EVIDENCE AND EVENTS IN HISTORY*

LEON J. GOLDSTEIN

The first part of the paper distinguishes between a real past which has nothing to do with historical events and an historical past made up of hypothetical events introduced for the purpose of explaining historical evidence. Attention is next paid to those so-called ancillary historical disciplines which study historical evidence, and it is noted that the historical event is brought in to explain the particular constellation of different kinds of historical evidence which are judged to belong together. The problem of explaining events is then taken up, and an attempt is made to defend the view that such explanation must presuppose general laws. And this is followed by a discussion, partly speculative, of social-historical laws. The final section of the paper tries to argue that the subjective intentions of individuals are irrelevant to historical explanation.

I. Historical Past and Real Past

There is an air of paradox about hearing, as we do from Collingwood, that history is present history. The events that are described in the writings of historians, we protest, did not just happen—today or the day before yesterday. It was long before anyone now can remember that Caesar crossed the Rubicon, Pope Urban preached his crusade, and Nelson died at Trafalgar. The whole point of history is the past, when those events described in history books were really present. Thus, in history idealism is finally routed by realism (cf. [17]).

Yet there *is* a certain plausibility to the idealist view, one which rests upon the distinction between history as a course of events that really happened when the past was present and history as a discipline the purpose of which is to describe and explain the events in question. The latter is clearly an affair of the present. Presumably the realistic critic thinks that by making the distinction and giving the Collingwood thesis a methodological interpretation he has drawn its sting and rendered it harmless. No doubt, it is much improved and no longer paradoxical when so interpreted. But what has the realist left for himself? We shall see that his distinction not only makes the idealist's thesis entirely reasonable, but, at the same time, renders his own entirely beside the point so far as history as a human enterprise is concerned.

The starting point of historical inquiry is a body of evidence. To be sure, historians will tell us varying things about what brought them to their subject, and not least among them is interest in or curiosity about the past. They want to know just what it was that happened at this or that point in the past, both because of any intrinsic interest it may be said to have and because of a belief that knowledge of the past may be put to practical use in dealing with problems of our own day. But whatever the personal motives of historians, these are not to be confused with the actual starting point of history as a discipline, and curiosity about the past—in a realistic commonsensical sense—does not

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establish that past as the starting point, or even the end point, of historical investigation. Inquiry, then, begins with a body of evidence. And the problem of the historian is to explain the evidence.

It is often thought that the historian's primary problem is to explain why something or other happened or why someone did this or that. Actually, there is a previous step and that is, in commonsense language, to determine *that* something happened or *that* someone or other did something. It is often thought that these important facts about the real past are discovered by historians through the means of their evidence. Whatever is intended by knowledge of the past, presumably all would agree that it depends upon historical evidence, yet so widespread is the commonsense realistic view of the nature of history that most philosophical writers simply assume that in one way or another the real past can be revealed and that the major problem is to account for the methods and laws which the historian uses in explaining it.[1] The philosophical problems of history, however, are by no means limited to the explanation of events, and we shall want to consider just how events are established.

When we say that the starting point is the evidence, we mean only that the suspicion that there were events is suggested by the fact that there are present certain things which seem not to fit into the present context of culture and life: writings which most of us cannot read, coins which will buy nothing at the grocery, ruins of buildings and of entire cities, and so on. Some writers (e.g., [28]: 99 ff. and elsewhere) see in these and similar phenomena traces of the past, which when properly interpreted give us some knowledge of how things once were. If taken literally, this view seems to be that the method of history enables us to get to the past, that the traces provide some more or less direct contact between the present and the past. The properly trained historian, then, is one who knows how to find the past in things which are present. Historical inquiry, to be sure, is of the present, but it results in uncovering before our eyes some aspects of the real past. Presumably, this is the point of the distinction to which reference has been made above.

But surely it is odd to talk of having contact with the past in any literal sense. No matter how diligently he has carried out his investigation, the historian never gets any nearer to the real past than he was when he began. The real past, whatever it was, has no more to do with history—the discipline—than Hylas' material substance has to do with the experienced objects that Philonous wants to talk about. In both instances there may be reasons for wanting to believe in substance or in the real past, for one does not want, presumably, to say that either the reality of objects or of human experience is dependent for its existence merely upon the thought of an inquirer or a perceiver. But however one

[1] Though the books and papers on history of Hempel, Popper, Gardiner and Dray are by general agreement among the most important discussions of the subject by contemporary philosophers, their readers will not find in any of them an account of the problem of historical knowledge and, particularly, of the relation of evidence and events. Now that Teggart's *Theory and Processes of History* [32] is once again in print, it may be hoped that more philosophers will take advantage of the opportunity to read its excellent account of precisely this problem.

wishes to effect the avoidance of solipsism, neither the thing as it exists apart from our experience of it nor the real past figures in any human activity.[2]

There is no unequivocal evocation of the past by the evidence, and calling present evidence traces of the past does not affect this at all. It sometimes happens that not-withstanding general agreement on what the evidence is, conceptions of what actually happened differ. The difference is over what the past would have to be in order to best make sense of the evidence. The past that the historian evokes is not a real past as it was when it was present, but rather a construction of his own: not, to be sure, a free creation as in the writing of a novel, or a tendentious creation contrived for the purpose of propaganda,[3] but a construction devised as the best explanation of the evidence he has. The historical event—the only historical event that figures in the work of historians —is an hypothetical construct. The historian does not look for evidence in order to explain the event, as if the event is clearly before him and he is required to make sense of it, but, rather, he calls it forth for the purpose of explaining his evidence. And while one might want to say that the historical construction which most nearly describes the real past is the best or the truest account, how can we ever know? How can we ever test the event except in terms of our evidence? We can never compare conflicting accounts of what "really happened" then and there with anything but our evidence. I am not, to be sure, denying the metaphysical proposition that the past is or was real, but only observing that it is irrelevant to history.

Not even an eye-witness account provides an exception to this. From the point of view of historical research, one should refer to it as "what purports to be an eye-witness account," for that it is such an account is an hypothesis that may explain it. It is the simplest hypothesis for dealing with the account itself, but it may well have to be rejected if there are reasons which make an alternative hypothesis more reasonable.[4] The simplest hypothesis concerning the date of the Old Testament book of Ezekiel—which would take as relevant

[2] Cf. Mead: "The outcome of what I have said is that the estimate and import of all histories lies in the interpretation and control of the present; that as ideational structures they always arise from change, which is as essential a part of reality as the permanent, and from the problems which change entails; and that the metaphysical demand for a set of events which is unalterably there in an irrovocable past, to which these histories seek a constantly approaching agreement, comes back to motives other than those at work in the most exact scientific research" ([20]: 28). The entire chapter from which this is taken, especially the note on pp. 28-31, may be read with profit.

[3] I fear that this is a distinction which is lost upon Mr. MacIver, who seems to think that Collingwood's view that history is present history is of a piece with and philosophical justification for the treatment of Trotsky's role in the Russian Revolution by Stalinist historians ([17]: 200 f.).

[4] My impression, based upon general reading of history books, is that historians will usually prefer this simplest hypothesis, and where other evidence is wanting will assume that their documents are rather exhaustive. For an example of this see Belkin ([3]: 5 f.), who seems to think that because "we know of only one visit of Philo to Palestine" we must assume that he never made others, though so little is known of his private life that his having gone every other spring is entirely consistent with our evidence. For a cogent discussion of this problem see Johnson ([15]: 47 ff.).

evidence the data upon which our knowledge of ancient Israelite chronologies are based and the second verse of the book's first Chapter—would place it early in the sixth pre-Christian century. If one broadens the basis for judgment, that is, if one concludes that an hypothesis concerning the date of the book of Ezekiel must take account of rather more evidence than the two just mentioned, then one may feel the need to offer a somewhat different hypothesis. Presumably one factor in back of scholarly disagreements in the matter is disagreement over what configuration of evidence—and its character—must be taken account of; though it is reasonable to think that most scholars would agree that stylistic, lexicographical and ideological factors must be included in any assessment. And while all would recognize that the second verse of the first chapter is part of the book, it need not follow that the truth of its claim must be accepted. If the only evidence is the verse itself and our chronology, then presumably our hypothesis accepts the content of the verse, but if we broaden the evidential base we are at liberty to offer an alternative. And if we can explain how it is that the verse is mistaken about the date, presumably our hypothesis that it is mistaken is even stronger.

It may prove instructive to pursue this illustration further by presenting, however briefly and inadequately, an alternative that was proposed by the late Charles Cutler Torrey.[5] That this proposal is generally regarded as extreme does not affect its usefulness here. In fact, Torrey's general tendency to be extreme is not a quirk which finds expression in a variety of ways, but actually represents one extreme interpretation of the historical status of the bulk of the later Old Testament books. From that point of view, his work is an excellent exemplification of what I am trying to say about history in this paper. For Torrey's hypotheses are an attempt to offer an explanation for a large body of historical evidence.

The evidence from the biblical book consists not only of its statements, but also of any number of elements that its writer or editor never dreamed they would be contributing to the work of future historians. There is, for example, the character of the Hebrew in which it is written: how does it compare with the character of the Hebrew of other books the dates of which may be less problematic? Torrey is of the opinion that its Hebrew reflects the strong influence of Aramaic and considers that this, among other reasons, supports a later dating than would be warranted were we required to decide merely on the basis of the statement in the first chapter. In addition, there is what, for want of a better term, may be called the ideological element. Torrey thinks the book is rather late—a view supported by several ancient traditions—not only for lexicographical reasons, but because he thinks its ideological content requires that it be treated as part of a group of late works, such as the books of Ezra, Nehemiah and Chronicles. But he thinks an attempt was made to make it appear earlier. The books of Ezra and Nehemiah reveal a conflict between the

[5][33]. It will be noted that in using this work as an example I am not to be taken as endorsing its conclusions. Interested readers may consult Pfeiffer ([24]: 525 ff.) for other theories as well as references to Torrey's critics.

returnees from the Babylonian exile and the Samaritans; and Torrey believes that the purpose of making Ezekiel seem earlier was to establish a continuity of tradition for the returnees to oppose to the manifest continuity of the Samaritan tradition which, of course, had not been uprooted. Whether or not Torrey is right, his argument provides a useful illustration of our point. One does not simply look at the document in order to know what happened. The author or editor of the document which provides the point of departure for Torrey's book may well have wanted his contemporary readers to believe that something was the case which in fact was not. That it was or was not is part of the hypothesis put forward which explains the document and other parts of the evidence.[6]

Scholars who disagree with Torrey—which means practically everyone else in the Old Testament field—would offer different reconstructions. One might say that they offer entirely different historical events as better explanations of the evidence. Indeed, these different events might be radically different from one another, and the various temporal locations which have been proposed for the book of Ezekiel range over hundreds of years. As we come closer in time to the present, divergencies are likely to be less startling, for evidence is apt to be fuller and the possible events which would make it intelligible are fewer. Too, in fields of history where such general agreement exists, the actual starting point of the individual historian is likely to be the professional tradition concerning its character, which is easily mistaken for the real past. But it is clear that what we have said about ancient historical events obtains for later ones as well. When our concern is with late events about which general agreement exists and we seek to fill in details here and there, it is often the practice to look at documents and assume that the details are filled by being drawn from the documents. But one could just as easily say that what is drawn from the documents enlarges our evidence, and the event as enlarged in detail is a slightly improved hypothesis which explains all the old evidence plus the newly added details. This view has the advantage of giving the same status to all events. Not, to be sure, in the sense that all events are equally well established, but rather that from the point of view of an inquiry into the methodology of history all events are hypothetical constructs introduced for the purpose of making our evidence intelligible (cf. [2]).

[6] Cf. Oesterely: "In regard to much that has been said we are prepared for the objection that the evidence of the Old Testament does not offer sufficient justification for the assumptions made. We agree that this is so if we are to rely upon the Old Testament alone. But the object of the whole of our investigation will be to show that the beliefs and practices of any one race of people must, to do them full justice, be studied in the light of analogous beliefs and practices of other peoples. Only so can one fill up the *lacunae* which inevitably exist in the records of the races of antiquity" ([23]: 42). Thus the kind of historical event or reconstruction offered will depend upon what one takes the relevant evidence to be. One may suspect that it is precisely this which is at the heart of the disagreement between philological-literary and folkloristic-comparative religionist approaches to ancient literatures; (cf. [10]: IX ff.).

II. Historical Evidence

In this section of the paper I want just to discuss precisely what is meant when I talk about historical evidence, and what is meant by insisting that concern with it, rather than, say, with the explanation of historical events, is that activity which distinguishes historical research from any other intellectual activity.

Relevant to any theory is a body of evidence which is said to confirm or disconfirm it. From another standpoint, it may be said that the theory in question explains the evidence which confirms it, or fails to explain it if the evidence disconfirms it. Thus, it appears that the term "evidence" is a correlative term; something is not evidence *simpliciter*, but rather evidence relative to some theory or hypothesis. Since we may believe that everything is subject to being explained, there is nothing that cannot be evidence. Yet in itself it is not evidence, or, if you like, "evidence" is not a monadic predicate word.

In using the term "historical evidence," I intend to depart from this usage. It is not that this departure is required for what follows, yet it seems to me somewhat convenient to have the term "historical evidence" refer to a determinate body of data—that very same body of data to which historians make reference when using the term—even when no hypothesis exists for its explanation. Thus it would have been perfectly correct to refer to the very first cuneiform tablets discovered in the orient as historical evidence even though no one had the foggiest idea as to what they were *evidence for*. One could perhaps see that, as the scratchings on the tiles were not produced by nature but were rather man-produced, they could have some possible bearing upon our understanding of man's career on earth.

One walks along and discovers rocks of all kinds, sizes and shapes. Presumably men have always found them, been fascinated by them, and made use of them. Being rocks, they need not be evidence. They do not become evidence—say, for the age of the earth, the geological character of the area in which they are found, or the material cultural attainments of prehistoric communities which may have made use of them—until geological or archeological theories are formulated. So it is with historical evidence; it does not become evidence until historical hypotheses are formulated. Still, a certain class of phenomena may be recognized as being rocks, and another class of phenomena may be recognized as being historical evidence. Thus the word "evidence" in the phrase "historical evidence" does not mean, at least in this paper, what "evidence" ordinarily means. Rather, the logic of "historical evidence" is like, if not actually the same as, that of "rock," for each is the name of a class of objects.

It is with historical evidence that historical research begins, and if history as a discipline may be thought to have made progress over the course of years it is likely owing to growing sophistication in the treatment of historical evidence. It is no longer possible to write Roman history by reading the great historians of antiquity—Livy, Tacitus and their confreres—and recasting the events they

describe in a new narrative form. If the narrative of the historian is, as many of them say, a creative and original act, it is, at the same time restricted by its need to conform to historical evidence.

No one can properly appreciate what is involved in historical research who has not paid attention to those disciplines—so-called ancillary disciplines—which are devoted to the assessment and classification of such evidence. The following paragraph, taken from a work by a historian, will make clear what these are.

> Protracted intensive study of these manuscript sources has given birth to a numerous progeny of highly specialized arts, such as paleography, epigraphy, diplomatics, sphragistics, and heraldry. To the paleographer, a specialist in handwriting, the historian owes the use of many manuscripts which, unnamed, undated, and sometimes undeciphered, would have been closed codices. The study of epigraphy has made inscriptions available for historical purposes. And because the chanceries of Europe had different methods of drafting, signing, sealing, and attesting decrees, ordinances, charters, and other records, a fund of special knowledge had to be accumulated before scholars could identify and interpret the documents. The term diplomatics covers this highly important art; but diplomatists must often summon two other specialists to their aid—the student of seals and the expert in heraldry ([15]: 16 f.; cf. [16]: chs. 1 & 2; [4]: ch. 3).

To Johnson's list one might add numismatics as well as the treatment of artifacts by archeologists. What the practioners of these various disciplines do is classify and correlate historical evidence. Once the scholar has become proficient in his work he has but to look at a document, say, and know where and when it was written. The form of the letters, the character of the dialect, the use of technical or legal terms all give away secrets which would be forever lost if we had to depend only upon explicit dating and locating within texts themselves. That modern scholarship has been able to discover that so many ancient texts are pseudepigrapha is owing entirely to the successful pursuit by scholars of these ancillary disciplines. The simple-minded counterfeiting of coins, as in the Renaissance period, to be passed off as products of classical antiquity is simply no longer feasible, and only a scholar as learned as he is perverse could hope to do it successfully today.[7]

The widely held view that historians are not concerned with generalizations or that history is not a generalizing discipline is entirely untenable in light of the fact that these ancillary disciplines, *which are the peculiarly historical disciplines*, are clearly generalizing. It is, of course, true that the generalizations they use are often unformulated, and certainly never formulated with the precision that characterizes generalization in physics. Yet they are generalizations for all that. And they have had much successful application to their credit. I suspect that there are some branches of the study of documents or coins for which the generalizations require very little qualification, and the

[7] With all the progress that has been made in the application of physics and chemistry to the dating of artifacts and other finds, even this may no longer be possible; cf. *New York Times*, January 1, 1960, pp. 1, 8.

result of their application may be very striking indeed. Again, we may quote Johnson.

> The results achieved by these laborious studies are often amazing. The German historian Giesebrecht, for example, pointed out that certain chronicles of the eleventh century drew their information from a common source which was no longer available. So sure was he of their dependence upon this early chronicle, which he name d *Annales Altahanses*, that he constructed the missing manuscript from these later derivatives. Some twenty-six years later the missing chronicle was found, and it confirmed these shrewd conjectures in every important particular ([15]: 17).

Such results show clearly how well established is knowledge of the writing of the period in question. On the basis of what he knew about how works influence subsequent ones, our historian was able to propose that a bit of historical evidence of determinate character must be assumed in order to make sense of certain pieces of historical evidence which were known to exist. In its own way, it is no less impressive than the anticipation of the discovery of the planet Neptune, particularly inasmuch as it reflects a degree of systematization in historical research of which many of the philosophers who write about history seem unaware.

In the course of working with the results of the various ancillary disciplines, historians discover a kind of convergence. By this I mean that certain documents seem to belong with certain artifacts, coins, and, if the period is sufficiently ancient, ruins. There emerges in the course of research a constellation of kinds of historical evidence. To account for the particular constellation becomes a question of some interest, and it is at this point that the historical event enters into consideration.[8] It is the function of the event to explain the evidence, that is, it must make intelligible the grouping together of some particular constellation of historical evidence which is believed to belong together. We have seen that differences between historians often stem from failure to agree on what the proper constellation is. And even were such agreement to be had, some disagreement could result from the incompleteness of the evidence. Different reconstructions may begin from and be consistent with the same evidence,[9] but would have different implications so far as evidence not known or not yet known is concerned. Should new evidence turn up, it would be possible to choose between differing reconstructions or propose still a new one (cf. [11]: 475). But, even then, we would have no reason for taking the historian's event to be a description of a real event.

[8] In prehistory, the constellation of correlated finds is taken to represent a stage of cultural development, but we sometimes discover a realistic tendency to think of the stage as independent of the constellation of evidence and to treat it as real even when newer evidence leads to the breaking up of the original constellation. If one thinks of the stage as an hypothesis to explain a particular constellation this would be less likely.

[9] Though not if the view that reconstructions are deduced from the evidence were correct, for in that case agreement about evidence should entail agreement about events.

III. Explaining Events

Though we have seen that historical events are constructed for the purpose of explaining historical evidence, it is possible to treat such events as descriptions of states of affairs. And one may wish to have an explanation of the state of affairs quite apart from our recognition of the function of the historical event in explaining the evidence. It does not seem that this problem is in any way different from attempts to explain states of affairs in general; that is, that they are states of affairs described by historians, hence not capable of being witnessed by anyone now, does not seem to raise any special problems. They may be classed with states of affairs described by present-day sociologists or anthropologists, journalists, or just perceptive eye-witnesses. That an historian finds it best—given his historical evidence—to believe that a state of affairs of determinate character took place *at some past time* in no way affects the character of the event. Presumably, what will explain it would have explained it no matter when it happened, and if events of like character happen in our own time, like explanations will be required.

If one distinguishes between the phenomenal panorama passing before the eye-witness and the report he makes of what he observes, one could argue that the latter is rather like our historical event in that it is possible to treat it as explaining the phenomenal panorama. Likewise, a sociological or ethnographic report may be treated as introduced for the purpose of making sense of what the sociologist or ethnographer saw or experienced. In all of these cases there seems to be a certain parallelism, and one is at liberty to conclude that all events as characterized in reports are hypothetical. The difference, between historical events and other kinds of events seems, then, to be even less, than we commonly think. But it would take us too far afield to explore this here. It will suffice simply to note that when we are concerned to explain an event as a state of affairs we are not required to make distinctions of a logical character which depend upon the event's temporal location.[10]

As has been noted earlier, the problem of explaining historical events is precisely one of the problems of history which has received most attention from philosophers in recent years. The question to which most inquiry is directed is whether or not such explanation requires that reference be made to general laws. I do not think it appropriate to burden this rather long paper with a review of what has been written on the subject, but will, rather, turn directly to the task of presenting some views of my own.

Let us assume that we are watching two little boys at play, and that suddenly one punches the other in the nose. The victim of this outrageous assault proceeds to burst into tears. Further, assume that soon thereafter someone else comes up, sees the crying child, and asks us why he is crying. We say that

[10] While one frequently hears it said that concern with time is especially characteristic of history—(cf. [30]: 24)—this can only mean that historical events are ordinarily understood as not being instantaneous. Temporal location, however, is simply a specific or more determinate form of existence, and if existence is not a predicate neither is temporal location.

the other punched him in the nose, and intend by this to answer the question. Is it, however, an explanation? Are we not, perhaps, required to connect crying and being-punched-in-the-nose before we may claim actually to have explained the crying? Michael Scriven would say that the answer given is a complete explanation of the phenomenon to be explained, and that we are required to separate the explanation itself from any justification of the explanation, which is what connecting the two would be. He believes that the former may be had without the latter ([29]: 445 ff.).

But rather than having someone else come up and witness the crying of the child, let us assume that instead it is a creature of some sort who has never before witnessed the phenomenon of someone crying, and let us assume that it asks the question. (For economy of expression, I shall use the made-up word "hume" to mean "a creature of some sort who has never before witnessed the phenomenon of someone crying"). Surely the answer does not explain the phenomenon to hume. To hume, the answer may seem very strange indeed; he asks about crying and we tell him that someone was punched. For hume the the connection is by no means obvious, for either one may be thought about without involving or entailing the other. Far from being a truism, as Scriven thinks, the answer seems beside the point.

It will not do to dismiss this as a silly and improbable example. The difficulty of hume could easily be extended to every so-called truism or common sense explanation of an event, historical or other. If the answer to the question is actually an explanation it is only because implicit in it is the generalizing of our experience with being punched and crying. Too, if anyone thinks that reference to his cupidity explains Cortez' decision to make a third expedition into Lower California—to use one of Scriven's examples—it is because we know something about cupidity and its effect upon the actions of men. Thus, it emerges that we accept an explanation as explaining only if we can justify it. To the extent that we cannot justify it, it remains problematic or doubtful or even unintelligible.

An alternative to the kind of justification offered above would require that the denial of the necessary connection between empirical concepts be itself denied. The mind might be said to grasp intuitively that between the "truism" and the event explained there is a direct connection, and this intuition in no way depends upon previous experience in order to be discovered. I do not know how many of the recent critics of the view that explanation in history presupposes general law would be willing to subscribe to a view such as this,[11] and it may well be that, in his firm present grasp of the meanings and uses of the truisms he discusses, Scriven has simply forgotten that he did not always know them.

The claim, then, of some philosophers that in history, as in any discipline, explanation requires reference, at least implicitly, to general laws, does not

[11] It may be that Mandelbaum does, or at least did at one time; (cf. [18]: 206 ff.) and see my [11]: 467 ff.

arise out of a study of historical writings, but is rather a consequence of a certain position in epistemology, a position traditionally associated with the name of David Hume. Thus, the fact that historians do not as a rule[12] seek to formulate generalizations cannot be taken as an argument against the view, as Dray seems to think it can ([8]: 11 f., 139, and elsewhere). One might have wondered before raising such a point how it is that such methodologists as Hempel and Popper, who surely know that historians do not generally formulate laws, nevertheless persist in their view. It seems clear that neither one of them considers this fact about historians to be pertinent, and that is because ours is a problem in epistemology, not historiography.[13] On the epistemology of empiricists no intuitive connection between an event and any other event is descernible. If one wants to attack this view, one ought to do it outright and not obfuscate a philosophical issue by appeal to historiography.

Such an appeal does not actually lead to a resolution of the question. It is, of course, the case that historians generally do not specify laws, but rather seek out antecedent causes of a highly specific character. From the empiricists' standpoint, however, the claim that this specific event led to or caused the event we seek to explain implicitly makes reference to general laws because on their epistemology there can be no other way of justifying the claim that the two events are connected. It may be that the reason why the epistemological basis for the explanation-by-general-laws position is never taken up is that such of its critics as Dray and Scriven are themselves in the empiricist tradition.

The outstanding accomplishment of physical science has resulted in that philosophy of science has tended to the view that it must analyze the logic and procedure of science, but must be careful not to dabble with matters of a substantive character. It is certainly the case that only one of unusual audacity could presume to challenge the material knowledge claims and methods of procedure of physics without first becoming thoroughly grounded in that subject himself. It is habits of work which come from the philosophy of physical science that writers such as Scriven bring to their treatment of explanation in history. Just as one starts the philosophy of physics by careful study of what physics has accomplished, so one begins philosophy of history—in the critical rather than speculative sense—by becoming familiar with what historians have done. And historians have not been formulating general laws. To say they do would be erroneous; to say they should presumably presumptuous. Yet neither history nor social science has so impressive a record as physics, and it may be doubted that there is much that is worthwhile to be derived today from a philosophy of social science which adopts the scrupulous hands off policy of

[12] See [31] for an interesting exception to the rule.

[13] Cf. Donagan: "It would not follow that the Hempelian theory is mistaken even if it should turn out to conflict with historian's or common reader's estimate of recent historical work." That he adds "Berkeley's criticism of fluxions has worn better than the replies of contemporary mathematicians," may mean that he is only suggesting that the Hempelian theory may be a sounder account of historiography than that of most historians, which is not of course, the point that I am making in the text ([6]: 145 f.).

philosophy of physics. We must be prepared to say what we think history and social science lack no matter how much we may hesitate to speak similarly about physics.

With this much said, let us now consider the character of historical or sociological laws. Things being what they are, some of the next section will be speculative.

IV. Social Laws

Those who have criticized the possibility of the application of general laws to the study of history have often buttressed their views by pointing to the simple-minded or trivial character of the laws that would be involved. Popper, whose views on explanation require the utilization of general laws, has insisted that they must be trivial, presumably because of his belief that any non-trivial law in history would be historicistic.[14] Thus, he suggests that one way to explain the partition of Poland in 1722 would be by application of a law such as "If of two armies which are about equally well armed and led, one has a tremendous superiority in men, then the other never wins" ([26]: 448). He allows that "never" may be interpreted as "hardly ever," no doubt to allow for such happenings as the bigger army marching into quicksands.

Not significantly different is Michael Scriven's conception of the character of laws that might be used in history, if any were at all. In considering ways in which we might explain his aforementioned example of Cortez' decision to make a third expedition to Lower California, he thinks that advocates of the view that general laws are relevant to explanations in history would have to consider the following: "All confident wealth-seeking people undertake any venture which offers very great wealth" or "All confident people seaking very great wealth undertake any venture which offers very great wealth," or, finally, "All confident people with Cortez' background of experience, seeking very great wealth, undertake any venture involving the hazards of this one, which offers very great wealth" ([29]: 454). In what follows, he argues that the most general of these—and presumably of any set of such statements no matter what historical event or set of events they were introduced to explain—is most likely false, whereas the one least likely to be false, because of its increased specificity, is hardly worth formulating. But for all their differences, the three are alike in showing that Scriven considers that general laws in history could only be generalized from the particular event to be explained. It is no wonder, then, that he can think only of trivialities. Of little interest, likewise, is Gardiner's conception of a law adequate to explain the unpopularity of Louis XIV at the time of his death,[15] and Gardiner is one of the defenders of the general laws-in-

[14] In his sense of "historicism" as belief in laws of development rather than in Mannheim's sense as the social determination of thought. That Popper has failed to distinguish these will be apparant to any who compare Mannheim's [19] with Popper's caricature of it in [27].

[15] ([9]: 82-90); another example of this kind of thing is to be found in ([22]: 303), and the notion of "Hempelian" general laws implicit in Donagan's criticism of them ([6]: 161) is of like

history point of view. In sum, the kind of example that philosophers have made up are too specific to be interesting, and this seems to be because of their view that laws in history would have to be fairly direct generalizations of what they explain. This, of course, is never explicitly stated, but it is clearly implicit in the examples they make up. Since each historical event is in many, if not most, ways qualitatively different from any other, it is no wonder that the program implied in the view that general laws are required or presupposed in history—as everywhere else—seems not worth taking time about.

For a more serious consideration of the character of laws in history, we may turn to the work of the late Frederick John Teggart. An intensive study of records bearing on certain wars and uprisings in antiquity leads Teggart to the conclusion.

> ... that between 58 B.C. and A.D. 107 barbarian uprisings in Europe were preceded invariably by the outbreak of war either on the eastern frontiers of the Roman empire or in the "Western Regions" of the Chinese. Also it has been found that the invasions which followed disturbances in the Roman East occurred both on the lower Danube and on the Rhine, whereas the uprisings which followed disturbances in the T'ien Shan affected only the upper Danube. Further, there were no uprisings in Europe which were not preceded by the respective disturbances in the Near or Far East, and there were no wars in the Roman East or the T'ien Shan which were not followed by the respective outbreaks in Europe .... ([31: 236).

Teggart claims that there are no known exceptions to these conclusions and "in spite of the unsystematic character of the Roman sources and their imperfect preservation," evidence is lacking for the correspondence of events in only a small number of cases ([31]: 237).

The application of Teggart's discovery is very limited, indeed, and we shall discuss this problem shortly. But two things may be remarked about it at once. It is general without being merely the generalization of a particular event. And it is not trivial. It is only in light of it—or something rather like it—that it makes sense to say that evidence of a determinate sort is missing, in this case evidence that would establish that the uprisings we conjecture because of Teggart's hypothesis, but know nothing more about, might be spoken of more confidently.[16] Too, should evidence turn up of a war or uprising similar to those characterized by Teggart but somewhat earlier or later than the span of time covered by his investigation, it would surely be reasonable to try to find a war or uprising of the kind that Teggart correlates with it.

There are obvious limitations to Teggart's hypothesis. It cannot, for example, be used in explaining wars or uprisings much before or after the span of time Teggart mentions, nor in explaining wars or uprisings which took place during that period but in other parts of the globe. It is, in other words, re-

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character." For a more serious consideration of the character of the laws that would explain Gardiner's example see [25]:583 ff.

[16] I.e., evidence an explanation of which would be an historical event of the kind that would confirm Teggart's hypothesis. Of course, we could conceivably find evidence of an opposite kind, the explanation of which would be an event which would disconfirm the hypothesis.

stricted in its scope both spatially and temporally. In this, however, it *resembles* Kepler's account of planetary motion, the scope of which is restricted spatially to our solar system and temporally to the period of time between the beginning and ultimate end of that system.

Related to its limitation of scope is the fact that its formulation is likely to evoke a feeling that the hypothesis cannot stand by itself, but rather that something ought to be done to make it intelligible. If theories of more general character could be introduced into this field, not only would our perhaps reasonable hesitation about accepting Teggart's hypothesis be overcome, its limited scope would cease to be an issue at all.[17] Here, too, we see a resemblance or parallelism of sorts between Teggart's account and Kepler's, the more general laws of Newton doing for the latter what more general laws in the social-historical field might do for the former.

Teggart was himself fully aware of the problem, as the following paragraph from his study makes clear.

> The establishment of correlations in historical events does not, indeed, solve the problem of the recurrent invasions in central Europe, but it does define this problem in new and explicit terms. How, then, are we to account for the circumstance that Roman wars in Armenia occasioned barbarian uprisings on the lower Danube and the Rhine, and that Chinese wars in the T'ien Shan occasioned similar outbreaks in Hungary? Now, stated in this form, the problem assumed an unexpected aspect, and in an embarrassing manner called for inquiries of a sort not represented in the chronological data previously compiled. In other words, the focus of inquiry shifted from the critical examination of dates to an explanation of the possible linkages, considered geographically, between peoples situated in Armenia and in Rumania, separated as they were by the Black Sea, and between peoples situated in the T'ien Shan and in Hungary, separated as they were by Dzungaria, Western Siberia, Russia, and Poland ... Suffice it to say that the outcome of this phase of the undertaking was the conclusion that the correspondence of wars in the East and invasions in the West was due to *interruptions of trade* ([31:]IX, his italics; cf. p. 239 ff.).

Teggart's suggestion here is that the various barbarian peoples involved depended upon contact with their civilized Roman and Chinese neighbors in order to procure needed supplies and that these sources were cut off during times of war. This forced them to make uprisings against or incursions into the settled territories of the Roman Empire. This explanation is still formulated in terms about as restricted in application as the original hypothesis discussed above, yet if it has serious explanatory force it would have to be—as, surely, it can be—formulated so as to apply more generally. Analogous economic relationships between large and civilized peoples and barbarians may be found elsewhere, and the newer explanation could conceivably be tested against a body of data wider than that provided by Teggart's own study. And, clearly, if the more general proposition tends to be confirmed, the intelligibility of the narrower hypothesis is given some degree of additional support.

[17] Failure to discover such more general laws would not, however, deprive Teggert's account of such independent worth as it may have.

It is impossible to anticipate the degree of generality that may ultimately be attained in the formulation of historico-social theories, but we may speculate about certain possibilities even if these remain, in the end, unrealized. Even in the more general of the theories mentioned above, the restriction in its range of application is apparent. It deals, or would if anyone troubled to formulate it, with a highly determinate kind of event, and if it may be applied to other times and places than those with which Teggart is concerned, it does not begin to deal with the wide variety of socio-historical phenomena in which we are interested. May we not conceive of at least the possibility of formulating social theories which are less specific in character, so general that in fact no specific kind of event is mentioned in their formulation?

We may begin by considering another social theory, also rather specific in its application yet, like Teggart's, in no way trivial. G. P. Murdock's book, *Social Structure*, is concerned to explain how systems of kinship nomenclature develop and change. It would take us too far from our present problem to discuss here the precise character of this problem in cultural anthropology, and the way in which Murdock deals with it. It will be sufficient to observe that he thinks that there are basically four aspects of the social system which are relevant to the development and change of kinship systems: rules of residence, of descent and of marriage, and the form of the family; whether or not this is adequate, need not concern us. The problem is to see how the various forms that these four can have affect kinship systems. Before presenting his evidence for this, but after a detailed discussion of the phenomena involved, Murdock offers what he considers to be a very general formulation of his theory:

> The relatives of any two kin-types tend to be called by the same kinship terms, rather than by different terms, in inverse proportion to the number and relative efficacy of (a) the inherent distinctions between them and (b) the social differentials affecting them, and in direct proportion to the number and relative efficacy of the social equalizers affecting them ([21] : 138; original in italics).[18]

In the context of Murdock's investigation, "inherent distinctions" and "social differentials" refer to social factors which lead to the distinguishing between kin-types; "social equalizers" refers to factors which result in distinct kin-types acquiring similar roles.[19] But surely these notions have applicability to a much wider range of social phenomena. One may suspect that social scientists could provide long lists of situations which might be characterized

[18] An instance of two kin-types called by the same kinship term in our society is mother's sister and father's sister. Whether or not Murdock's use of "inverse proportion" and "direct proportion" is to be taken seriously, need not concern us here.

[19] In a matrilocal society, say, the sister of a person's mother may often be able to do many things for or concerning him that his own mother might do, and this social equalization may result in one term being used both for "mother" and "mother's sister." In such a society, one's father's sister lives elsewhere and does not do the things in question. Thus, between mother's sister and father's sister there is social differentiation, they are referred to by different kinship terms, and our category *aunt* does not apply.

in terms of such ideas. Then why, one may wonder, might we not expect that all of this might be unified by some system of theory in which these notions would be used and within the formulation of which such more restricting terms as "kin-types" and "kinship terms" do not appear? Presumably such a more general theory would explain the more limited theory constructed by Murdock—or if not his something like it—and much else besides. I do not say that this will ever actually be done, nor have I any idea at all concerning what such a theory would be like. But I can think of no argument that leads me to think it impossible, nor must it of necessity be so obviously false as the more general of Scriven's three examples.

As long as we are speculating about unrealized possibilities, one may wonder about the possibilities of general theories using the concept of anomie. Since Durkheim introduced this notion into sociology (cf. [7]) social scientists have characterized a wide variety of social and psychological phenomena as anomic, that is to say, reflecting a breakdown of norms, the realization that expactations cannot be fulfilled or that one's situation cannot be structured in terms of the norms and values to which one has become habituated. Sebastian De Grazia's *The Political Community* will provide the interested reader with many examples of this sort of thing, entirely apart from whether or not one subscribes to his point of view. If it should prove possible to unify theoretically the wide variety of anomic phenomena, presumably we would have a theory of great generality within the formulation of which no mention would be made of particular kinds of anomie, just as we suggested for the ideas discussed in the previous paragraph.

We are now in a position to consider the circumstance under which an historical event may meet the requirement that it offer an adequate explanation of historical evidence and yet have to be rejected on other grounds. To be sure, an event may be rejected because its rejector thinks that the evidence it explains is properly part of a wider configuration of evidence, a view perhaps not shared by its original formulator and others who accept it. But considerations such as this have been dealt with above, and I want to end the present section with consideration of another point. Should the possibilities concerning which we have just been speculating ever approximate realization, it may well be the case that certain logically possible historical reconstructions, which seem admirably to account for the evidence, must be rejected on the ground that their acceptance would be in conflict with acceptance of fairly well established theories. Thus, we might have theoretical reasons to choose between two or more conflicting reconstructions, reasons which we do not have today. One can imagine that such reasons would be of great value for research in ancient history where evidence is sparse and conclusions somewhat tenuous.

It may be worth noting that even in the present state of social theory there is at least one known instance of this kind of possibility. Murdock notes that while it is logically possible for an amitolocal rule of residence—one according to which upon marrying the couple settles down with the husband's father's sister—to exist, in point of fact none has ever been discovered ([21]: 71). Sub-

sequently, Ward Goodenough argued that there are good reasons for believing that this logical possibility can never actually be realized, that the realization of the rule is nomologically impossible [13]. Thus, if there were scraps of data —perhaps owing to incompetent ethnography—which might be explained by supposing either that the society in question had an amitolocal rule of residence or some other determinate rule, if we could reject the former on nomological grounds, the claim of the latter would be strengthened.

V. Subjective Intentions and the Explanation of Events

In this final part of the paper, I should like to consider the view that historical explanation, at least sometimes, makes reference to the intentions of an agent, an historical person of importance ([8]: ch. V). It would be far, indeed, from my purpose to suggest that the decisions of such persons are not important and even intrinsically interesting. But I do intend to say that knowledge of this kind cannot be offered in explanations of historical events. It would explain only how it was that the agent in question initiated that chain of happenings which is the event. At the very least, the notion that an event is explained by reference to the subjective intentions of the agent presupposes that the best laid schemes o' mice an' men *do not* gang aft agley. If one cannot explain the defeat of the Spanish Armada by reference to the subjective intentions of its commanding officer, I do not see how one can expect to explain the victory of the English in terms of the subjective intentions of its commander. Such subjectivity may well enter into an account of the strategy which each officer sought to carry out, and a student of naval tactics might be very much interested in determining what led each of them to make the decisions he made. But having arrived at the end of this inquiry, our student of naval tactics has still not offered an explanation of the course the battle took.

The notion that an "historian's explanation ... will involve a detailed examination, *mainly in rational terms*, of the activities and motives of countless individuals and groups" ([8]: 142, italics added), is clearly a confusion of psychology and history, the kind of confusion one associates with a theory called "methodological individualism."[20] Such a view, with its inability to

[20] See [12]. While methodological individualism purports to explain social phenomena in terms of individuals, it is hard to tell these days just what its most active recent proponent, Mr. J. W. N. Watkins, is trying to claim. From a recent note on the subject ([35]) one may suspect that his individualism is non-methodological and his methodology non-individualistic. He insists that only individuals exist, which is an ontological and not a methodological point, and that social explanation can be in terms of anonymous individuals displaying anonymous dispositions. But these individuals and dispositions are in no way individualistic, being entirely social in character; cf. my [12]: part 2). Only a perverse logic could take my remark, near the end of the aforementioned paper, that "there is no science of the anonymous," as denying that in the formation of theories we do not make reference to actual particulars, a denial which would seem to make *me* a methodological individualist. If on Watkin's methodology we can explain *only* in terms of individuals, and if he thinks we can explain by reference to anonymous individuals and anonymous dispositions, then he treats the latter as

distinguish between the individual and the non-individual sociocultural content and context of his life and experience, is entirely incapable of appreciating the possibility for developments of the kind speculated about in the previous section and can never do more than describe the behavior of individual behavers. In our present context, such a view confounds the initiation by an individual of a course of action or event with the event itself.

Actually, this last is but one of two possible interpretations of the view in question, but inasmuch as the other is so improbable it seems reasonable to suppose that none of the explanation-by-reference-to-subjective-intentions theorists holds it. It seems that according to some theological cosmogonies, the creation is never a fully completed event after which the universe continues on its way, each subsequent state of which being accounted for by reference tot he laws established by the creator and a state description of the universe at its previous state. Rather, on this view, during each successive stage it is sustained by the care of the creator, who is in some way taken to be the causal agency of the changes within it. It seems to me that the only alternative to what I have said about the view that historical events may be explained by reference to subjective intentions in the previous paragraph, is an historical analogy to this cosmogonical theory. One must imagine that the initiator of the action "keeps control" of it throughout the entire course of its duration. This theory is even more complicated than the theological one, however, because presumably some way must be found to harmonize the "control" exercised by the initiators of overlapping events—such as the winning by the English and the losing by the Spanish of the battle of the Armada.

Since no one wishes to hold such a position, we need consider it no longer, and will simply assume that it is the failure to distinguish between the initiation and continuation of an event that is in question. That which follows from the initiation of the action follows not merely because the action was initiated, but because it was initiated under the particular condition which obtained at the time. Initiating actions of such character, quite apart from the subjective intentions of the initiator, in contexts of such character have certain results, and in any given case the results constitute the event the historian describes or constructs. But to explain it, we are required to know considerably more than the motives, intentions or dispositions of an agent. A full explanation must presumably await the satisfactory formulation of socio-historical theories.

It may be noted that the view of this paper does not make the initiation of

being not conceptual or theoretical but as real entities. The point of my remark, as is obvious in its context, is precisely to insist upon the conceptual rather than individual character of the anonymous. Watkins likes to say that he has consistently defended the same position over the years and that I keep missing its point, yet any comparison of this recent note with his characterization and defense of "individualistic ideal types" in [34] will show that this is mistaken. If attempts to treat his anonymous individuals and dispositions as individualistic, in the way his preferred ideal types are taken by him to be, are not properly representative of his views, then perhaps he is no longer a methodological individualist even if he insists upon the title. One may venture to suggest that his words are like the handiwork of Socrates' ancestor Daedalus.

the course of action by an individual agent irrelevant. Nor does it require that the individual initiation be explained away so that it cannot be taken to be a genuinely new departure in the socio-historical scene. Our view is only that the new departure becomes an element in a context, and its effects within that context are amenable to explanation within a possible system of socio-cultural theory. Such theory would be non-individualistic, but it would not be historicist in Popper's sense. The theory could explain what happens in history, but it would not require that what happens happens of necessity. Our explanations are hypothetical, and there is no reason why our "if" clause cannot, in part, assert that such-and-such was done by so-and-so. Our explanation of so-and-so's doing such-and-such would have to take account of motives and intentions. Our explanation of the historical event which follows from the initiation would not.[21] The construction of the event is often, perhaps mostly, quite independent of the account of its initiation, and could be explained even if on the basis of available information the intention of the initiator could not be explained or even determined.[22]

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[21] Though made in a context different from ours, some remarks of Hempel seem apposite at this point: "This kind of information about purposes and beliefs might even serve as a starting point in explaining a self-regulating feature in a human artifact. For example, in an attempt to account for the presence of the governor in a steam engine, it may be quite reasonable to refer to the purpose its inventor intended it to serve, to his beliefs concerning matters of physics, and to the technological facilities available to him. Such an account, it should be noted, might conceivably give a probabilistic explanation for the presence of the governor, but it would not explain why it functioned as a speed-regulating safety device: to explain this we would have to refer to the construction [noun, not verb—L. J. G.] of the machine and to the laws of physics, not to the intentions and beliefs of the designer" ([14]: 299).

[22] As Banton has said in another connection, "We can never with certainty infer the actor's intention from observing his behaviour" ([1]: 17); cf. [32]: 23, 78 and ch, vi.

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