

A STUDY OF HISTORY

12

Reconsiderations

ARNOLD J. TOYNBEE

OXFORD UNIVERSITY PRESS

London New York Toronto

Oxford University Press, Amen House, London E. C. 4

GLASGOW • NEW YORK • TORONTO • MELBOURNE • WELLINGTON
BOMBAY • CALCUTTA • MADRAS • KARACHI • LAHORE • DACCA
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A Study of History was first issued under the auspices of the Royal Institute of International Affairs, of which Arnold J. Toynbee was then Director of Studies.

© Oxford University Press 1961

First published, 1961

First issued in this format 1964

The illustration on the cover of this volume is a rendering by Charles Gottlieb of *The Thinker* (detail) by Rodin, 1879-1900

PRINTED IN THE UNITED STATES OF AMERICA

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6. EMPIRICAL

This modern Western word is derived from the Greek adjective *empeirikós*, this from the Greek substantive *empeiria*, and this in turn from another substantive: *petra*. The Greek word *petra* means an attempt, a try, an experiment, a test, a temptation; *empeiria* means the experience that is the fruit of experimenting; *empeirikós* means believing in the value of experience and taking account of it.

In this book I have claimed throughout that I am using an empirical method of inquiry. A number of my critics have taken note of this claim of mine, and most of these have contested it.¹ I must therefore explain what I mean by the term.

I am not claiming that I approach the historical record of human experience without preconceptions; and I entirely agree with W. H. Walsh when he says that this would be 'a claim which could certainly not be sustained'.² 'Some theoretical framework and some working hypotheses are unavoidable',³ because the human mind's process of

¹ For instance, A. J. P. Taylor in *Toynbee and History*, pp. 120-1; H. Trevor-Roper, *ibid.*, p. 123; P. Geyl, *ibid.*, p. 36 ('the pretence of an empirical investigation') and p. 44 (in his claim 'that his whole argument is based on empirical methods', Toynbee 'is deceiving himself'); M. A. Fitzsimons in *The Intent of Toynbee's History: A Cooperative Appraisal*, and in *The Review of Politics*, October, 1957, pp. 554-5 ('the confusing profession of empiricism and scientific method and the obvious dominance of his philosophy of history'); E. E. Y. Hales in *History Today*, May, 1955, pp. 322 and 320 ('His myth of history' has no better claim than the Whig and Marxist myths have 'to be regarded as having an empirical basis. . . . He is misleading in treating his views about the births of civilizations as though they were laws, arrived at by empirical analysis'); E. F. J. Zahn in *Toynbee und das Problem der Geschichte*, p. 39 ('What Toynbee calls empiricism is in reality speculation which misuses myth'); A. Hourani in *The Dublin Review*, vol. 229, No. 470 (December, 1955), p. 388 ('The book is by no means empirical in its method'); W. H. McNeill in *The Intent of Toynbee's History: A Cooperative Appraisal* ('His "empiricism" is an empiricism which already is keenly aware of what it is seeking.' The value of his generalizations does not rest upon his empirical surveys. 'The heart of Toynbee's intellectual procedure has always been the sudden flash of insight'); T. J. G. Locher in *De Gids*, May, 1948, offprint, p. 16; C. A. Beard in *The American Historical Review*, vol. xi, No. 2 (January, 1935), p. 308; P. M. Sweezy in *The Nation*, 19th October, 1946 ('The historical uniformities which he believes he has discovered by empirical means are in reality imposed upon his materials from without'); J. A. Tormodsen and G. C. Wasberg in *Samtiden*, vol. 58, hefte 12 (1949), pp. 647-6c; B. D. Wolfe in *The American Mercury*, No. 64 (1947), pp. 748-55 ('It becomes clear in volumes iv, v, and vi that his "empirical method" of studying history is only a pretence, an unconscious tribute to the secular, rational, scientific method which, at heart, he rejects' (p. 755)); H. Frankfort in *The Birth of Civilization in the Near East*, pp. 25-26 ('If he describes "the consummation of human history" as "accomplishing the transformation of Sub-man through Man into Superman" . . . we may respect his faith but can hardly accept it as the argument of "an empirical student of history"'); G. Lefebvre in *La Revue Historique*, January-March, 1949, pp. 109-13 (what passes for empiricism in Toynbee's work is merely a means of bringing forward a new Augustinianism).

Crane Brinton, in *The Virginia Quarterly Review*, vol. 32, No. 3 (Summer, 1956), pp. 361-75, convicts me with a recommendation for mercy. My constant appeal to 'our well-tried empirical method' infuriates the matter-of-fact mind, to the point that I seem to such a mind to be deliberately hypocritical. 'This', Brinton submits, 'is surely not so. Toynbee was trained as an historian, and he is an Englishman, heir to a long tradition of philosophical empiricism. He just hasn't solved the dichotomies of this world-the other world or real-ideal or body-soul—a predicament in which he is not alone.'

On the other hand, A. L. Guerard, in *The Herald-Tribune*, 28th October, 1934, pronounces that 'there is a radical difference, in spirit and method', between Spengler and me. 'Spengler is a prophet; Toynbee is an inquirer. In his boldest attempts the British scholar remains an empiricist, a Baconian.' A. L. Kroeber, too, judges, in *The Nature of Culture*, p. 373, that my procedure is empirical in the main.

² *Toynbee and History*, p. 128.

³ Social Science Research Council's Committee on Historiography's Report (1954), p. 132, quoted in this volume already on p. 43. See also footnote 11 to p. 43.

thought is analytical and classificatory.¹ If I have seemed, to so careful and discriminating a critic as Walsh, to be implying that I am approaching history without preconceptions, that must be my fault. It must mean that, in volumes i-x of this book, I have not made it clear that I agree with Walsh on this crucial point.² For reasons already set out in Chapter I of this volume, I disagree with Hales, when he talks of 'laws arrived at by empirical analysis'.³ I agree with E. Berkovitz that 'laws' cannot be derived from facts,⁴ and with Erdmann when he says, *ad hominem*, that my guiding ideas are not derived from the observation of history,⁵ though I do not agree with Mumford that my conclusions, as well as my hypotheses, 'for all his empiricism, are inevitably as much the product of his own ideology as of the situations that he "interprets"'.⁶ This point has been put in telling words by H. Baudet:⁷

'Many critics have censured Toynbee's primary vision on the theoretical ground that it is "apriori". Certainly it is, as they say. But, "epistemologically", is not an "apriori" of this kind a basis [of mental operations] which speaks for itself because it is unavoidable? Is it not a compelling necessity?

'All vision is engendered on an "apriori", and . . . an "apriori" of this kind has its roots—as all thinking has, *au fond*—in will and passion.'

The point is driven home by K. R. Popper. He rejects

'the view that science begins with observations from which it derives its theories by some process of generalization or induction'.⁸ 'I do not believe that we ever make inductive generalizations in the sense that we start from observations and try to derive our theories from them.'⁹ 'Before we can collect data, our interest in *data of a certain kind* must be aroused: the problem always comes first.'¹⁰ 'Theories are prior to observations as well as to experiments, in the sense that the latter are significant only in relation to theoretical problems.'¹¹

When Trevor-Roper says that, in my work, 'the theories are not deduced from the facts',¹² the answer is that neither my theories nor anyone else's are or ever have been or ever will be generated in that way. If being 'empirical' meant this, the word would have no counterpart in reality, and had better be struck out of the dictionary.¹³ On the other

¹ See Chapter I of this volume, *passim*.

² For instance, I have not made this clear to Father D'Arcy, to judge by his comment that what Geyl's criticism 'proves is that Toynbee should not have claimed to rest his case entirely on empirical methods' (M. C. D'Arcy: *The Sense of History*, p. 72).

³ E. E. Y. Hales in *History To-day*, May, 1955.

⁴ *Judaism: Fossil or Ferment?*, p. 10.

⁵ *Archiv für Kulturgeschichte*, xxxiii. Band, Heft 2 (1951), p. 246.

⁶ *Diogenes*, No. 13 (Spring, 1956), p. 13.

⁷ In *Historie en Metahistorie*, p. 46.

⁸ *The Poverty of Historicism*, p. 98. Cp. p. 121.

⁹ *Ibid.*, p. 134.

¹⁰ *Ibid.*, p. 98.

¹¹ *Ibid.*, p. 121. Cp. p. 134.

¹² *Toynbee and History*, p. 123.

¹³ 'Is it true that Toynbee's asseveration of the empirical character of his method is in fact an untruth, because his work is fundamentally aprioristic? I cannot see that these two processes are mutually exclusive.'

'The process of apriori formulation and of working the proposition out is, in the second stage of the operation, undoubtedly an empirical process of work.' It is not a valid criticism of Toynbee, or of any other author of a book *de longue haleine*, that the author foresees at the beginning what he is going to write years later (Baudet, in loc. cit., pp. 46-47).

hand, when Trevor-Roper goes on to say that my theories are not tested by the facts either, he is laying down a legitimate requirement,¹ and my claim to be using an empirical method of inquiry does stand or fall according to the verdict on this count. I agree that my claim cannot be sustained if I have not tried to test my theories and hypotheses by the facts, or if I have tried but have not done the job properly or successfully.² For, while it is true that theories and hypotheses can never be deduced from facts, it is also true that they can be validated only if they are confronted with relevant facts and are confirmed by them. More than that, the whole purpose of formulating a theory or an hypothesis is the heuristic one of trying to increase our knowledge and understanding by applying the theory or hypothesis to the phenomena.³ I maintain my claim that I have tried to be empirical in this sense, which is, I believe, the correct usage of the word and does mean something that an inquirer not only can be but ought to be.

In making my claim to be empirical, I have been tacitly contrasting my approach with Dilthey's approach and with Spengler's.⁴ While the plan of the present book was brewing in my mind, the first volume of Spengler's *Der Untergang des Abendlandes* was published, and, when I read it, my first impression was that, in Spengler's work, what I had been planning was already an accomplished fact. My second impression, however, was that Spengler's work suffered from being too dogmatic, in the sense that he was apt to enunciate his theories about the configuration of human affairs and to leave it at that, without putting these theories to sufficiently thorough tests on the touchstone of the phenomena.⁵ Having decided to go on with my own enterprise, I was told by a distinguished philosopher, the late Lord Lindsay of Birker, that I

The points here made by Baudet answer K. D. Erdmann's contention (in *Archiv für Kulturgeschichte*, xxxiii. Band, Heft 2 (1951), p. 206) that, when I call my method 'empirical', the expression is a misapplication of the word, because 'there can be no question here of an inductive procedure'. Erdmann goes on to say, correctly, that 'the experience from which Toynbee obtains his pair of ideas is not historical but meta-historical in character'. But he is not correct in adding that 'Toynbee is a "realist" in the Schoolmen's usage of the term. Revelation, as fundamental religious experience (*Urerfahrung*), is [for him] the criterion of truth.' I take challenge-and-response, and any other ideas of mine that come from the Bible, not as revelation, but as hypotheses to be applied to the phenomena with a view to gaining knowledge and understanding. My criterion of truth is whether the hypotheses fit the phenomena. I maintain that this is the inductive method, and that, so far from induction being incompatible with having an *a priori* hypothesis, it is impossible without having one.

Again, when W. Gurian declares that my 'fundamental concepts' are 'means of subjective classification' (*The Review of Politics*, vol. 4, No. 4, p. 511), he misses the point that this is true of everyone's 'fundamental concepts' unless and until they have been tested by being confronted with as large an array of relevant phenomena as the inquirer is able to assemble. It is a more pertinent criticism of Gurian's, supposing that it is justified, when he says (*ibid.*) that my fundamental concepts 'appear as very thin'.

¹ Baudet observes, in *loc. cit.*, p. 47, that the process of proof must be kept clearly distinct from the original vision.

² 'Assuredly, if induction . . . were an invalid process, no process grounded on it would be valid. . . . But, though a valid process, it is a fallible one, and fallible in very different degrees' (J. S. Mill: *Philosophy of Scientific Method*, ed. by E. Nagel (New York 1950, Hafner), p. 290).

³ See pp. 22-23, 41-45, and 158-70.

⁴ My wish to distinguish my approach from Spengler's has been guessed by W. H. McNeill in *The Intent of Toynbee's History: A Cooperative Appraisal*.

⁵ This makes Spengler a poet, according to Holborn. 'History is the re-enactment of the past in the mind of the historian, and even "facts" exist only there. But, in contrast to poetry, they call for critical verification' (*The Saturday Review of Literature*, 31st May, 1947, p. 29).

should find in Dilthey's work the very thing that I was looking for. What I was looking for was a bridge between theory and fact. But, in Dilthey's work, I did not find even theories about the configuration of human affairs. I found nothing but epistemology. I was, and am, grateful to Dilthey for that, since the relation between theory and fact cannot be studied without taking epistemology into account. But the bridge for which I was looking was not to be found in Dilthey's work, and I had to try to build it without getting help from him.

Some critics have given me credit for making this attempt. Guerard, for instance, draws the same contrast between Spengler and me that I have drawn in my own mind.¹ Feibleman says of me² that 'he tries to analyse cultural structure, and, in doing so, takes the first step towards the establishment of the empirical field of human social structure as the empirical study of a science'. To try, however, is not enough. The attempt that I have made has been criticized on at least six counts. According to the critics, the examples that I have taken as test cases have been denatured by being taken out of their context.³ Some of these examples are ruled out of order because they are taken from phenomena of a different order of magnitude from the civilizations on which I am seeking to throw light.⁴ My citation of examples, relevant to whatever the point in question may be, is not exhaustive and is therefore unrepresentative and thus misleading. Alternatively, I cite so many examples that I clutter up my argument with an indigestible mass of details.⁵ Whether the number of examples that I cite is too small or too great, I am guilty of selecting them to fit my theories.⁶ When they will not fit,

¹ See p. 243, footnote 1.

² J. K. Feibleman in *T'ien Hsia Monthly*, vol. xi, Nos. 1 and 2 (1940), p. 171.

³ See pp. 234-5.

⁴ See p. 235.

⁵ The book is criticized in this sense by A. L. Guerard (in *The Herald-Tribune*, 28th October, 1934), by L. Mumford (in *Toynbee and History*, p. 141), and by P. Sorokin (ibid., p. 178). As Mumford puts it (ibid., p. 142), 'his Study of History . . . is . . . in its vastness, its complexity, its impenetrability, and its magnificent profusion and confusion, an image of that great overgrown megalopolis [London], stifled by its very success.'

⁶ 'He selects the instances which will support his theses, or he presents them in the way that suits him. . . . Those cases he does mention can be explained or described in a different way so as to disagree no less completely with his theses' (P. Geyl in *Toynbee and History*, p. 45). 'Often other and quite contrary examples are readily available' (P. Bagby, ibid., p. 105). W. H. McNeill observes that a frequent procedure of mine for trying to justify my generalizations is to select for attention only those bits and pieces that fit in with my notions (*The Intent of Toynbee's History: A Cooperative Appraisal*). 'An apparently random citation of instances to illustrate patterns' (M. A. Fitzsimons in *The Intent*). 'Conclusions drawn from incomplete or partially selected evidence' (L. Stone, ibid., p. 112). My selection is arbitrary (Geyl in P. Geyl; A. J. Toynbee; P. A. Sorokin: *The Pattern of the Past: Can we Determine It?*, p. 85). 'Too many selected facts' (W. Gurian in *The Review of Politics*, vol. 4, No. 4, p. 511). T. J. G. Locher finds my procedure too selective, as well as too simple, subjective, and one-sided (*De Gids*, May, 1948, offprint, p. 15). H. Holborn observes that 'imagination by itself' cannot 'produce an objective selection and arrangement of facts' (*The Saturday Review of Literature*, 31st May, 1947, p. 29). P. Geyl finds a contradiction between my 'imaginative method and my empirical claims' (*Debates with Historians*, p. 154). O. H. K. Spate finds that my selectivity goes too far (*The Geographical Journal*, vol. cxviii, Part 4 (December, 1952), p. 409, and *Toynbee and History*, p. 291).

On the other hand, a number of critics point out that selection is, in itself, something inevitable.

'Any large hypothesis must go beyond immediately available data and is never completely verifiable' (Social Science Research Council's Committee on Historiography's Report (1954), p. 130). 'In history there is no possibility of making quantitative demonstrations' (Anderle's unpublished paper). While Geyl is right in saying that an *a priori*

I force them with Procrustean violence.¹ I have a rigid *a priori* scheme.² If even this Procrustean treatment cannot make awkward facts conform, I ignore them.³ Some of these charges cancel each other out, but what is left is still formidable.

involves predetermined selection, the truth is that historical writing is always selective (H. Baudet in *Historie en Metahistorie*, p. 46). 'Any problem of interpretation involves a selective evaluation of evidence' (R. L. Shinn: *Christianity and the Problem of History*, p. 56); so 'historical method must be based on selection' (*ibid.*, p. 7).

Ad hominem, Spate observes that 'even Toynbee's selectivity, so severely handled by Geyl, might be admitted within limits' (*Toynbee and History*, p. 290). I am content to accept Sir Llewellyn Woodward's comment that, 'for all the astonishing number of particular facts brought together, one may wonder whether Professor Toynbee (or any single man, in the present state of learning) can really be sure that his selection from the accumulated data about the past is not open to attack' (*The Spectator*, 6th July, 1934).

¹ 'Frequently his point is made at the price of a radical distortion of facts' (P. Bagby in *Toynbee and History*, p. 105). 'Dr. Toynbee imposes patterns on history' (Sir Ernest Barker, *ibid.*, p. 95); and the facts rebel, like the flamingo-mallets, hedgehog-balls, and soldier-hoops in the famous game of croquet in *Alice in Wonderland* (P. Geyl, *ibid.*, pp. 62 and 375). Facts are made to fit a Procrustean bed (L. Stone, *ibid.*, p. 113). 'The Procrustean method of handling Chinese history' (W. Altree, *ibid.*, pp. 266-71). 'Forcing it all into the scheme of a presumptuous construction' (Geyl, *ibid.*, p. 373). I adapt fact to suit theory (R. H. S. Crossman in *The New Statesman*, 8th March, 1947). 'One of the dangers of "pattern history", if it may be so described, is that facts must be woven into the pattern. One of the advantages is that a good and attractive pattern prints itself upon the memory' (H. A. L. Fisher in *The Nineteenth Century and After*, December, 1934, p. 672). I impose my patterns by force (R. Pares in *The English Historical Review*, vol. lxxii, No. 279 (April, 1956), p. 262). 'His doctrine that facts are less important than the abstract system into which he herds them' (J. Bishko, in *The Richmond News Leader*, 21st October, 1954). 'Claiming to be an empiricist, he forces the facts to suit preconceived ideas' (H. Frankfort in *The Observer*, 31st January, 1954). 'He cannot resist the temptation to fit complex facts . . . into a Procrustean scheme' (Granville Hicks in *The New Leader*, 18th October, 1954, p. 23). 'He distorts some parts of it [history] by pushing it into his iron cubby-holes' (A. Nevins in *The New York World-Telegram and Sun*, 17th December, 1954). 'Sometimes Toynbee seems to force the pieces into place, whether they fit or not' (F. L. Ralph in *The Saturday Review*, 16th October, 1954, p. 19). 'The system sometimes makes a poor fit—for example, with the Arabian-Islamic' (J. A. Tormodsen and G. C. Wasberg in *Samtiden*, vol. 58, hefte 12 (1949), pp. 647-60). Other civilizations are more or less ruthlessly fitted into the Procrustean framework of the Hellenic Civilization (R. K. Merton in *The American Journal of Sociology*, vol. xlvii, No. 2 (September, 1941), pp. 205-13). J. Vogt finds that I am arbitrary in my treatment of the material and that I distort the facts (*Saeculum*, No. 2 (1951), pp. 557-74). On this point my critics are unanimous, according to Anderle (*in op. cit.*). The point is put genially, as well as wittily, by the Rev. E. R. Hardy Jr.: 'His scheme is in some ways Procrustean, although the bed is comfortably furnished and the pulleys work smoothly' (*The Intent of Toynbee's History: A Cooperative Appraisal*).

² 'The real rock of offence to many (including the present writer) is the somewhat rigid schematism' (O. H. K. Spate in *Toynbee and History*, p. 291). My scheme, like Ellsworth Huntington's, is too neat to be convincing (O. H. K. Spate in *The Geographical Journal*, vol. cxviii, Part 4 (December, 1952), p. 408). 'His main trouble is that he has explained too much' (A. L. Kroeber: *Style and Civilizations*, p. 121). 'It is all subordinated, and intended to contribute, to a system, a message.' Though Toynbee is obviously interested in the spectacle of the particular, 'not for one moment does it free him from the obsession of his dream' (P. Geyl in *Toynbee and History*, pp. 360-1). 'Has the [empirical] method been sacrificed to the design, or are we . . . dealing with a deeper question?' (E. Fiess, *ibid.*, p. 380). G. Weil finds a 'contradiction between the principles on which his scheme of the development of civilization is based and his personal judgment of historical phenomena as they really are', and he criticizes my 'scheme-bound point of view' (*ibid.*, p. 285). J. K. Feibleman reproaches me for my 'iron rigidity' (*T'ien Hsia Monthly*, vol. xi, Nos. 1 and 2 (1940), p. 23; *cp.* p. 20). A. Hourani judges that 'the schematism is too rigid' (*The Dublin Review*, vol. 229, No. 470 (December, 1955), p. 381). J. Vogt criticizes my schematization in *Saeculum*, No. 2 (1951), pp. 557-74. A. Nevins finds that Toynbee 'makes history too schematic' (*The New York World-Telegram and Sun*, 7th December, 1954).

³ 'As for those items that just can't be made to fit, they are quickly tossed into the huge garbage heap of discarded facts' (L. Stone in *Toynbee and History*, p. 113). I ignore exceptions to my laws (R. Pares in *The English Historical Review*, vol. lxxi, No. 279

The first of these indictments—that I have taken episodes out of their context—is evidently incompatible with the criticism that my work is superficial, and my spirit hybristic, because I attempt the impossible enterprise of trying to cover the whole history of the Age of the Civilizations.¹ It is true that I have attempted to do this; and it surely follows that my work, as a whole, is likely to have suffered less from distortion as a result of taking episodes out of their context than the work of many other present-day historians. I agree that taking things out of their context does distort them. In the first chapter of this volume I have argued that it is a grievous limitation and a radical defect of the human intellect that it is incapable of apprehending Reality as a whole, and has, perforce, to take it piecemeal at the cost of failing to see it as it truly is. When we are applying our minds to study, and not to practical action, we ought to contend against this inherent infirmity of theirs as far as is humanly possible. My own criticism of the present vogue for 'specialization' is that, so far from trying to combat and, to some extent, counteract this intellectual infirmity of ours, specialization gives way to it and thereby accentuates it. The charge of denaturing Reality by taking episodes out of their context does hit me, no doubt; but I should have thought that it hit, with rather greater force, the school of specialists which is the predominant school among present-day Western historians—a school in whose more polemical exponents' eyes I am something of a heretic, just because I have been unwilling to follow this current fashion.

The charge that I draw many of my illustrations of features in the histories of civilizations from social units of a lower order of magnitude has been noticed and discussed already² and therefore need not be re-examined here.

The charge that my citation of examples is not exhaustive hits not only me but everyone who has ever sought to test an hypothesis by confronting it with relevant phenomena. It hits me perhaps less hard than some of my fellow prisoners in the dock, if it is true that I have surfeited my readers with examples *ad nauseam*. But it hits every student of phenomena, human or non-human, since phenomena, of whatever kind, are innumerable. The only class of things that could conceivably have a membership that was limited by its own nature would be some class, not of phenomena, but of mathematical abstractions that had been abstracted with the express design of creating a self-evidently closed class. Even if our momentary state of knowledge enabled us to enumerate every one of the representatives of some class of phenomena that were in existence at the moment, the exhaustive enumeration would be no better, in logic, than a 'simple enumeration', as has been noticed in Chapter I.³

(April, 1956), p. 262). I do this although 'often other and quite contrary examples are readily available' (P. Bagby in *Toynbee and History*, p. 105, quoted above). Other critics, e.g. Anderle in his paper, hold that this criticism is unjust. Mumford says of me that 'he usually gives enough free play to the data to provide his reader with the necessary correction, and sometimes generously enlists the aid of other critical minds to correct his own bias' (*Toynbee and History*, p. 143). And he recognizes my method as being 'empirical' in the sense of 'qualifying doubtful conclusions in one place by contradictory data in another place' (*Diogenes*, No. 13 (Spring, 1956), p. 13).

¹ See p. 85, footnote 1, and Chapter II, Annex: *Ad Hominem*, p. 638, with footnote 2.

² On pp. 234-5.

³ See pp. 23-24.

If the charge that my citation of examples is selective has to be dropped because it applies, not just to me, but to everyone who tries to test a theory, I am still confronted with the further charge that I make my selection of examples with an eye to fitting my theories. This charge, too, applies to everyone who tries to test a theory.¹ For my part I certainly have not consciously made selections to suit my purposes, and I doubt whether any other scholar ever has either. To do this might be a temptation to a company-promoter, politician, barrister, or member of some other practical profession in which this form of cheating, if the fraud remained undetected, might reap lucrative material rewards. But what interest could a scholar have in spending laborious man-hours in deliberately trying to diminish the knowledge and understanding that he is concerned to increase? The charge is unconvincing—whatever may be the individual against whom it has been made.² At the same time it is hard to rebut, because it is always possible to switch the indictment from the offender's conscious self to the subconscious underworld of his psyche. However upright his conscious self may be admitted to be, his subconscious may be a rogue that has inveigled him into cheating and into doing this bona fide, inasmuch as he has never been conscious of what he is, in fact, doing.³ I do not know how to clear myself of a charge against my subconscious; but I do know that anyone else who was arraigned on account of alleged misdoings of his subconscious would find himself in the same plight.

The same defence holds for the charge that I force facts that will not fit. I can only reply, again, that I have never done so consciously. It is true that I start with a 'schema' in the sense of a formulated but still untested hypothesis or theory. But I plead 'not guilty' to the charge of being 'schema-bound'. Where I believe that I have found some pattern or regularity or recurrence, I have always tried to ascertain the limits of the realm in which this particular 'law' holds good—for instance, in volume ii, where I am dealing with a number of variations on the theme of challenge-and-response. So far from ignoring contradictory instances, I have always brought them up and discussed them when I have been aware of their existence. Of course, many will have escaped me, as also will many other instances that support my hypotheses instead of impugning them. In the numerous surveys made in the first ten volumes of this book, for the purpose of testing how far, if at all, my hypotheses might or might not be valid, I have always made my net as big, and its meshes as close, as I have been able. I am ready at any time to modify or abandon any of my hypotheses if I am given convincing reasons in the shape either of the citation of relevant phenomena previously unknown to me or of the reinterpretation of phenomena of which I am already

¹ 'However valid this criticism may be for Mr. Toynbee's empiricism in particular, it is unerringly true with respect to empiricism in general' (K. W. Thompson in *Toynbee and History*, p. 219).

² A propos of me, A. R. Burn observes: 'It would be unjust to say that he forces facts into his mould, and much more so to imply conscious lack of integrity' (*History*, February–October, 1956, p. 3). G. J. Renier thinks that 'this book cannot be a mystification' (*Toynbee and History*, p. 75).

³ 'Though there is no deception of others, there is the nearest approach to self-deception' (Renier, *ibid.*).

7. MYTH

My usage of the word myth certainly needs defining. The literal meaning of the Greek word *mythos* is 'story'. Like the word 'story' in colloquial English, the word *mythos* in Greek is used in two senses. One of these senses is the usual sense of the derivative word 'myth' in modern Western languages. But the sense in which I use the word 'myth' is that of the other usage of the word in Greek.

The distinction between the two meanings of 'myth' and the two meanings of 'story' is not the same. One kind of 'story' is fiction, the other kind is true to 'fact'. Neither kind of 'myth' is true to fact. One kind is a substitute for statements of fact where the facts are either unknown or ignored; the other kind is a story about a sphere of Reality that is of the highest significance and importance for human beings and is at the same time beyond the range of the 'factual' knowledge that human minds acquire through analytical and classificatory intellectual operations. This second kind of myth is the kind that plays so striking, and so illuminating, a part in Plato's *Dialogues*. Of all men that ever wrote in Greek, Plato has the best claim to be the father of the usage of the Greek word 'myth' in this second sense, because it was he who first consciously and deliberately used 'myth'—as many seers, in many societies, before him and after him have used poetry—to extend the range of human intuition and understanding beyond the limits of the knowledge attainable through logical processes of thought.

This Platonic meaning of the word 'myth' is the one that I have adopted; and, though I have explained this in more than one place in this book up to the present point, I fear that my usage, being not the usual one, has nevertheless caused some misunderstanding in some readers' minds, and has incidentally exposed me to criticism that may be only partly deserved.

Myth, in the sense of a fictitious substitute for the statement of facts, has been regarded in two lights, both in the Hellenic World and in its modern Western successor. It has been held to be not only innocent, but entertaining, in narrative poetry and in 'fairy-stories' told to children. It has been held—and rightly held, I too believe—to be pernicious where it has been brought in to fill a gap in our factual knowledge, and *a fortiori* where there is genuine factual knowledge on record which a mythical narrative has ousted. For instance, the Alexander Romance is so entertaining that it has been translated into a host of languages; but nobody

¹ 'The point is that one should recognise the fact and its difficulty, and should recognise its unavoidableness. The point is that, in and through the recognition of all this, one should strive, with a high and pure [resolve], to hold fast to "honnêteté"' (H. Baudet in *Historie en Metahistorie*, p. 46).

'One has the feeling that Toynbee, unlike Spengler, is really trying to subject his historical laws to empirical tests, and that he would have the integrity and the modesty, if given sufficient reason, to modify or abandon his laws' (W. H. Coates in *The Journal of Modern History*, vol. xxi, No. 1 (March, 1949), p. 27, quoted already on p. 46). Cp. F. Neilson in *The American Journal of Economics and Sociology*, Supplement to vol. xiv, No. 3 (April, 1955), p. 3, already cited in the Introduction to the present volume, p. 2.