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EXPLANATORY GENEALOGIES AND HISTORICAL TESTIMONY

ABSTRACT

This article proposes that a general theory of assessment of historical testimony should do justice to the long tradition of adjudication in accordance with maxims of reliability and competence. I argue that an explanatory genealogical theory (along lines first adumbrated by Charles Seignobos) satisfies this condition, and that it has further notable virtues: respect for the strengths of rival theories, regard for the links between adjudication of testimony and other basic procedures of historical inquiry, and the promise of profitable lines of investigation.

1. INTRODUCTION

According to what I shall call the "explanatory genealogical" theory of testimony, the basic criterion for the assessment of testimony is whether or not the production of the testimony is better explained by an account of its origin that makes probable its truth than by any that casts doubt on it. The present article deals with the credentials of the explanatory genealogical theory in relation to the assessment of historical testimony. In particular, I am concerned to show how this theory does justice to the tradition of historians' practice in the adjudication of testimony.

The plan is as follows. I first look briefly at the vexed question of the scope of the notion of testimony. I then set out certain conditions of adequacy for a theory of historical testimony. The first such criterion is that the theory should do justice to rival theories, and in particular to the maxim-based account that has dominated historical practice since Antiquity. After sketching the history of the maxim-based view, I emphasise one feature of it that appears especially challenging to the explanatory genealogical theory. The second proposed criterion of adequacy is to the effect that a theory of testimony should do justice to the varied and complex ways in which adjudication of testimony is linked with the other basic procedures of historical enquiry, namely, the interpretation of sources and the analysis of non-testimonial evidence. There follows a quick overview of some of the rival theories on offer, and a more detailed presentation of the explanatory genealogical theory as adumbrated by Charles Seignobos in 1887 and more analytically developed in

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recent writings of Elizabeth Fricker (1995) and Peter Lipton (1998). I go on to show how the explanatory genealogical theory satisfies the conditions of adequacy, and conclude with some indications of promising lines of inquiry suggested by the theory.

2. THE BOUNDS OF TESTIMONY

What is to count as testimony? At one extreme we have the strict view, adopted in many philosophical accounts, according to which testimony that p requires explicit statement that p and, moreover, that the testifier offer the statement as evidence that p.[1] At the other extreme lie accounts, well grounded in ordinary usage, which allow any trace or signal that conveys reliable "information" to the effect that p to count as a testimony that p: thus field and cropping patterns testify to past farming practices, and the beautiful stratified cliffs above Llangollen bear witness to past seismic upheavals and inundations.

That the narrow view of testimony is too narrow for our purposes is obvious enough. Historical testimonies are frequently offered unwittingly, for example, by copyists or by those who do not appreciate the full implications of what they have said or written. Indeed, it is often argued that such testimonies are especially valuable for historians precisely because they are unwitting, and hence exempt from charges of deliberate contrivance. At the opposite extreme, the inclusion a wide range of informative traces is tempting. For, as Aviezer Tucker has shown, the ways in which historians acquire knowledge from multiple documentary testimonies show close analogies with the ways in which philologists, linguists, archaeologists and evolutionary biologists acquire knowledge of past events and states of affairs by tracking down the common origins of multiple traces (Tucker 2007, forthcoming). But there are, I think, two good reasons for resisting this expansive usage. First, it is surely a condition for something to constitute a testimony in a literal rather than a metaphorical sense that it be in a medium governed by conventions of representation.[2] Verbal and documentary statements evidently satisfy this condition. So too, on occasion, do images. Thus it is uncontroversial that maps do so: the claim that Ely is a town north of Cambridge can be read off a map in accordance with unambiguous conventions embodied in the projection and key. But when we turn to portraits, narrative paintings, landscapes, photographs, and films, the extent to which the medium is governed by conventions of representation and can hence deliver definite testimonies becomes a contentious matter.[3] For example, were the conventions of physiognomy at play in much seventeenth- and eighteenth-century European portraiture sufficiently well defined and sufficiently current to constitute a language in which the painter could testify to the personality of the sitter? It is hard to say.[4]

One reason for preferring an intermediate notion of testimony has to do with testimonial chains. Philosophical accounts of testimony have tended to view it as a means of transferring knowledge from person to person. However, Jennifer

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Lackey has provided a particularly clear-cut type of counterexample. Suppose that p is the case, and that the testifier has good evidence that p, but lacks knowledge because of additional false beliefs counting strongly against p. A hearer who lacks these false beliefs can, nevertheless, acquire knowledge that p from the testimony.[5] Historical practice provides copious further counterexamples; indeed, many areas of history are largely founded on knowledge derived from the unwitting testimonies of messengers and copyists.

A second and more pragmatic reason for adopting this moderately relaxed view of testimony has to do with the practice of historians. In the historiographical tradition, the interpretation of documents has long been distinguished sharply from all forms of inference from material remains or, as they are often collectively called, "monuments". Further, the maxims that have long guided historians' interpretations and adjudications, in so far as they relate primarily to the circumstances, motivations, and competences of the authors of documents, can be extended readily enough to images governed by reasonably well defined "visual languages". But they cannot be extended to the business of adjudicating the reliability of material traces as indications of past events and states of affairs. So the moderately extended notion of testimony outlined here forms, I suggest, a historiographical natural kind.

3. CONDITIONS OF ADEQUACY

A key requirement is that our theory of testimony should respect alternative accounts, at least to the extent that they themselves take serious notice of actual practices of adjudication of historical testimony. It is, therefore, especially important to do justice to the long tradition of maxim-based judgements.

Here, for example, is a sample of the *règles de critique* – that is, rules for the authentication of historical sources and for the adjudication of their testimony on matters of historical fact – offered in the *Cours d'études historiques* of 1842 by P.-C.-F. Daunou, imperial archivist under Napoleon, then professor of history at the Collège de France.

> XXVIII. A man's testimonies will be of no value if one becomes aware that he did not have the means to examine the facts that he reports.
>
> XXIX. There are no grounds for accepting a man's accounts when it is evident that they were dictated to him by his personal interests. [...]
>
> XXXIV. In distrusting a man who reveals his devotion to a sect, a party, a faction, one ought not, however, to imagine that the truth is to be hoped for only on the part of characters sufficiently apathetic to register with indifference the enterprises, revolutions and catastrophes of which they declare themselves witnesses.
>
> XXXV. When there is disparity or contradiction between original reports, criticism decides between them by comparison and by the weight more than by the number of testimonies. (Daunou 1842, 467ff)

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Instruction in the adjudication of historical testimonies in accordance with such maxims has a very long history, going back at least to Cicero and Quintilian. In the early-modern period it formed a part of the teaching of rhetoric and logic, especially in the context of legal training;[6] and it loomed large in eighteenth- and early-nineteenth-century treatises on the interpretation of sacred and profane sources.[7] Such accounts generally employ a distinction between external criteria of authenticity relating to dating, authorship, and circumstances of composition, and internal criteria relating to content and its interpretation. With the consolidation of history as an academic discipline in the second half of the nineteenth century, source criticism, the scholarly interpretation of documents and critical assessment of their testimonies, was widely presented as the basis for a "scientific" historiography,[8] with Leopold von Ranke being often cited as an exemplary practitioner in this respect.[9] The major late nineteenth-century works on historical method, for example, Ernst Bernheim's encyclopaedic *Lehrbuch der historischen Methode* of 1889, continue the maxim-based approach to testimony, distinguishing internal from external criteria of authenticity; and this approach remains much in evidence in twentieth-century historical manuals, for example, Marc Bloch's *Apologie pour l'histoire ou métier d'historien* of 1939 and John Tosh's *The Pursuit of History* of 1984.

One striking feature of the maxims that figure in such accounts is their banality. Any theoretical account of testimony should come to terms with these obviosities, either confirming them or, if it calls them in question, explaining the sources of their specious appeal. This is, in fact, one aspect of the more general requirement that an account should respect judgements of testimony of the kinds we customarily make, whether unreflectively or guided by maxims and theories. It should do so either by showing them to be justified or, failing that, by showing how it is that despite their lack of justification we are in the habit of making them.

There is, however, a long-standing feature of maxim-based accounts that is more surprising and more challenging, namely the pairing of maxims for the acceptance of testimony with countermaxims for its rejection.[10] Thus to the injunction to assent to testimony that is coherent, we find opposed the counterinjunction to be wary of such testimony on the grounds that a measure of incoherence and untidiness is a mark of spontaneity and authenticity; and the advice to distrust derivative testimonies as likely to be corrupted is set against the advice to trust them on the grounds that in the long run error falls away and the truth will out; and (as in Daunou's rules above) the advocacy of disinterested testimony is accompanied by a warning against the testimony of those entirely indifferent to the events they report.[11] At the very least a theory of testimony owes us some indication of why such contrary maxims seem plausible to us. And we may hope for more: for a justification of practices of adjudication that make use of them.

A second general condition of adequacy has to do with the place of adjudication of testimony in the general practices of historical enquiry. Adjudication of one testimony often interacts not only with the adjudication of others, but also with

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the weighing of evidence from non-testimonial sources: for example, when analysis of ink or paper authenticates the date of a document, or when study of surviving instruments casts light on testimony concerning the results obtained with them. Further, interpretation of sources and adjudication of their testimonies go hand in hand: thus interpretation is a prerequisite for grasp of the content of testimonies, and the facts concerning authorship, circumstances of composition, etc., on which interpretation often depends are themselves the fruits of adjudication of testimony. This mutual dependence of the interpretation of testimony and its adjudication is one of the many forms of the so-called "hermeneutic circle", that is, the mutual dependence between interpretation and the evidence on which it is based. It is to be wished that a theory of adjudication should come to terms with these long-recognised linkages. So our second general criterion of adequacy is that a theory should at least respect, and at best clarify, the connections between testimonial assessment and the other fundamental procedures of historical inquiry, interpretation, and collation of evidence.

4. THEORIES OF ADJUDICATION

It is obvious that in everyday life testimony is often accepted unreflectively without the recipient going through any explicit process of adjudication. Several authors have maintained that acceptance of testimony is the "default setting", and that adjudication is reasonably undertaken only when triggered by grounds for doubt.[12] Thus Tony Coady has presented such a disposition to trust testimony as a condition of the possibility of language (1992); Tyler Burge has maintained that in identifying an utterance as testimony, and hence as an assertion, we attribute it to a rational agent and thus have prima facie grounds for trust, because a measure of trustworthiness is constitutive of rationality (1993); and Axel Gelfert has suggested on Kantian grounds that we have a moral duty to make trust our default position with respect to testimony (2006). Such accounts, however, leave open the issue of the proper way to proceed in case some source of doubt does trigger explicit adjudication.

A simple account of adjudication of testimony is implicit in the majority of the instructions offered by lawyers, rhetoricians, biblical critics, philologists and historians from classical times to the present. This is a judgmental account, one according to which the testimony is evaluated by measuring it against the indications and counterindications codified in the various maxims. Often, as in Ernst Bernheim's *Lehrbuch der historischen Methode*, the criteria enshrined in the maxims are treated as tests to be applied to the testimony, which is to be accepted unless it fails some of them (1889, Kap. IV). Sometimes, a little more sophisticatedly, the process is conceived as one of estimation of weight in the light of the indications and counterindications, and in the wake of the Port-Royal *Logique* this weighing has often been considered as estimation of a probability that is in principle quantifiable, in the manner recommended by Leibniz (Arnauld & Nicole

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1662/1996, IV, ch. 14, Leibniz 1704/1981, 312ff). This is the model adopted, for example, in Marc Bloch's *The Historian's Craft* (1939/1954).

Inductive models, often invoking the authority of David Hume, have loomed large in the philosophical literature, though rarely figuring in writings on historical method. The simplest such account is to the effect that trust in testimony is to be justified by appeal to cases in which our personal observations have correlated facts with testimonies to those facts. This does not, of course, in itself provide any explication of the differential adjudication of testimony; though it may figure in such an explication by providing an estimate of the probability or weight of testimony prior to evaluation by specific criteria.[13] Differential assessments of testimony may, however, be accounted for by inductive models according to which trust and distrust in testimony of particular kinds is to be justified by appeal to cases in which one's personal observations have shown testimonies of those kinds to have been true or untrue.

A fourth model we may call abductive. This treats judgement of testimony as an instance of inference of hidden causes from manifest vestiges and signs. In classical and early modern writings inference from signs was generally considered as a practical art that can be cultivated but not codified.[14] Accordingly, philology and history were often grouped with the various divinatory arts – augury, cheiromancy, etc. – along with medical diagnosis, as the province of tact and skill rather than of reason. However, as Carlo Ginzburg has shown, in the course of the nineteenth century in a whole range of fields – philology, art history, palaeontology, psychology, criminology – reading of signs came to be considered not as an art but, in the manner of Sherlock Holmes, as a science of systematic reconstruction of absent or hidden realities from present and manifest clues.[15]

Of this more elaborate kind is the account developed by Charles Seignobos and later offered in watered-down form in the long-standard textbook of historical method that he co-authored with Charles Langlois (Seignobos 1887, Langlois & Seignobos 1898, bk. 2, chs. VI–VIII). All knowledge of historical facts is, Seignobos argued, derived by reasoning from traces or "*documents*".[16] In the case of "material documents", traces imprinted directly on objects, the historian may proceed directly to infer the fact by working out how it gave rise to the trace. In the case of "psychological documents", that is, written and oral testimonies, the progress from trace to fact is complex and indirect. It involves a double reconstruction, of the psychological state of the testifier that is expressed in the testimony, and of the processes which gave rise to that psychological state. Seignobos offers a detailed account of the steps involved in any such reconstruction of the genealogy of written testimony, focussing in turn on the problems which arise in deciding whether the literal sense of the testimony expresses the author's "conception", whether this conception represents a belief or a fabrication, and whether in case it represents a belief, that belief arose directly (or indirectly, if the testimony is derivative) from a direct impression of the relevant fact (Seignobos 1887, 12–21). Once such a reconstruction has been carried out, the historian will attempt to confirm it by

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appeal to independent testimonies (21–7). The major premisses appealed to in these reconstructions are to be drawn from the science of descriptive psychology, "the foundation of history" (27–179). Despite this scientific foundation, however, the "history of events" derived from these reconstructions remains a matter of probability and conjecture (29). This uncertainty arises from the analogical inferences involved whenever the psychological laws based on observation of present-day persons are applied to past persons; and it is exacerbated by the uncertainty of the singular premisses concerning historical circumstances to which the reconstructions appeal, these being themselves the product of merely probable inferences from other "material" and "psychological" documents (28ff).

Now there is much in Seignobos's powerful and ingenious account that is questionable and dated. To start with, there is the problematic status of his ten steps in the reconstruction of past facts from documents. Partly they serve as a didactic device, indicating the links in the causal chains that a satisfactory positive assessment of documentary testimony must establish. But there are occasions on which Seignobos writes as if they were steps of inference – as if one should always first establish a literal interpretation, then seek to establish the author's "conception", then find out if it corresponded to the author's belief, then trace the antecedents of that belief, and so on. Such a view overlooks the links between the steps, for example, that between interpretation and adjudication of testimony. Thus, to take an artificially simple case, faced with the question, "Which Frankfurt?", it would be sensible to consider other testimonies and clues relating to the questions whether the writer had ever visited either Frankfurt, whether events of the type detailed were likely to have occurred in either Frankfurt, etc. Seignobos's positivistic tendency to represent the acquisition of historical knowledge as a matter of step-wise inference from facts is likewise in evidence in his view that the laws of psychology are established by induction on directly observed present-day behaviour, and that their historical application involves an analogical inference from present to past persons. Quite apart from the vexed question of whether everyday psychological generalisations of the sorts that Seignobos has in mind – for example, that people tend to misrepresent persons they greatly admire or greatly hate – really are inductive generalisations, Seignobos shows a lack of appreciation of the conditional nature of their application in the assessment of testimony. For the appropriateness of application of such commonplaces of folk psychology to a particular author can often be judged only against the merits of the entire reconstruction of the genesis of the testimony. Thus, whether it is appropriate to invoke the link between partisanship and misrepresentation in a given case will often depend on whether the misrepresentation would have been perceived by the author as serving the partisan ends, as well as on the collateral evidence, or lack of evidence, for misrepresentation as opposed to sincere avowal or honest mistake. A final, and very serious, objection to Seignobos's account is his apparent failure to take any cognizance of the roles in reconstruction of the genesis of testimony of the local norms and conventions governing past activities, including the observation

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and reporting of events. This is a major omission even by the standards of the period; for the need to take due account of past actors' viewpoints had long been a major theme in the theory of interpretation and source criticism.[17]

These objections, however, leave the core of Seignobos's account unscathed. This is the claim that the basic criterion for the assessment of testimony is whether or not the production of the testimony is best explained by an account which makes probable its truth.[18] As it stands, this criterion is in need of a little elaboration. For, as has often been noted, the "goodness" of an explanation is relative to the purpose for which it is offered.[19] Thus there may be good explanations of testimonies that are more or less neutral on the issue of their truth – for example, that the testifier said that p because he had it from a friend whom he trusted, or because he wanted us to believe that p, etc. But excellent though these may be in terms of coverage of the evidence, simplicity, beauty, etc., they are worthless for the purpose of adjudicating the testimony. To clarify the account, making explicit the interest in adjudication, let us consider two kinds of story about the source of a testimony. A vindicatory genealogy of the testimony that p is an explanation of its production that makes it probable that p. A critical genealogy, by contrast, is one that calls p in question. Our criterion is then that there be a vindicatory genealogy that better explains the testimony than does any critical genealogy.

5. RESPECT FOR ALTERNATIVE THEORIES

One of our primary conditions of adequacy for a theory of adjudication of testimony was to the effect that it should do justice to rival theories, at least in so far as they embody aspects of the tradition of historical practice. Let us start with the most starkly alternative approach, that which denies the need for justification in the "normal" cases in which doubts are not triggered by warning signs. Two obvious dismissive ploys turn out to be, equally obviously, unwise. The first of these "so what?" responses is to the effect that our genealogical theory is concerned only with cases in which testimony is adjudicated. What happens in the default cases, the ones in which no adjudication is triggered, is of no concern to it. The second, equally hasty, response is to the effect that doubtless in the default cases the acceptance is in fact the product of unconscious or tacit abductive inference to a vindicatory genealogy. The first of these hasty responses forgets that the various theories of adjudication propose to tell us in general terms when it is reasonable to accept testimony. So it is not open to any of them to remain thus agnostic about the cases in which no explicit adjudication occurs. The second response is too brisk because the genealogical theory becomes empirically implausible if construed as a sketch of often tacit psychological processes involved in all reasonable acceptance of testimony.

But unlike the first "so what?" response, the second one is along the right lines in conceding that the genealogical theory must come to terms with default/trigger accounts. However, to do so there is no need for doubtful postulations of

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unconscious mechanisms. Rather, what are needed are grounds for holding that the default acceptances of testimony by properly wary and critical persons are generally ones that would pass the genealogical test. To see this, it suffices to note that very high levels of incompetence and insincerity of testifiers, the conditions under which the genealogical account will deliver almost entirely negative verdicts, lead in obvious ways to evident breakdowns in social relations and institutions. Under tolerably clement social conditions, however, we may assume that there is a fair level of competence and sincerity in play, and hence that in the absence of counterindications testimonies are likely to be such as would pass the test. So default acceptance is under normal circumstances reasonable, especially since life is short and its exigencies often pressing. It is to the credit of the genealogical account that it thus commonsensically justifies acceptance as a default position, without having to invoke general conditions of rationality, humanity, or possibility of language.

The genealogical theory is at odds also with the simple inductive model which grounds the justification of acceptance of testimony on observed correlations between testimonies and attested facts. Quite apart from any general scepticism about induction, this model is famously conducive to a general scepticism about testimony arising from the sparseness of the observed correlations. The genealogical account does not simply ignore, but rather displaces the sceptical challenge.[20] It faces the well-known problem of justifying the mundane folk-psychological generalisations that figure in almost all such reconstructions. Quite apart from the doubts about them arising from unclarity about their evidential basis, there is the suspicion that such generalisations may be strongly conditioned by our own cultural and historical standpoints. It is, however, important to avoid undue polarisation of this issue. The options are not exclusive reliance, as in Seignobos's account, on the generalisations of a supposedly "scientific" psychology versus exclusive reliance on the norms and conventions of behaviour prevailing in the societies to which testifiers belonged. Rather, in the assessment of historical testimony, as in many other types of historical explanation, due account must be taken of psychological commonplaces as well as the norms and conventions of the period and society in question. It is, I think, a strength of the genealogical account that it replaces the drastic scepticism entailed by the inductive account with a moderate scepticism based on reasonable doubts about the historical constancy of human nature and about our ability to access the mentalities of testifiers from cultures very different to our own.

Our genealogical theory is, however, friendly to the traditional maxim-based accounts. For many of the maxims can be considered as providing prima facie indications of the competence and sincerity, or otherwise, of testifiers, and hence of the type of genealogy, whether of the vindicatory or of the critical sort, that would best explain the testimony. This is not, it should go without saying, an unqualified endorsement. Consider, for example, the injunctions, prevalent in early-modern Europe, to the effect that gentlemen are far more trustworthy than commoners

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and women. Without close historical investigation we cannot absolutely rule out the possibility that in certain societies gentlemanly codes of conduct produced surpassing trustworthiness; but it seems likely that in all periods and settings such maxims have been the product of "epistemic prejudice", useless as indicators of vindicatory or critical genealogies of testimony.[21]

As for the frequent distinction in maxim-based accounts between internal and external criteria of authenticity, this does not correspond to any evident distinction between types of explanation of the genesis of testimony. It does, however, on two counts mark a useful distinction between kinds of evidence that can be brought to bear in the quest for a critical or vindicatory genealogy. For external criteria concerning dating, circumstances of composition, and authorship can often be used to eliminate briskly testimonies unworthy of serious consideration. Further, the distinction marks a division of labour between types of historian, with those expert in the "technical" businesses of palaeography, epigraphy, codicology, diplomatics, etc., garnering external evidence, where those proficient in textual matters assemble the internal evidence.

Finally, and in my view crucially, the genealogical account provides a rationale, albeit a qualified one, for the pairing of maxims with countermaxims. Critics and forgers have, as Anthony Grafton has elegantly demonstrated, walked hand in hand through history (1990). What today the critic or judge discerns as the mark of authenticity and validity, the forger or bearer of false witness will tomorrow imitate in order to deceive. Thus, to take a well-known example, the circumstantial details and the vivid scenes that were widely recognised in the seventeenth century as distinguishing the matters related in genuine travellers' tales from the fabrications of mere romance and fable were brilliantly used by eighteenth-century novelists to lend authenticity to their exciting fictions (McKeon 1987, ch. 2). The careful descriptions, the naming of witnesses of gentlemanly standing, the indications of where original specimens might be seen, etc., that lent credence to the early *Philosophical Transactions of the Royal Society* were mimicked to perfection in the transactions of the imaginary Athenian Society, themselves in turn emulated in the transactions of the equally imaginary Lacedaemonian Society, a fake of a fake (Johns 1998, ch. 7). Hence the maxims and countermaxims: consistency is a mark of authenticity in testimony, but too meticulous a care for consistency may betray the contrivance of the deceiver; circumstantial detail lends credence, but too much of it may suggest the ingenuity of the dissimulator; etc. On the explanatory model, the qualified validity of the countermaxims is attributable to the reflexive nature of much testimony. For testimony is commonly couched in terms consciously crafted to display what are conventionally taken to be the marks of authority and authenticity. Thus it is that those who bear true witness most effectively are well-equipped to bear false witness to equal effect. Of course, this does not provide a blanket justification for appeal to countermaxims in the judgement of historical testimony. Rather, it implies that in judging testimony from a period and society in

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which certain maxims for judgement of testimony were widely held, the counters to those maxims should always be borne in mind.

Lastly there is the question of how our account relates to other accounts which treat adjudication of testimony as a form of inference to a causal explanation. We have already considered it in relation to Seignobos's account of the conjecture of historical facts on the basis of psychological laws governing the production of testimony. In addition to the shortcomings of Seignobos's psychological approach noted above, there is a more general problem with accounts which treat all the evidence which goes into the assessment of testimony as traces from which inferences are made. For there are many cases in which, though an assessment is manifestly "influenced" by certain apprehensions and beliefs, it does not involve actual inference from them. Consider, for example, the case in which I am told something I at first find a bit unlikely by a stranger whose subsequent conversation and behaviour inspires my confidence: my eventual acceptance of the testimony may well owe much to the trust-inspiring actions, although they played no part in my initial thinking up of a vindicatory genealogy. On the genealogical account this is unproblematic. Typically the inference is from the production of the testimony itself together with other clues, but very often the estimation of the goodness of the genealogy or genealogies I infer may be affected by other evidence, including evidence acquired subsequent to the production of the testimony.

Then there is the question of the relation of our account to probabilistic estimates of the strength of rival explanations of testimony. It is unclear how far such estimates can in principle eliminate appeal to other criteria of goodness: informativeness, precision, simplicity, etc; for it seems that these virtues will figure as criteria for the assignment of prior probabilities invoked in the calculations. But this leaves open the question how far probabilistic accounts can in fact be reconciled with genealogical adjudication.[22]

6. TESTIMONY IN THE CONTEXT OF HISTORICAL INQUIRY

Our second general requirement was to the effect that our theory should at least respect, and at best illuminate, the links between adjudication of testimony and the other basic procedures of historical inquiry having to do with interpretation and use of evidence. I shall illustrate the capacity of the genealogical account to do justice to these links by way of a historical example. But first, there is a general issue that must be faced by any account. Consider the case of multiple testimonies that p, in which some or all of those testimonies are when taken in isolation doubtful, because, for example, p is far-fetched and/or the testifier is flaky. At least when the testimonies are fully independent their multiplicity obviously renders them less doubtful, especially when p is far-fetched. But how can this be so? What is wrong with the view that, since taken individually the best explanations of the testimonies count against p, far from strengthening each other, they are mutually undermining? It is a strength of the genealogical theory that it blocks this suggestion. For in

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weighing up possible genealogies in cases of multiple testimonies that p, we are bound to check for independence of the testimonies. When the evidence for this is strong, then the case becomes correspondingly strong for postulating a single vindicatory genealogy, with p, or something that makes p probable, as the common origin of the testimonies.[23]

To illustrate the links between adjudication of testimony and other sources of historical evidence, let us consider some puzzling reports of Tycho Brahe concerning his observations of Martian parallax. Horizontal parallax of a heavenly body – the apparent difference between its positions when observed at zenith and at the horizon – increases with decreasing distance of the body from the earth. On the Ptolemaic earth-centred model of the cosmos, Mars is always further away than the Sun and hence should always show a smaller parallax than the Sun does. On the Copernican (heliocentric) and Tychonic (geoheliocentric, that is, the Sun goes round the Earth but the other planets go round the Sun) hypotheses Mars is sometimes closer to the Earth than is the Sun and should then show a larger parallax than the Sun ever does. In a letter of 1 November 1589 to the Imperial Physician Thaddeus Hagecius, Tycho Brahe reported that he had confirmed this prediction "from parallaxes of Mars most carefully examined in acronychal transit [that is, when in opposition to the Sun], especially in the year 1582 when the Martian parallaxes were somewhat larger than the solar one" (Brahe 1913–29, VII, 199). In 1584, however, referring to the 1582 observations, Tycho had written to a correspondent that he had not been able to detect Mars's parallax (VII, 80). We know that the parallax of Mars is far too small to have been detected with Tycho's instruments. However, Tycho supposed the Sun to be much closer than we do and hence to have a parallax that he could, at a pinch, detect. But, as Kepler noted, examination of Tycho's records shows his original report to have been correct: he had not detected Martian parallax in 1582 (Kepler 1937–, III, cap. xi). What are we to make of Tycho's testimony of 1589?

Tycho's later testimony was far from disinterested. For he was concerned not only to vindicate the geoheliocentric world-system he had published in 1588, but also to establish his priority in it over others, notably the Imperial Mathematician, Nicolai Reymers Baer (Ursus), who had published a similar system in the same year, claiming to have thought it up in 1584.[24] Both the advice to distrust inconsistent witnesses and the warning against those with an interest in misrepresentation suggest deception on Tycho's part.

A more charitable interpretation was suggested by Johannes Kepler. Noting that Tycho's 1582 observations did not show significant Martian parallax, he speculated that assistants might have misunderstood Tycho's instructions, computing the parallax not from the observations but as it ought to be according to the Copernican hypothesis. However, as Owen Gingerich and James Voelkel have shown in their close study of Tycho's repeated attempts to detect a Martian parallax, there is a very much better explanation (Gingerich & Voelkel 1998). Through computations of past night skies and meticulous reconstructions of

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Tycho's observational techniques, his records, and his calculations, together with an analysis of his work in the latter 1580s on stellar refraction, they show how Tycho briefly came to believe that, when corrected for refraction, his 1582 observations showed the required Martian parallax, as did further observations he had made in 1585 and 1587. If Gingerich and Voelkel's account is correct, and it is highly convincing, then the 1589 testimony about the 1582 observations was not an outright deception. For in 1589 Tycho really did believe that his 1582 observations implied Martian parallax. So the best explanation turns out to be in terms of incompetence – by our lights, not by the standards of the period – rather than dishonesty. Be that as it may, Tycho's testimony to Hagecius is misleading, suggesting as it does that he had in 1582, rather than later, come to believe that his observations entailed an appreciable parallax of Mars. And there is reason to believe that this is no accident. In later letters to Hagecius, Tycho denounced Ursus as a plagiarist; and it seems likely that in this communication Tycho was covertly pursuing his priority dispute with Ursus by backdating his formulation of an argument from parallax against the Ptolemaic system to 1582, two years before Ursus claimed to have thought up his new world-system.[25]

This example clearly illustrates the limitations of assessments of testimony which take into account only immediate circumstances of composition, psychological commonplaces about testifiers, and literary context. For we see here how assessment of a single apparently simple item of testimony may require a whole series of complex reconstructions: of Tycho's instruments and their uses, of the states of the night sky on particular dates, of Tycho's development of a theory of refraction, of the genesis of Tycho's cosmology, and of the course of his priority dispute with Ursus. And these reconstructions involve, as would be evident had we spelt them out fully, inferences about a host of historical circumstances remote from the composition of the letter itself, along with all sorts of more general considerations about beliefs and conventions in the period. It is a strength of the genealogical theory that it can do justice to the full range of types of evidence relevant to the adjudication of testimony.

The interpretation and the adjudication of testimony are often mutually dependent. Appreciation of the historical circumstances reconstructed through assessment of testimony is often crucial for its interpretation, this being, as already noted, one notable version of the "hermeneutic circle". As an example of the inseparability of interpretation from testimonial assessment, let us consider further Tycho's 1589 report to Hagecius that in 1582 he had ascertained that Mars in opposition was closer to the Earth than the Sun "from parallaxes of Mars most carefully examined", alongside his statement earlier that year to Christoph Rothmann that he had established the approach of Mars closer than the Sun "by means of accurate and precise observations". These statements are ambiguous on two scores. It is unclear whether or not the calculation of the parallax from the observed positions involved corrections for supposed sources of error. And more subtly, as Kepler's discussion of the matter shows, it is unclear whether Tycho

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made the observations personally or had them made for him by an assistant, for it was quite standard in the period for someone of Tycho's status honestly and literally to ascribe to himself acts of dependents, so that "I observed" could be sincerely stated by one whose servants or assistants had carried out the observation. Reconstruction of the practices at Tycho's observatory at Uraniborg supports the broad construal of "I" which covers activities of dependents and servants. And the genealogy which best explains Tycho's testimony implies that "observation" is to be taken in a broad sense, covering results obtained by correction of observations in the narrow sense for supposed error due to refraction. Of course, as in other typical instances of such "hermeneutic circularity", the inference here is not entirely circular: for there is plenty of collateral evidence from the period for such broad usages of the first person singular and of the term "observatio".

7. CONCLUSION: PROSPECTS

I have argued that a genealogical theory of adjudication of testimony has notable strengths: it explains, or rather explains away, the rival theories; it respects the long tradition of maxim-based practice of assessment of testimony; and it does justice to the connections of assessment of testimony with the procedures of interpretation and collation of evidence on which all historical inquiry is based. Our discussion has, however, brought out at least three knotty problems.

First there is a problem of explication. How far can probabilistic analyses help in estimation of the goodness of genealogical explanations of testimony? I have expressed strong doubt about the complete eliminability of non-probabilistic criteria, but, as already indicated, the prospects for probabilistic partial explanations of the grounds for choice between rival genealogies are promising indeed.

A second, and fascinating, problem has to do with the bounds of testimony, and in particular with the question of the presentation of testimony in images. I have suggested that the crucial issue here is that of the extent to which the various genres of images are governed by well-defined conventional languages of representation. Here we are apt to face a peculiarly dangerous hermeneutic circle. In textual interpretation it is generally the case that basic features of the language – of alphabet, grammar, vocabulary, etc. – are established quite independently of the interpretation and adjudication of particular testimonies. Not so in many of the visual cases – here we are in a situation more like that of the first interpreters of Linear B, in which hypotheses about the elements of the language were hardly extricable from hypotheses about what was being conveyed (Chadwick 1958). Consider, to take a famous example, the hotly disputed issue of allegorical or "iconographic" readings of early-modern paintings. Such interpretations presuppose elaborate symbolic codes grounded in classical mythology.[26] But there is often little independent textual evidence of the symbolic codes. Further, what evidence there is, notably from emblem books and "iconotexts" – that is, texts which form parts of an image – shows that much of the allegorical symbolism

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was wilfully multivalent, designed to provoke varied and often playful readings.[27] So the suspicion arises that here the hermeneutic circle may be a truly question-begging one, the messages allegedly conveyed by the paintings depending on the assumption of the symbolic codes, and most of the evidence for the codes being constituted by the supposed messages.

The third, and for practising historians most salient, of the difficulties has to do with the roles in the explanation of testimony of universals of folk psychology on the one hand and local norms and conventions on the other. There can, I think, be no general solution to this problem; but it may be observed that the genealogical model offers prospects for fruitful empirical investigation of the issue by historians. Consider, to take an example of considerable interest to historians of early-modern European culture, the question how far forms of communication within learned and elite circles were governed by local norms relating to honourable conduct and exchange as opposed to supposedly more universal "Hobbesian" principles of self-interest.[28] By considering cases in which the worth or worthlessness of testimonies is clinched by independent evidence for the fact attested, historians can hope to estimate the extents to which honourable codes and self-interested stratagems governed the giving of those testimonies. Tycho's testimony about his observations of the parallax of Mars can be used to illustrate this. Collateral evidence indicates that here, as in others of his claims about his path to discovery of his world-system, Tycho was honest about his "raw" observations and his inferences from them, but that on occasion he slightly backdated them and somewhat downplayed the contributions of others (Gingerich & Voelkel 1998). Two hypotheses spring to mind. The first is that both the dishonesty on minor points and the honesty on major ones were self-interested, the former being motivated by his desire to be credited with priority in his world-system, the latter by his wish not to be discredited by being exposed as incompetent and/or fraudulent. The alternative hypothesis is to the effect that his code of honour required honesty about big issues, but allowed self-interested manipulation of details. Though by our present standards of behaviour, the latter explanation seems the less plausible, it receives support from consideration of Tycho's edition of his correspondence with other astronomers, the *Epistolae astronomicae* of 1596. One of the modern editors of this edition opined that Tycho had committed serious frauds, planting in the letters of others passages favourable to his priority claims and unfavourable to his rivals (Raeder 1920/2003). But close investigation has refuted this claim, showing that although Tycho selected the material to be published in ways favourable to his cause and made some minor adjustments so as to omit confidential materials and to amplify the respect with which others had addressed him, he did not commit the supposed major frauds (Mosley, Jardine, & Tybjerg 2003). Though improper by modern editorial standards, his editing was well within the bounds of the honourable editorial practices which he claimed, in the introduction to the work, to have followed. This, together with the obsession with issues of honour displayed by Tycho in his many controversies, strongly suggest that honour, and not just

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self-interest, played a large role in keeping him within the (to us rather loose) bounds of propriety of the period in his testimonies.

So, to end on a cheerful note, the theory of adjudication of testimony promoted here opens up promising lines of investigation. For philosophers there is the issue of the extent to which adjudication of testimony on explanatory genealogical grounds can be explicated in probabilistic terms, and for historians (sociologists, and anthropologists) there is the project of using testimonial genealogies to explore the relative contributions of universal and local norms in the governance of behaviour.

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NOTES

1 See, for example, Graham (1997).

2 Cf. Dretske's (1981, chs. 2 and 8) distinction between meaningful representations and other sources of information.

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3 For interesting reflections on the extent to which these genres are governed by conventions, see Burke (2001).
4 For a fascinating treatment of the issue, see Montagu (1994).
5 Lackey (1999); see also Lackey (2008), ch. 2.
6 For a comprehensive overview of treatments of adjudication of testimony in logical works, see Kennedy (2004); also, Serjeantson (1999). On specifically legal treatments, see Otte (1971) and Maclean (1992).
7 On the history of theories of interpretation, see Eden (1997), Szondi (1975/1995), Wach (1926/1996). On biblical criticism see, for example, Frei (1974).
8 See, for example, Bernheim (1889).
9 See, for example, Schleir (1990). Ranke did not himself produce any codification of critical methods, although his polemical *Zur Kritik neueren Geschichtschreiber* (von Ranke 1824) does explicitly promote a scholarly historiography founded on extensive critically examined archival materials.
10 It would be interesting to look into the connections between this feature and the adversarial practices of the common law tradition: see, e.g., Shapiro (1983, ch. 5).
11 On maxims and countermaxims in the early modern period, see Shapin (1994, 232–8).
12 Here I follow the terminology of Fricker (1995).
13 See, for example, Owen (1987), Sobel (1987).
14 On Ancient Greek treatments of inference from signs and traces, see the classic Detienne & Vernant (1974/1978).
15 On inference from signs, see Ginzburg (1983).
16 Note that the French '*document*' is here a false friend to English 'document', in that it covers both textual and non-textual sources of information.
17 Indeed, J. M. Chladenius's (1757) notion of *Standortsgebundenheit*, "dependency on standpoint", relates specifically to the ways in which the reporting of facts is conditioned by the historical situations of witnesses. On this aspect of Chladenius's theory of interpretation and its reception, see Szondi (1975/1995, chs. 4 & 5).
18 Here, and in what follows, I draw on the inferential accounts of assessment of testimony of Fricker (1995) and Lipton (1998, 2007).
19 For witty observations on the interest relativity of explanation, see Putnam (1978, 41–5).
20 This displacement is noted by Seignobos (1887, 28ff).
21 On epistemic prejudice with respect to women's testimony, see Fricker (2007).
22 Lipton (2004, ch. 8) has argued cogently that the two approaches are complementary. This view is confirmed by the fascinating probabilistic analyses of the weighing of common-cause against multiple-cause explanations of matching testimonies by Friedman (1987) and Tucker (forthcoming).
23 See Friedman (1987).
24 Ursus (1588). On Ursus's world-system, see Schofield (1981), Lerner (1997, vol. 2, 142–6), Granada (1996, ch. iv). On Tycho's priority dispute with Ursus, see also Jardine (1984, chs. 1 & 2), Rosen (1986), Gingerich & Westman (1988).
25 In the prefatory letter to his *Tractatus*, addressed to Moritz, Landgraf of Hesse-Kassel, Ursus sought to undermine Tycho's priority claim by asserting that he had thought up his own system in 1584 whilst living in Pomerania, and that in 1586 Jost Bürgi had made a model of Ursus's system for Moritz's father, Wilhelm IV.

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26 See, for example, Charles Hope's (1981) memorable attack on overly allegorical "iconographic" interpretations of paintings by Titian which to the untutored eye seem merely to portray plump courtesans; see also Burke (2001, ch. 2).

27 On the roles of ambiguous and esoteric emblems as stimuli to courtly displays of wit, see, for example, Biagioli (1990), Starn (1989).

28 An excellent work on early-modern codes of honour is Neuschel (1989). For an account of early modern "scientific" practices which emphasises codes of honour, see Biagioli (1993). For a more Hobbesian approach, see Shapin & Schaffer (1985).

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