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THE PHILOSOPHY OF HISTORY

A DIALOGUE

Max H. Fisch

Persons of the Dialogue: a Philosopher and a Historian

Time: April 14, 1957

Scene: the Historian's study

P. I've been reading lately what the philosophers have been writing about history. They are having a symposium about it at Chicago in May, and I've been asked to lead off with something for my fellow symposiasts, Donagan and Hinshaw, to shoot at. ⁽¹⁾

H. Oh? I thought the philosophers had long since lost interest in anything so vast, windy and interminable as history. Has the pendulum swung, and are they beginning to take large views again?

P. No, it is not large views of the human past they are taking, but minute views of the historian.

H. *The* historian? A fictitious character, I presume?

P. Now don't be captious; leave that to us. I admit they sometimes seem to be imagining themselves doing history, and to be analyzing what they imagine themselves doing. But sometimes they examine short passages from real historians.

H. Dead ones, you mean? It would spoil the fun if the historians were alive to answer their questions for them. And what kinds of questions do they raise?

P. They are nearly all of one kind, epistemological. What is the historian's object, how does he come to know it, and in what does his knowledge consist? Are his statements verifiable, and, if so, how? Is moral judgment a part of his business, and, if so, how does he reach such judgments, and are they confirmable? Can he be objective in his statements and judgments alike, or in his statements only, or in neither? Does he treat his dated and placed events as unique, and as material for his own narrative only, or

as susceptible of generalization also; and, if the latter, does he prepare them for the uses of the generalizing social sciences, or do his own generalizing? Does he explain the events, actions, thoughts, or other changes which he reports or narrates? Does the Popper-Hempel model fit all his explanations, or some only, or none? Are other models feasible, and are they required? (2)

H. The questions are fine, but now do the philosophers hope to answer them?

P. For the most part, by watching closely what the historian does, though I won't deny they proceed sometimes as if the power of definition and a certain dexterity in linguistic analysis would do the trick.

H. If the questions are honestly inductive, and inductively honest, I should think a wide induction would be in order. There are many historians, and they do many different things. Has a sampling technique been devised? Is there a repertory of samples? Are historians invited to submit additional samples, to represent kinds of historical performance they think are not so far represented? May it not happen otherwise that explanation, for instance, is defined without regard to what historians do, and it is then decided that whatever they do that is not that is not explanation, or is at best an explanation sketch?

P. You have taken the words out of Hempel's mouth. (3)

H. Why, in the work even of a single historian, taken almost at random, I wager more different things would be found than are dreamed of by your philosophers.

P. That is a hypothesis worth trying. If I knew a historian of modest parts, old enough to have done every kind of thing he is likely ever to do, but lazy and unproductive enough to permit my examining the whole of his published work, I might make the trial myself.

H. Here am I; try *me*. I've been writing history for thirty years. You've known me all that time. You claim to have read every-

thing I have written. Besides, it is all here on my shelves or in my files, and we can refresh our memories. Will I do?

P. Hm... At least for a start. As a historian, you're an ill-favored thing, but mine own. 'Tis a poor humor of mine, sir, to take that that no man else will. If I may be your Touchstone, what rubs onto me from your work shall be my philosophy of history, until Donagan and Hinshaw teach me better.

H. Where shall we begin?

P. Where *you* began. How did you find your way into history in the first place?

H. I was brought up on Biblical history and Biblical criticism, higher and lower, J and E, first and second Isaiah, the prose folk tale of Job and the dramatic poem sandwiched into it, and all the rest. As a child, with scissors and paste, I made my own harmony of the gospels.

P. Well, that qualifies you as an amateur in scissors-and-paste history which Collingwood says was nearly all there was until recently and in critical history, which he says was the last phase of scissors-and-paste history. The question is whether you ever graduated into what he calls scientific history. ⁽⁴⁾ Let's come down to your prentice job, your thesis on "The Influence of Stoicism on Roman Law." ⁽⁵⁾ Let me see again that carbon copy on which I once made so many marginal comments. I remember our having much argument as to what might constitute influence, and what might be evidence of it.

H. Here it is, and right you are.

P. You must have been under the influence of something heady when you took on such an impossible thesis. What was it?

H. Oh, they had taught me in college that besides Biblical history and literature the chief ancient sources of the intellectual substance of western civilization were Greek philosophy and Roman law. Then, in graduate school, in the scholarly literature I came upon statements to the effect that the philosophy had had this or

that influence on the law, and, through the law as well as directly, on the Christian theology whose other chief source was the Bible. I knew the Bible, and I had studied the philosophy, but not the law. I had to write *some* thesis. I foresaw no other occasion for studying the law, so I took that one, and nobody had the pity to stop me.

P. The title of your thesis would suggest to our philosophers that certain features of Roman law are to be explained as effects of certain features of the Stoic philosophy. Your table of contents would suggest to them that you are trying to show how those causes came to have those effects. You do this by exhibiting the intermediate links. You trace the Roman reception of Stoicism, and the emergence of Stoic ideas in Roman religion, education, literature, and science, and in the commonplaces of moral counsel and public speech. You then trace the same ideas and other more technical Stoic doctrines in the detail of Roman law. As I recall, you found more Stoicism there than any previous historian had found, and you went farther toward explaining its presence there. Now would you agree that the influence you assert is a form of causation, and that causation is intelligible only in terms of causal laws? If so, the standard model for what you were doing would seem to be the Popper-Hempel or covering-law model.

H. I can't think what the covering law in this case would be. That in any society in which a certain philosophy prevails in other aspects of culture it will prevail in law also? But I nowhere assert that, and I nowhere assume it.

P. Then how does the prevalence of Stoicism elsewhere explain its prevalence in law?

H. It certainly doesn't if an explanation must logically entail what it explains, and if general laws are required for that purpose. Unfortunately all the universal propositions available to the historian for conferring necessity upon his conclusions are propositions he knows to be false. And he suspects that if he took one of them and quantified and qualified it until there were no longer any ex-

ceptions to it, there would also be no instances of it but the one he set out to explain.

P. But if you forego that sort of explanation, what have you left to offer?

H. In the present case, I merely assume that my reader, when he has before him the evidence that some of the lawyers were Stoics and all of them lived in a society in which Stoic ideas were current, will agree with me that it would not have been unnatural for them to do some of their legal thinking in Stoic terms, so that what looks like Stoicism in the legal texts may really be Stoicism. On this assumption I proceed to explain the texts by placing the doctrines they contain in the framework of the Stoic system. Surely explication of texts is something all historians do, and surely it is not any kind of causal explanation, and calls for a model or models quite different from the covering-law model.

P. I agree that if it were simply a matter of your telling a story based on evidence, and of the reader, believing your story, being thereby prepared to understand the texts as you explain them, the Popper-Hempel model would not fit. But it still seems to me that you are doing more than that. For example, you explain the reception of Stoicism at Rome in part by its congeniality to the Roman character, and I see I wrote this comment into your margin: "Perhaps there has never been another people with such a taste for edification as the Romans, and there was no Greek philosophy so ready as Stoicism to do the edifying."

H. The covering law in that case would presumably be that character determines philosophy, or determines a choice among the philosophies offered. But characters are habits or dispositions, and surely dispositions are not causes in the covering-law sense.

P. Objection sustained. It just happens that Donagan, starting from Ryle's distinction between law statements and dispositional or law-like statements, is in a fair way to work out a model of historical explanation in terms of disposition statements. But it appears

that explanations on the Ryle-Donagan as well as on the Popper-Hempel model will logically entail what they explain. (6)

H. What a lust for necessity you philosophers have! Such knowledge is too wonderful for me; it is high, I cannot attain unto it. I should have said my explanation amounted to no more than this. Choosing Stoicism was of a piece with other Roman choices and actions; there is nothing surprising about it; no special explanation is required. Or, if you prefer, whatever is to explain that must first explain the Romans; which I leave to those who are better equipped.

P. Let's move on, then, to your later works. But first, why did you never publish the thesis?

H. It was immature and incomplete. I needed to take more account of philosophies other than Stoicism, and I needed a wider and firmer grasp of Roman law. There was great controversy as to the extent to which the surviving legal texts had suffered from interpolation, and many of my texts were suspect. I spent a decade revising and reworking. Meanwhile, during the Great Depression, apart from rude shocks to the foundations of the society in which we lived, I encountered two shocks to the foundations of my thesis, and I have still to finish the repairs. The first shock was administered by the historian Tarn, and the second by the philosopher Vico.

P. Now don't forget Marx. When so many of our friends were turning Marxist in theory, if not Communist in party, I remember your reading him extensively and closely for what I believe was the first time, and giving a more sympathetic account of him than I imagine you would now.

H. So I did. The point that came home to me was the way in which law and philosophy alike, along with the other non-economic aspects of culture, were alleged to be ideological reflections of the class struggle and of its dialectic. But, exciting as Marx was, and great as the pressure then was to master and

assimilate what he had to say, he was for me a way-station to Vico. Marx had recommended Vico's *New Science* to Lassalle "for its philosophic conception of the spirit of Roman law in opposition to the legal Philistines," and it dawned upon me that I might be one of the Philistines. Instead of deriving Roman law in part from Greek philosophy, or deriving both entirely from economy, Vico derived Greek philosophy from Greek law; or, more exactly, he derived the logic, metaphysics, and ethics of Socrates, Plato and Aristotle from the disputations of the Athenian assembly and courts: first, popular government, he said; then laws; then philosophy. Roman law, like Greek law, was an indigenous growth, and the borrowings I had made so much of, if real at all, were late and insubstantial. So it appeared that the interpolation detectives were but following in Vico's footsteps at an interval of two centuries. (7)

P. Shock number two. And number one was Tarn?

H. Yes. He was the historian on whom I had most relied for my general conception of the primacy of the Stoa in Hellenistic civilization. Three years after my thesis was accepted, he gave a famous lecture before the British Academy in which he removed all the props on which I had built. (8) Until then, he had presented the Stoics as heirs of Alexander, who systematized and propagated the ideas that Alexander had projected or inspired. Now Tarn disinherited the Stoics, and robbed them of most of the influence he had previously ascribed to them. Alexander was in fact "the first man known to us who contemplated the brotherhood of man or the unity of mankind." The attribution of those ideas to Alexander could not be a projection backwards of Stoic thought, because there was an "irreconcilable opposition" between Stoicism and those ideas, and the Stoics did not adopt them until two centuries or more later, when other philosophers had made them commonplace.

P. I seem to recall that you not only spent two or three years constructing an argument to show that Tarn had been right before and was wrong in his Alexander lecture, but you undertook to give

a psychological explanation of his unacknowledged change of mind. And I think I shall have you there. May I see the article? Yes, here it is. (9) You say that Tarn had step by step been asserting his independence of Kaerst, the German historian of the Hellenistic age, and that he uncritically seized upon a specious essay by Goodenough on "The Political Philosophy of Hellenistic Kingship" to complete his emancipation. Were you not here relying on a psychological law which you did not explicitly formulate, and have we not here a clear case of the covering-law model?

H. Let's see. I show that in relation to Kaerst the Alexander lecture is but another step in a direction in which Tarn has been steadily moving, but that in relation to his own previous views concerning Stoicism the lecture is a nearly complete reversal. There is no new evidence to explain the change; there is only Goodenough's construction, from familiar but dubious evidence, of a non-Stoic political theory which Goodenough presents as the official philosophy of the Hellenistic age. Tarn dethrones Stoicism and puts this pretender in its place. If he is right now, he has been propagating for twenty years a radical error. He does not acknowledge his previous error, or his conversion from it. It seems clear that he is not aware of the extent of the change, or of the insufficiency of his reason for it. If he had been aware, he would have faltered, and he would not have been able to take that final step away from Kaerst which he wished to take. When an eminent historian grasps at straws and gives the lie unawares to much that he has hitherto stood for, an explanation is in order. I believe it is possible for a strong desire to have such an effect. For lack of a better explanation, I hazard a guess that this has happened in Tarn's case. I label it a guess; I say that even if it truly explains Tarn's latest views, it cannot explain them away; and I proceed to consider these latest views on their merits without further reference to my explanatory guess. Had I thought I needed a law to sanction my guess, I would have skipped it.

P. Are you sure you didn't suppress the law because it would have served equally well to explain your attack on Tarn by your need to cling to the primacy of the Stoa as a prime support of your thesis?

H. Touché. A hit, but not a wound.

P. Well, to get on: I don't suppose your various essays on the history of American philosophy have anything to do with your thesis?

H. Yes, they do, in this way. Marx and Vico had opened a view of the relations between philosophy and law quite different from that which I had assumed in the thesis. Now the fashionable theory of law in our society was legal realism. It seemed to stand in some logical relation to pragmatism. Was there also a historical relation between the two? Did either develop out of the other? If so, was the movement from philosophy to law, or from law to philosophy? This was the primary focus of all my researches into the origins of pragmatism. The fact that Holmes and Green and Warner, three of the founders of pragmatism, were lawyers, seemed to me significant, ⁽¹⁰⁾ the more so since Peirce had called Green "the grandfather of pragmatism." I traced legal realism and the prediction theory of law back in Holmes's published writings to 1872, ⁽¹¹⁾ and verified Peirce's claim, under Green's inspiration, to have drawn pragmatism as a corollary from Bain's theory of belief, but at a time when the legal theory was already in print. ⁽¹²⁾

P. In thus moving from the relation between Greek philosophy and Roman law to that between Greek philosophy and Greek law, and finally to that between American philosophy and American law, were you not preparing the way for some sort of comparison, and for a hypothetical generalization which might then be tested by study of the relations between philosophy and law in other societies?

H. Not at all. I had been interested in Greek philosophy and Roman law, not as samples of philosophy and law in general, but

as the particular systems out of which western intellectual culture had developed. I was interested in American philosophy and American law as the particular systems of our own more immediate society. To be sure, what I found in the American case suggested questions concerning the Greco-Roman case, and vice versa. I was struck by a certain kinship between Stoicism and Pragmatism, which had been a source of embarrassment to Peirce, but this was nothing to my purpose. I was not in search of generalizations of that kind. I did eventually try my hand at another kind of generalization, much more common among historians, for which the philosophers have not yet given us a model.

P. What was that?

H. A general characterization of what I called "The Classic Period of American Philosophy," the period from the end of our Civil War to the beginning of the Second World War. I presented this in two slightly different forms, one an unfocussed statement, ⁽¹³⁾ the other a statement with Dewey as focal figure. ⁽¹⁴⁾ In both forms I generalized in terms of the major influences in the prevailing climate of opinion, and of the major themes and tendencies common even to philosophers the most critical of each other, and distinguishing them all from those of other times and places. Each of my generalizations was illustrated by quotations from all or most of the major philosophers of the period, each quotation being at the same time characteristic of its author.

P. If our philosophers were to provide a model for that sort of mutual or reciprocal explanation, it just might turn out to be a variation on Hegel's concrete universal, or identity in diversity, the identity affording an explanation of the diversity and of each diverse detail, and the diversity being an explication of the identity. Hegel is a bit out of fashion; but I see there's going to be a Hegel symposium following ours. Perhaps it will start something in this direction. Or perhaps a model not yet dreamed of will be found to fit.

In any case I am still puzzled about your continually shying

away from the covering-law kind of explanation. What puzzles me is that Vico is one of the philosophers you have most admired and studied, and about whom you have written most. Now Vico was surely a philosopher of history, and he presented his philosophy of it under the title of "Principles of a New Science concerning the Common Nature of Nations." His ideal eternal history traversed in time by the histories of all nations looks very much like a general law. He at least wanted history to be a science. Don't *you* want to be scientific?

H. Yes, if it means doing better what I am already doing. No, if it means doing something different. Mind you, I had no objection to Vico's search for law, and I have none to other historians seeking it. If they succeed, I shall be instructed; if they continue to fail, they will take the curse off my laziness. I only say that I don't seek it, and that it is my impression from a wide range of reading in history that few historians do. And I confess that it was something of a problem for me why Vico had made so much of his ideal eternal history. That seemed to require explaining. I thought I found the explanation in the scientific movement at Naples in his youth, and particularly in a scientific academy called the Academy of the Investigators. Its most flourishing period was the five years preceding Vico's birth, but there were two revivals of it during his lifetime, and he was a member of it during the second revival if not also during the first. I thought it was the prestige of that Academy that led Vico, in emulation of the new sciences of nature which it cultivated, to construct a new science of history that should have the same generality or universality, and a higher certainty. After several years of preparation, I spent a year writing the history of the Academy and tracing the lines of its possible influence on Vico. ⁽¹⁵⁾

P. Is there not a general law assumed in your thus explaining the genesis of Vico's new science?

H. A law to the effect that when historians live in a society in

which natural science has high prestige and accepts the covering-law model of science, history will tend to do likewise? The only way to keep that from being falsified is to keep it vague; but, let it be precise or vague, I have no use for it. It seemed an hypothesis worth trying that Vico in particular was influenced by the Academy of the Investigators in particular, with no general law in the premises. If he was so influenced it should be possible to find fresh evidence of the influence. I found the evidence, confirmed my hypothesis, and thereby provided a partial explanation of Vico's excessive addiction to the covering-law model of history. So far as I was then or am now aware, I did not at the same time confirm any more general proposition than that.

P. I think the kind of generalization you practice, that of "the character of the age," may depend upon the kind you forego; but let that pass for the moment. It is time we made a first bow to the question of objectivity and subjectivity in history. Would you admit that your interest in the Academy for its bearing on Vico gave a certain subjective cast to your history of it?

H. I admit that I mention Vico's name more frequently, and put more emphasis on his relations with the Academy and with its individual members, than a historian with a different interest need have done. But I do not admit that this resulted in any misrepresentation or distortion of fact. Without that interest, I should never, after long preliminary study, have devoted a year to the Academy. I searched the libraries and archives of Italy for source materials and for accounts based on source materials no longer available. I made use of every scrap of evidence I could find, and I appended a complete bibliography in which I reported locations of manuscripts and rare publications, so that if anyone cares to check my statements, he can do so with a minimum of trouble. I think I have shown by example that a highly specialized interest is not incompatible with a full and balanced history, meeting the requirements of scholars who do not share that interest but have

other equally specialized but quite different interests, so that no one of them need feel obliged to repeat any part of my labor.

P. May I return now to the question of generality? The Academy of the Investigators was one of many scientific academies. Surely you took account of that fact and considered it as a member of a class?

H. I took account of its relations, by correspondence and exchange of visits and by repetition of their experiments, with the other scientific academies of Italy and of western Europe at large. I ranked it next after the Lincei and the Cimento. I searched the archives of those and other academies, including the Royal Society of London, for evidence of relations with the Investigators. I offered summary statements about the rôle of academies in general, and of the Investigators in particular, at that particular juncture in the history of science. For example, about their relation to the universities in the Quarrel of Ancients and Moderns. Summaries of that kind have a large place in history, but they are nearly as time- and-space-bound as the particular statements that are directly warranted by the evidence. So far as I can see I neither asserted nor assumed a law.

P. From Stoicism and Roman law it seems a farther cry to the Academy of the Investigators than to Pragmatism and American law, but I see the bridge by way of Vico. But how about your other work in the history of science, and how about your work on the historian Binkley?

H. Vico had confirmed me in a belief I had already formed that the history of philosophy made insufficient sense apart from the history of science, and that medicine made a good focus for the history of science. (Vico, you know, had a theory of medicine on which he prided himself.) Well, under the name of intellectual history, I was teaching what was in fact history of science with special attention to medicine. I might nevertheless have remained an amateur in that field but for the series of accidents that put

me in charge of the incunabula and the sixteenth and seventeenth century books in the Army Medical Library during the Second World War. Here was a chance to do something on the beginnings of modern science. Perhaps there is no better collection of source materials for that purpose, if medicine be taken as focus. The tercentenary of the first edition of Vesalius's *Fabrica* gave me in 1943 a sufficient occasion for trying my hand at a series of modest essays. What interested me was the relation between Renaissance science, Renaissance drawing and wood engraving, and Renaissance printing. I tried to trace the steps by which they were brought to a single focus in Vesalius's book. (16)

P. I am struck by the fact that, of all your writings, the essay on "Vesalius and His Book" comes nearest to being a piece of literary criticism, and I recall that the purple passage with which it concludes was used as a motto in Harvey Cushing's *Bio-Bibliography of Vesalius*. Is that sort of stuff the proper business of a historian?

H. Why not? The natural bias of the historian is toward individualization rather than generalization. He is not merely the recorder but the critic of the works of man. If he differs from the professional critic, it is in ranging more freely over the entire human past, and in working out in greater detail the steps by which the achievements of art and science became possible.

P. Your "Vesalius in English State Papers," (17) on the other hand, is almost straight biography of the practicing court physician. Does any particular interest attach to it for our present purpose?

H. Not much beyond the fact that Vesalius's German biographer, Roth, though he had made use of the state papers and letters of the Empire, of Spain, and of several of the countries that maintained embassies at the two courts, had overlooked entirely the English state papers. It was merely historian's luck that from these fresh sources there emerged a vivid picture not only of the last twenty years of Vesalius's life but also of the institution of the

court physician. At both courts, Vesalius spent most of his time as physician to the administrative and military staffs, to the ambassadors from abroad, and to the embassy staffs. He was in demand not only for his medical services but also for his company and for news of the emperor's or the king's health and of goings-on about the court.

P. In your book on Nicolaus Pol, ⁽¹⁸⁾ which I have more freshly in mind, you reconstruct the life of another Renaissance court physician who probably knew Vesalius's father and grandfather at court, and you reconstruct his remarkable library, and place his tract on the guaiac cure for syphilis in the history of syphilis. But from the point of view of the philosophy of history, the most interesting passages are those bearing on the controversy as to whether syphilis originated in the Old World or the New, or independently in both. From scattered bits of evidence, each slight in itself, you reconstruct the story of a medical commission sent by Cardinal Lang of Augsburg to Spain and Portugal to investigate and report on the guaiac treatment as it was practiced there by those who had learned it in Hispaniola. Will you hand me the book? Thank you

Two things strike me here, and they are closely related.

The first is that the theory of the American origin of syphilis was one of the first conscious applications of the covering-law model to history. You quote the covering law as follows: "Such is the divine mercy that wherever it permits us to be afflicted for our sins, it places a remedy equal to our afflictions." By illegitimate conversion, this yielded the axiom, "Whence the remedy, thence the disease." The remedy, the wood of the guaiac tree, came from the New World. Therefore the disease for which it was specific, namely syphilis, came from the New World.

Now you don't subscribe to that covering law, but my second point is that you do seem to subscribe to others in the same context. Pursuing hints supplied by Paracelsus and by Ulrich Hutten, you

proceed to connect both the medical commission of Cardinal Lang and the theory of the American origin of syphilis with the virtual monopoly in the guaiac trade enjoyed by those merchant princes the House of Fugger. "The prestige of the wood," you say, "was greatly enhanced by widespread acceptance of the doctrine of the American origin of the disease, and the spread of that doctrine was doubtless promoted, for the most part in good faith, by traders in the wood." Your covering law for that would perhaps be that doctrines are spread by those who have most to gain by their spread. You also suggest an explanation of the popularity of the guaiac treatment in terms of the class structure of society and of the medical profession itself. "The only really effective remedy then known," you say, "was mercury. It was commonly administered by inunction, which was a crude, untidy method suited for surgeons who treated the common people, but scarcely for learned physicians. The mercurial poisoning that followed was often more terrible than the disease itself. Only the harmless guaiac treatment, with its endless refinements and adaptations, gave full scope to the scholastically trained minds of the physicians. Only it was long and intricate enough, and called for sufficient variety of services, to gratify the vanity of their well-to-do patients."

H. I have three things to say in reply. In the first place, these are but parts of a total explanation. Among the other parts is the therapeutic fallacy. "Syphilis was not clearly differentiated from other venereal diseases; its selflimited character was not understood; the wood was credited with effects which would have followed from the diet and regimen alone, or even without their assistance. The patient took the decoction; the symptoms present when the treatment began disappeared in its course or shortly afterwards; therefore the decoction was the effective remedy."

In the second place, the trouble with the covering laws you read between my lines is that, as laws, they do not hold. I do not believe any of them, and I do not assume them, and I do not

suggest that my facts lend them any confirmation. I merely say that there is some evidence that certain individuals were influenced in some degree by one or more of these considerations, and that if we extrapolate and assume that most or all of the believers in the efficacy of guaiac were influenced in varying degrees by these and by similar considerations, it will no longer surprise us that what we know to be false was so long and by so many as intelligent as ourselves believed to be true. The argument is not that people who have certain beliefs and certain motives will have certain other beliefs, but that it is possible for them, within the bounds of rationality, to hold such beliefs. Thus one may not unreasonably believe that guaiac regularly and normally cures syphilis (a) if it is not evident that it never does, (b) if it seems in some cases within a limited range of observation to do so, (c) if one believes that certain other things regularly happen which do in fact sometimes happen, and (d) if the guaiac cure can be subsumed under the formula for these other happenings. Now it sometimes happens that where a disease is common a remedy is also common which is in fact a specific for the disease in question. If one believes on religious grounds that this is regularly the case, and one is informed on good authority that syphilis is common where the guaiac tree is native and common, one is not thereby compelled to believe, but one may not unreasonably believe, that guaiac is specific for syphilis. I adopt this as a heuristic hint to guide my search for evidence. I find evidence that the people who believed in the guaiac cure did in fact believe one or another of the propositions under which that belief could reasonably be subsumed.

My third reply is that more important than the psychology or the logic of individual belief, in a case of this kind, is what we may call the vesting of belief in institutional structures, such as the merchant empire of the Fuggers or the complicated ritual of the medical profession. For example, the Fuggers had a model housing project at Augsburg called the Fuggerei, in which there was an

infirmary named the Holzhaus or Woodhouse, where the guaiac cure was practiced with all its refinements, as a model to the entire profession.

P. I begin to suspect that before we are through you will have institutions doing the work my philosophers want covering laws to do. Let's get on. May I have another look at your edition of the papers of your historian friend Binkley? Thank you Your biographical essay, like the essays by him which you bring together, is full of ideas about historical research, writing and teaching, library and archival policy, the organization and reproduction of research materials. Perhaps the most interesting is the assumption that the objects of history are institutions, and the proposal to treat European history in such a way as to play down the institution of the national state and to play up other institutions, such as the family, the village and the city on the one hand, and on the other the institutions operative in what he called the world net of power, the world of debts and markets, and the world of opinion. Also to our present purpose is the fact that, like so many other American historians of his time, he was a relativist. In his view, as you put it, "every history, however remote the period with which it ostensibly deals, has its real terminus and controlling frame of reference in the time of its composition." (19)

H. Yes, and the two things went together in his case. He was working toward a history of Europe in the nineteenth and twentieth centuries which should be deliberately oriented toward the explanation of matters of urgent public concern at the time of composition and likely to continue to be matters of public concern for some years thereafter. He proposed to play down the national state and play up other institutions because the national state obsession was the chief obstacle to the development of the forms of order he thought were needed for our time.

P. Did you share his relativism, as I begin to see you did share his conception of institutions as the proper object of history?

H. I did not. Or rather I should say that the relativity he had in mind is a variable and admits of degrees. It is likely to be greatest in a general history that has its explicit terminus in the present, and may easily be much less in histories of remoter periods and of narrower scope, and least in those that admit of monographic completeness. I submit, for example, that my history of the Academy of the Investigators, though intended to answer questions that concerned me personally at the time I wrote it, is but little affected by relativity to that occasion. And I would make the same claim for my earlier piece on one of Coleridge's letters, and for my essay on Peirce at The Johns Hopkins. (20)

P. I take it this is the same question as that of subjectivity and objectivity on which we touched before.

H. Yes, and the obvious remark is that if there is a subjectivity or relativity which affects all history alike, it concerns the philosopher and not the historian. The only subjectivity that concerns the historian is that which is variable, which can be criticized, and which is incorrigible. It is a subjectivity which is properly blamed, and is the opposite of an objectivity which is properly praised.

P. "Objective" means "value-free" here?

H. By no means. The historian is not blamed for praising and blaming, and praised for doing neither, but blamed if antecedent judgments of value blind him to contrary evidence, and praised if his selection and treatment of evidence is clearly not unbalanced by the desire to support judgments formed in advance of the search for evidence. As I intimated previously, the historian of art is a critic of art, the historian of religion a critic of religion, the historian of science a critic of science, and similarly the historian of economic, social, and political institutions is a critic of those institutions. Objectivity is not absence of criticism, but unreserved submission to further criticism, complete openness, withholding nothing from judgment. Furthermore, it is guaranteed not by the psychology or logic of the individual historian, but by the institutions of historical

scholarship themselves, by the orderly procedures of review in the journals, by the ethics of bibliographic citation, by indexes, catalogues, bibliographies, reference works of all sorts, which protect the historian against involuntarily ignoring anything relevant to his undertakings, and provide ready means for calling him to account when he does.

P. That is, objectivity is institutionalized, and subjectivity is a weakness of the historian who is not yet institution-broken?

H. The analogy is far-fetched but, since you have fetched it, I won't ask you to throw it out if you will agree not to press it farther but to leave the historian otherwise unbroken.

P. Agreed. And now we have accounted for most of your published historical writing, and I still think that, in spite of your stubborn resistance, we have uncovered some covering laws, both laws that explain what the historians try to explain and laws explain their explanations. But that will take some closer and more leisurely looking, and you may persuade me in the end that they are all nugatory. What bothers me now is this: If, as you say, the historian is, among other things, a critic of institutions, what is there left for philosophy to do? As no one knows better than you, the question is more embarrassing to me than it would be to many another philosopher, or than it would have been to me until a year ago, when, in a presidential address, I proposed to define philosophy itself as "the critic of institutions." (21)

H. I did not mean to needle you, but just to pull your leg gently. The historian criticizes only those institutions that come within the scope of his investigations. He does not often make explicit the principles of criticism upon which he relies. Still less often does he justify those principles as against alternatives that have been proposed by philosophers or assumed by other historians. And even when he does do so, his justifications are piecemeal and *ad hoc*, not systematic or comprehensive. So the philosophers need not fear that the historians will leave them little or nothing to do.

But I seem to discern a continuum of inquiry connecting the shortest-tethered grubbing of the historian with the most abstract and universal critiques and speculations of the philosopher.

P. In any case, the ultimate problem of the philosophy of history may well be that of the relation between history and philosophy. Our symposium will not get that far. For the solution we had best look to philosophers who are also historians, or to philosophers who have carefully considered the work of such philosopher-historians. Meanwhile, you've given me enough for Donagan and Hinshaw to shoot at.

1. This dialogue was read on May 3, 1957, as the opening paper of a symposium on the philosophy of history at the fifty-fifth annual meeting of the Western Division of the American Philosophical Association, at the University of Chicago. The papers of the two other participants have already been published: Virgil Hinshaw, Jr., "The Objectivity of History," *Philosophy of Science* 25: 51-58, 1958; Alan Donagan, "Social Science and Historical Antinomianism," *Revue internationale de philosophie* 11: 433-449, 1957.
2. Karl R. Popper: *Logik der Forschung*, 1935, 26 f. (English translation: *The Logic of Scientific Discovery*, 1959, 59 f.). Carl G. Hempel: "The Function of General Laws in History," *Journal of Philosophy* 39: 35-48, 1942, reprinted in: *Readings in Philosophical Analysis* selected and edited by Herbert Feigl and Wilfrid Sellars, 1949, 459-471. Karl R. Popper: *The Open Society and its Enemies*, revised edition, Princeton, 1950, 445-449, 720-723. Patrick Gardiner: *The Nature of Historical Explanation*, Oxford, 1952. William Dray: *Laws and Explanation in History*, Oxford, 1957.
3. In Feigl and Sellars (cited in note 1), 465.
4. R. G. Collingwood: *The Idea of History*, Oxford, 1946, 249-282.
5. M. H. F.: *Stoicism and Roman Law*. Ph. D. thesis, typewritten, Cornell University Library.
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 11. M. H. F.: "Justice Holmes, the Prediction Theory of Law, and Pragmatism," *Journal of Philosophy* 39: 85-97, 1942; "Evolution in American Philosophy," *Philosophical Review* 56: 357-373, 1947, at 366 f.
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 13. M. H. F. (editor): *Classic American Philosophers*, 1951, 1-39.
 14. M. H. F.: "Dewey's Place in the Classic Period of American Philosophy," in: *Essays for John Dewey's Ninetieth Birthday* (edited by K.D. Benne and W. O. Stanley, Urbana, Illinois 1950), 9-36.
 15. M. H. F.: "The Academy of the Investigators," in: *Science, Medicine, and History: Essays on the Evolution of Scientific Thought and Medical Practice written in honour of Charles Singer* (2 vols., Oxford, 1953), I, 521-563.
 16. M. H. F.: "Vesalius and His Book," *Bulletin of the Medical Library Association* 31: 208-221, 1943; "The Printer of Vesalius's *Fabrica*," *ibid.*, 240-259.
 17. M. H. F.: "Vesalius in English State Papers," *Bulletin of the Medical Library Association* 33: 231-253, 1945.
 18. M. H. F.: *Nicolaus Pol Doctor 1494*. New York: Herbert Reichner, 1947.
 19. M. H. F. (editor): *Selected Papers of Robert C. Binkley* (Harvard University Press, 1948), 22.
 20. See note 15 above. M. H. F.: "The Coleridges, Dr. Prati, and Vico," *Modern Philology* 41: 111-122, 1943; "Peirce at The Johns Hopkins University," in: *Studies in the Philosophy of Charles Sanders Peirce* (edited by Philip P. Wiener and Frederic H. Young, Harvard University Press, 1952), 277-311, 355-360, 363-374.
 21. M. H. F.: "The Critic of Institutions," *Proceedings and Addresses of The American Philosophical Association* 29: 42-56, 1956.