer considers it possible to conjure up all that was forgotten and became unconscious back to the «I» level because the material that analyst handles is much more complex and elusive and because the state of the art is still so primitive (p. 214) However that is not the main point, because «this is where our comparison of the two types of work ends, for the main difference between them is that, whereas for the archaeologist reconstruction is the whole aim and the end of his efforts for the analyst construction is only preparatory work.» (p. 214). Perhaps Freud is being reductive about the historic dimension of archaeology, in fact one could say that the archaeologist just as the historian or the sociologist will all have to deal with the crucial subject of meaning and value after reconstructing their objects (Weber, for instance, is quite clear about that, and Hempel does not think otherwise but only refrains himself from discussing that issue).

In any case, Freud's essay's second part starts with a point of particular relevance to us, the observation of the simultaneity of construction of the hypothesis by the analyst and its empirical test by submitting it to the analysand's opinion. There is, in short, an inextricable relation between scientific elaboration (call it interpretation, construction or reconstruction) and its practice. Not only that, such a process remains scientific because it is deployed under the protocol of criteria presented by Freud (pp. 215 217/8). And, not surprisingly, when this second part of the essay ends (pp. 218/9) what Freud presents to us is a procedure of constant revision of constructions and the gradual revision and structuring of the model proposed by the analyst that is in clear conformity with Hempel's view that a sketch should be construed and tested by its ability to withhold more data and of a more specific kind without hindering the explanation initially proposed (Hempel 1942, ck. 5.4).

It is in fact noteworthy just how much of Hempel's language appears in the third and last part of Freud's essay: «explanation» (p. 219), «historical truth» (p. 221), «analogy» (idem), not to mention the whole of the last paragraph (p. 222) and its constructivist take on the «micro-macro» problem, which is an entirely different problem on its own. Nonetheless, Hempel and Freud are not from the same world just because

they used the same concepts, lived in the same time and had a background alike in other respects (German origin, higher education, philosophical interests, among others). Therefore, they could not become irrelevant by the same set of reasons; in fact we can assume that it is the fact that they are so different and yet we can understand the similarity of their viewpoints so clearly that calls for a new interest in the reconstruction, sketch model of explanation.

## 2.2. Hempel on social sciences

Even if Freud gave some credit to empathy as a way to knowledge for practical, clinical reasons that are self-evident (I hope!), whereas Hempel dismisses it (ck. Hempel 1942, 6.) as irrelevant in the historian's (i.e. social scientist) work, that does not undermines the fact that covering-laws (or general hypothesis) and constructions (or interpretations) are the laws that are relevant for research in the way Max Weber noticed. In fact, in section 7 of his 1942 essay, Hempel is much less subtle than Weber in his observations of the cognitive value of scientific laws, but that also does not hinders the closeness of their views, as Hempel is interested only in the matter of the logical structure of the problem (which is only a part of Weber's much longer and composite essay).

Section 8 of Hempel's essay, which serves as a conclusion, makes once more quite clear that to his eyes there are no laws specific to History as distinguished from sociology, etc., and moreover that all forms of scientific knowledge (historical or natural) draw upon other areas of knowledge as a normal procedure (history uses geography as well as biology ). It is on this ground that the last paragraph of the essay insists on the methodological unity of empirical science in terms that could be subscribed both by Weber and Freud, presumably. But let us notice: it's a methodological unity, not a linguistic one - it does not lose relevance let alone disappear by a change in vocabulary (or language) either in an historical way or in any other way. To push this point through, I will now comment a later essay of Hempel.

## 3. Applying covering laws today

The use of historical landmarks in the history of social sciences (broadly conceived) in this paper does not entail any attempt to use Weber or Freud's authority to overbear that of Danto. As we have said («1», above), Danto himself does not intends to disavow Hempel, only the relevance of its «philosophical world» in our days. Now, as we argue in favour of the relevance of the «covering law» model it is pertinent to expand a little more of Hempel's reflections on the subject and present more recent examples of the model's possible use.

I will comment on a 1963 paper, «Reasons and Covering Laws in Historical Explanation» (quoted from a later edition, hence Hempel 1974), where alongside a discussion of a monograph by William Dray (*Laws and Explanation in History*) Hempel restates his case for his own model. From the start (Hempel 1974: 91) Hempel stresses that the kind of explanation he is referring to as covering-law explanation is relevant «in various branches of empirical science» despite the fact this is an explanation in «statistical-probabilistic form»; therefore he refers to this explanation «*as probabilistic or inductive explanation*» (p. 92).

Immediately (in section «II» of his essay) Hempel reiterates the model both for explaining and predicting events and adds two «amplificatory remarks» (p. 93), the first about the model's applicability and the second explicating the model's deliberate ignorance of matters of psychology of explanation. He goes on

insisting on familiar topics to the readers of his 1942 essay, noticing that both natural and historical empirical sciences cannot meet the standard of a complete description/explanation of a particular event (pp. 94/5) and soon after (section «III», pp. 95-98) not only the subject of the importance of a complete explanation is cut down, but there is a new development as well. The rigidity of the covering law model assumed by its critics (and, ultimately by someone like Danto, when he reduces it to a mere part of analytic philosophy) is put aside: «the covering-law analysis of explanation presents a thesis about the logical structure of scientific explanation, but not about the extent to which individual occurrences in the world can be explained that depends on what laws hold in the world and *clearly cannot be determined just by logic analysis* [our italics]. In particular, therefore the covering-law analysis of explanation does not presuppose or imply universal determinism.» (pp. 96/7). *Ergo*, historicity is not considered irrelevant, only set aside for further consideration of a less logical and more psychological nature (much the same as in Weber or Freud in their essays mentioned above).

What an explanation by covering laws does is applicable to all kinds of events in all scientific endeavors because all events are represented by a predicate-expression regardless of the science that studies them or of their specific kind (p. 97); furthermore, the statistical-probabilistic nature of this model of explanation allows for a third sense of Hempel's model of explanation and a correspondent third sense of completeness, that of «partial explanations of concrete events and in which some of those explanations might be called more complete – in a third sense of the term – than others» (p. 98). As in Freud's «constructions», here too there is a work-in-progress, not reducing all explanation to its logical organization but structuring that organization in a logical (not chronological) priority in the methodology (philosophy, if you will) of history and science in general. Another longer quote:

First, any set of deductive-nomological explanations, each of which explains some aspect of a concrete event, might be called a partial deductive-nomological explanation of that event; and if the aspects explained in one of the sets form a proper subset of those in the other the former set might be said to provide a less complete explanation of the concrete event than the latter. These notions can be generalized so as to apply also to sets containing probabilistic explanations, but this is not the place to enter into further details. (p. 98)

Agreed – such a place is research itself in whatever field of science. Just as Freud emphasized and other post-«Hempel world» authors will insist on (think of Paul Feyerabend, for example) theory should be worked in connection with its practice otherwise it can be a nuisance to scientific progress instead of a stepping stone for it. A good example of this open-ended covering-law model is (on my account as Hempel is not even mentioned in it) Albert O. Hirschmann's analysis in *The Rhetoric of Reaction* (Hirschmann 1991). By formulating a general hypothesis drawn from experience about that rhetoric as reactionary and dividing that specific political rhetoric into three subsets (those of perversity, futility and risk in relation to social change), he examines their mutual support (and contradictions) as discourse (as it is not the substance that Hirschmann discusses), and the essay succeeds not only in presenting a general explanation of the historical event (a specific discourse) but also in exploring its own possible application into other discourse events (chapter VI, on progressive rhetoric, and VII, on democratic discussion).<sup>3</sup>

But a last word about Hempel's essay. Its last section (IV) is mostly dedicated to a discussion of William Dray's views, but in it we also find a schema that can be filled by researchers in any field of knowledge that is worth considering:

Agent A was in a situation of kind C.

A was a rational agent at the time.

Any rational agent, when in a situation of kind C, will invariably (or, with high probability) do X.

and it will logically imply (or confer a high inductive probability on) the *explanandum*:

A did X. (pp. 100/1).

For instance, someone studying (be it as sociology, history, political science, etc.) social change in, say, southern European countries during the 20<sup>th</sup> century, will certainly find several agents («A»), such as intellectuals, politicians, labor unions, etc.; by referring them to their specific situation (C), diverse one from another however slightly, the action taken by each («X») will necessarily change. In this example, intellectuals are (i.e. «C») often in exile, politicians are conducting reforms (openly or otherwise) and unions are frequently outlawed; hence what they do («X») *tends to be* different but supplementary: criticism (intellectuals), reforms (politicians), social strife (unions).

This construal is recognized by Hempel to be akin to that of Gilbert Ryle's explanation by reference to dispositions, in this instance a «broadly dispositional» scheme (see p. 101 and note 14) and as such it can be criticized as being not entirely suitable as a «covering law» inasmuch the third proposition of the *explanans* (relating «C» to «X») is not really an empirical law; however, such a strictly logical objection (typical of the analytical philosophical world to which the Hempelian point of view owes its relevance, according to Danto) is considered by Hempel as failing to «do justice» to the nature of the concepts used (all of which are composed and governed by «large clusters of general statements»). Notice again that there is no narrowing of this model's applicability to historical (i.e., non-natural, or exact) sciences; and that the very nature of the covering-law explanation in all empirical research calls for a further analysis of value (historical meaning) clearly demarcated from it.

## Notes for a conclusion

It has been this paper's perspective that the reason why philosophy of history became so unusual in the second half of the 20<sup>th</sup> century is the growing relevance of historical, i.e., social sciences, and not as much a new historical attitude in philosophy itself (although that occurred as well, and quite logically). When Danto wrote that explanation is no longer a relevant subject, he obliterated all the methodological disputes that accompanied the growth of social sciences in a way that does not only excludes philosophy from today's academic world but also depreciates philosophy's (and particularly philosophy of history) contribution to that growth. Hempel's 1942 paper was a seminal contribution for that at least in two accounts: it maintained the relevance of philosophy of history's traditional disputes in terms (such as causation, explanation, prediction) suitable to today's scientific disciplines, and it also established the relevance for a further investigation of those disciplines (in psychology, about values, or using other designations deemed appropriate).

Naturally our conception of logic is not as rigid and strict as Hempel's and as of his analytic tradition, and not only for the influence of Kuhn; but as we noticed in the second footnote of this paper, even Danto noticed early on that Hempel's 1942 paper did not exclude narrative as a theory-valid device, something that we tried to exemplify in section 3 of this paper. Looking back to the history of philosophy, where Danto lay to rest philosophy of history, one can say, in fact, that the narrative vein of



philosophy of history is present from the start, with Voltaire and Herder (and above all in the very attempt to replace theology of history). Moving beyond historical considerations, one can even suggest a new categorization of the very terms used in this discussion. Science («theory») should be considered narrative, of a conceptual kind, whereas fiction is narrative of a personal kind («personal» as in «persona» in Greek drama, meaning specific characters). If we consider historical writing, i.e. the science of history, even a straightforward biography (even an autobiography!) does not render us a person, only a simple representation of that person (i.e., its concept) as sketched by the scientific writer, regardless of his specific discipline (history, sociology, etc.).

One explanation for the decay of philosophy of history as practiced until early on in the 20<sup>th</sup> century (Toynbee can be read as the last representative of that tradition) is its «original sin» of taking up from theology of history a moral bias that overrode empirical data whenever the two collided. Such a point of view, often referred to as «metaphysical», was justly disputed by the likes of Hempel, Carnap, etc., and it is no matter for surprise that, in their option as philosophers criticizing metaphysics, they had chosen to hold on to logic as a way to avoid that unscientific use of language. But their language and above all their aspiration for a common scientific language, do not have to be considered today their greatest achievement; it is rather the possibility, through the «covering-law» model, of a common scientific method that should be valued as relevant and exerted in as many areas of research as possible, in order to allow a mutual comprehension (*Verstehen*) of disciplines otherwise less attentive to each other and *per se* unable to elaborate those points of view of historical nature that philosophers, scientists and human beings in general so badly need.

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## Notes

1. The reference to a new philosophy of history alludes evidently to the volume's title, whose editors attribute such a new philosophy of history to Hayden White (ck. P. 280).
2. Not surprisingly, it should be noted. Danto himself (ck. P.71) refers to his own work as trying «to demonstrate an equivalence between explanation as construed by Hempel, and narratives, thus vindicating the so-called 'covering-law model' against the claim that narrational models were deeply alternative to it (...)».
3. Hirschmann's adherence to Kuhnian language, by the way, is patent in a previous work where «paradigms» are expressly referred to Kuhn's work (*The Passions and the Interests: Political Arguments for Capitalism before Its Triumph* Princeton U. P., 1977).

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