

**THE PRINCIPLES OF HISTORICAL
EVIDENCE CONSIDERED IN THEIR
BEARING ON THE HISTORY OF
REMOTE TIMES; THE ARNOLD
PRIZE ESSAY FOR 1868; PP. 1-37**

Published @ 2017 Trieste Publishing Pty Ltd

ISBN 9780649259007

The Principles of Historical Evidence Considered in Their Bearing on the History of Remote Times; The Arnold Prize essay for 1868; pp. 1-37 by W. H. Simcox

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Edited by Trieste Publishing Pty Ltd.
Cover @ 2017

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W. H. SIMCOX

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(58)

*THE PRINCIPLES OF HISTORICAL EVIDENCE
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HISTORY OF REMOTE TIMES.*

THE ARNOLD PRIZE ESSAY FOR 1868.

BY THE
REV. W. H. SIMCOX,
FELLOW OF QUEEN'S COLL.



OXFORD:

T. & G. SHRIMPTON, BROAD STREET.

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ANALYSIS.

When a science is defective, the temptation to give it a delusive completeness is felt especially by the most zealous students of science.

Methods proper to the deductive Science of Historical Evidence, when evidence is abundant and intelligible :—

When obscure, corrupt, or ill-authenticated.

- In the latter case, the evidence may be supplemented by historical analogies ;
Which must be held legitimate ground for positive reasoning, without any assumption that all history is reducible to physical laws.

But the field of material is narrowed by the rarity of true historical analogies.

Degeneracy of the Niebuhrian deductive school into arbitrary word-splitting, uncritical use of authorities, groundless plays on numbers, and the like,

Rise of the school of interpretation of comparative mythology : uncertainty of the range of its application.

Inadequacy and inappropriateness of either *a priori* method, when pursued singly and not in conjunction with the critical examination of evidence.

Four classes of evidence for remote or uncertain history :—oral tradition, monuments and inscriptions, fragments of ancient literature, and later compilations.

- I. Tradition is trustworthy for a considerable range of time, in the rare case of civilised nations without written history : for a generation or two in nearly all cases.
- II. Inscriptions, supposing them to be rightly deciphered and interpreted, have the highest value direct and indirect.
But their interpretation is the subject of a special study, which has its own laws, though the laws of historical criticism must also be applied.
Other monuments are useful for confirming tradition if otherwise credible.
Ancient customs, for confirming even traditions that might otherwise be doubtful ; but this last only in special cases where a special origin for the custom must really have existed.

III. The date of ancient literature must be discovered from philological notes, which are a vague guide; and from representations of society which are scarcely less so: rarely from notices of known facts.

Homer's date has an inferior limit supplied by the oldest Greek inscriptions.

His stories may have an historical element, but we cannot determine it. Professed history may be trusted when contemporary: though even there reconstruction from fragments is dangerous.

Works really ancient should not be held to be forgeries later than their professed date, unless on the strongest evidence.

When not contemporary, their date must be determined, if possible, and their matter treated as the tradition of their age.

But their testimony in this case is always doubtful.

IV. In correcting an historical compiler, we must discover by literary criticism what his materials were, and what is the amount and character of what he has added to them: we shall then treat his materials according to the method proper to materials of that class.

Though our conclusions are never demonstrative, this is no reason either for pretending that they are so, or for abandoning them.

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ACCORDING to Aristotle, all men naturally desire to know and when it is possible to gratify the desire, wise men do it. But, unhappily, knowledge is sometimes unattainable; most frequently, perhaps, in the very subjects where it is most desirable. Accurate knowledge is doubtless in all cases hard to attain, and easy to counterfeit; but the wise man is prepared to overcome the difficulty,—to detect the counterfeit and pursue the reality. It is his aim and profession not to rest contented in ignorance, or to allow to unverified conjecture more than a provisional value. But there must be a certain amount of materials for science, if science is to exist. From obscure facts in confused arrangement, it may be possible to elicit truth and system; but where the investigation has to proceed on *data* not only obscure but uncertain, not only confused but incomplete, the proper work of science cannot be done, or can only be done imperfectly. And when a subject is in this state the danger of false science arises, because here the man of science has a peculiar temptation. The scientific spirit is of its own nature loth to acknowledge a repulse,—to admit the existence of limits to its own powers of conquest and comprehension. Doubtless it is bound by its own nature to acknowledge such limits where they do really exist; but the scientific man is inclined to evade the obligation imposed on him by his profession, where to yield to it shocks a desire which his profession inspires; for in this evasion there seems to be no infidelity to the spirit of the profession itself. In a subject where there are not materials for a perfect science, positive and demonstrative, there may yet be enough to enable us to trace a tentative outline of a science: if any one manipulate the materials so as to promise more, his work will be tested by the men who understand the laws of the scientific method, if they have any hope that in obedience to those laws they may reach a more stable result. But they may be deluded into reluctance to apply the *eleuchus*, if the reasoning is specious and even sound up to a certain point, and if it is evident that no other method can ever prove fruitful. Like Admetus, the man of

science, when robbed of the object of his affections, consoles himself with the *ψυχρὰ ἔρψις* of embracing her image,*—an image whose sculpture must be his own work, since no one else knows her true features so faithfully as he.

This seems to be the only way of accounting for the phenomenon of a seeming science, founded and pursued by men of genuine scientific temper, and accepted as certain by a generation of scientific critics, yet found at last to be illusory, both in method and results. But for such considerations as these, it might seem presumptuous to reject a system so firmly established as the Niebuhrian historical criticism lately seemed to be. It is unquestionable that Niebuhr had a real insight into the principles of the course of ancient history; if we dissent from his method of tracing that course, we may be required to show how it was that his insight served him in no better stead—that it even led him into definite error, because it supplied him with the conception that historical certainty might be based not on evidence, but on deduction. It is true that in all cases the science of the principles of evidence must be deductive; but there are variations, some legitimate and some more questionable, admitted in the method that is held proper to different problems of the science. The evidence before us may vary in amount from the fullest to the scantiest, in an approximate proportion to several elements of uncertainty, one of the chief of which is the remoteness of the events in point of time. Now, when the evidence is extensive, it is almost always possible to attain moral certainty as to the real facts; the difficulties in the way of doing so, however great, arise from the redundancy, not from the absence, of materials for forming opinions. There is in this case a double process to be performed—a weighing and a sifting of the evidence.

As to the first, no very definite rules can be given. A few broad principles are familiar enough: that eye-witness is better than hearsay; yet that a well-informed bystander may be more trustworthy than an actor, and the like; but anything more special than these, or even the application of these to individual cases, it is impossible to bring into scientific form. The practical sagacity which judges that one man is more intelligent than another, or better informed on the matter in hand, or more unprejudiced, disinterested, or straightforward, as it does not proceed by rule, so cannot be reduced to rule: its operations are too subtle for analysis. But when the value of the external attestation of various facts has been determined, the real work of deduction commences. We first assume the truth of every fairly-attested statement as *prima facie* probable; then we examine the self-consistency of each, its agree-

* Euripidis *Alcestis*, v. 353.

ment with those of other witnesses of equal or greater authority, the presence or absence of the results which might be expected on the hypothesis of its truth; and by a comparison of all these we are enabled to form an opinion of our own, intelligent, though not infallible, which shall embody and reconcile as far as possible all the facts contributed by the various witnesses. Lastly, this opinion must be tested by comparison with each item of the evidence it rests on; we have to ask the question, "If the facts were such as we suppose, would the account that this and that witness give of them be such as it is? would this and that fact be represented to us in this or that way?" Sometimes the verification is perfect and simple: all the matters of fact attested are seen to be real and consistent with each other, and the true state of the case is found by merely combining them and their necessary consequences. But even in the more difficult cases, where some of the evidence that seemed credibly attested in itself has to be rejected, we may yet find our hypothesis satisfactorily verified. The stories told, let us suppose, by some of the witnesses are self-contradictory, or contradicted by others. Does our hypothesis show cause for confusion in their minds, or motive for prevarication in their evidence? Or does it account for difference of opinion between them, owing to imperfect means of observation, or to prejudice or interest that might colour their views? Some circumstances alleged are rejected as inconsistent with others: are these such as might have been imagined or invented by those who relate them? Others are supplied merely from conjecture: are these such as might, even if real, have been unknown or unrecorded by our informants? Do we, as far as possible, accept that testimony which bears the higher authority, and only sacrifice it in cases where the internal probability and its own inconsistency with certain facts are really decisive against it? Do we avoid in all cases relying for particular points on evidence which is held untrustworthy as a whole, or where so much has to be rejected as to shake its general authority? If in these and similar respects a hypothesis give a satisfactory explanation of the actual state of the evidence, it is held to be established as certain.

The process is, in fact, exactly the same in an historical investigation as in a judicial,—except so far as in the latter the practical and moral ends involved (as, regard for the protection of society; reluctance to convict on mere probability; or, again, to throw needless discredit on the character of a witness) interfere with the purely scientific object of discovering the truth. The witnesses tell their story: then they are cross-examined: then the evidence is summed up and arranged; and so the verdict and sentence are passed. Not infrequently the division of labour is carried as far in treating the one class of questions as the other. The antiquary