

## *Quare?* Argument in David Daube, after Karl Popper

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Daube's work has a quality we can admire even when we are not persuaded by it: he will explain a text in a way which is entirely unexpected, but which seems suddenly to reveal something that had lain unnoticed. How does he do this? According to Alan Rodger,<sup>1</sup> Daube would notice something in a text and ask why it was there; he would then explain the text by answering the question.<sup>2</sup>

This essay discusses Daube's method of reading texts, and discusses in particular why it is useful to begin with a question, how Daube finds the right questions to ask, and what makes one answer better than another. I argue that Daube's method of reading texts produces the explanations it does because it does not rely on *inferences* from the text so much as *prior guesses* about what the text means. Though much of this essay is concerned with Daube's work, my interest is not in Daube's work alone, but in a wider issue I regard as enormously important: how a method like Daube's contributes new ideas to the Roman law literature.

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\* In preparing this essay I have had the benefit of correspondence with Daube's widow, Helen Smelser Daube, and the advice of Charles Donahue, Jr., of the Harvard Law School.

<sup>1</sup> Above, 11–14.

<sup>2</sup> A few examples of this kind of argument may be found in these works by Daube: "Certainty of Price," in D. Daube (ed.), *Studies in the Roman Law of Sale* (Oxford, 1959) [= *Collected Studies*, 2:757], 17–20 (D.18.1.37); "Turpitude in Digest 12.5.5," in R. S. Bagnall and W. V. Harris (edd.), *Studies in Roman Law in Memory of A. Arthur Schiller* (Leiden, 1986) [= *Collected Studies*, 2:1403], 33–38 (D.12.5.5); "Generalisations in D.18.1. *de contrahenda emptione*," in *Studi in onore di Vincenzo Arangio-Ruiz* (Naples, n.d.) [= *Collected Studies*, 1:527], 1:186–92 (D.18.1.28); "Das Selbstverständliche in der Rechtsgeschichte," 90 *ZSS* (rom. Abt.) 1, 12–13 (1973), translated as "The Self-Understood in Legal History," 18 *Jur. Rev.* (n.s.) 126 (1973) [= *Collected Studies*, 2:1277] (Gaius 1.5); "The Earliest Structure of the Gospels," 5 *New Testament Studies* 174 [= *Collected Works*, 2:329], 174–87 (1959) (Mark xii. 1–37; Matt. ii. 13–18); *Roman Law: Linguistic, Social and Philosophical Aspects* (Edinburgh, 1969), 72–3 (XII Tab. V, 4), 93–4 (D.46.3.67); "The Finale of Horace's 'Satire' 1.4," 22 *Index* 371, 371–88 (1994) (Hor. *Serm.* 1.4.139–143). The first four of these texts are discussed below.

In discussing Daube's "question" I rely on certain works of Karl Popper, a philosopher of science from the last century. This needs some explanation. Daube was not influenced by Popper, so far as I am aware.<sup>3</sup> The value of Popper's thought to this discussion lies in the fact that Popper was a critic of induction, and that Daube's method of reading texts is non-inductive. Popper very much favored the kind of creative, hypothesis-driven research that Daube practiced, and Popper strongly defended this kind of research against the claim that a researcher is simply a fact-finder. Accordingly Popper's works, I believe, explain in an exceptionally clear way what makes Daube's "question" valuable.<sup>4</sup> On the other hand, Popper held certain controversial views about induction, i.e., that one may prefer a hypothesis based in part on its ability to survive, rigorous, negative tests ("falsification"). Popper's theory of falsification, in fact, is what he is best known for. I should make clear that these views are not part of this essay, simply because, in my opinion, they are not useful to historians — something Popper himself all but admitted.<sup>5</sup>

#### I. Ulpian on the certainty of price in sale

It will be useful to have a single text to lead the discussion, and for this I use the following text of Ulpian: *D.18.1.37* (3 *Disputationes*), on the requirements of a contract of sale. The text was analysed by Daube in a 1959 volume dedicated to de Zulueta.<sup>6</sup>

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<sup>3</sup> I have no reason to believe that Daube consciously followed Popper, and I am not aware that Daube ever cited Popper. However, I have learned from Daube's widow, Helen Smelser Daube, that although her husband, as far as she knows, did not own any of Popper's books, she had many conversations with him about Popper. She herself attended a seminar by Popper at the London School of Economics in 1952.

<sup>4</sup> On the other hand, a different essay could be written comparing Daube to, for example, R. G. Collingwood (who argues that history is a reenactment by the historian) and Hans-Georg Gadamer (who accepts *Vorurteil* as necessary to the understanding of a text). See R. G. Collingwood, *The Idea of History* (Oxford, 1946), 282–302; H.-G. Gadamer, *Truth and Method*, 2nd ed. (London, 1975), 238–40; P. Skagestad, *Making Sense of History: The Philosophies of Popper and Collingwood* (Oslo, 1975), 87–91. There is also room for comparison in the psychology of art; there is a clear counterpart to Daube's question-and-answer in the "making and matching" described by Ernst Gombrich, himself a Popperian. See E. H. Gombrich, *Art and Illusion*, 5th ed. (London, 1977), 157–61.

<sup>5</sup> See the discussion in note 29 below, and the authorities cited in note 43 below.

<sup>6</sup> Daube, "Certainty of Price" (note 2), 9–45. This text, as with all other texts from the *Digest* in this essay, is that of T. Mommsen and P. Krüger (edd.), *Digesta Iustiniani Augusti* (Berlin, 1870; reprinted 1962–63).

Si quis fundum iure hereditario sibi delatum ita vendidisset: "erit tibi emptus tanti, quanti a testatore emptus est," mox inveniatur non emptus, sed donatus testatori, videtur quasi sine pretio facta venditio, ideoque similis erit sub condicione factae venditioni, quae nulla est, si condicio defecerit.

If one had sold a plot of land, which came to him by inheritance, with the provision that "it shall be purchased for as much as it was purchased by the testator," and it is then discovered that it was not purchased by but given to the testator, it is treated as a sale made without a price, and is therefore similar to a sale under a condition, which is void if the condition fails.

Daube addresses the extent of interpolation in the text (the meaning, he notes, is quite clear). The question raised in the text is whether the events give rise to a sale which fails for want of a price. In the state in which it comes to us, the text makes it clear that the seller has introduced the "erit . . . est" language innocently, and that only later is it discovered that the sale lacks a price. There is, as Daube notes, a superficial resemblance to a condition that fails, but treating the parties' ignorance and subsequent realization as a failed condition makes a jumble out of invalid and imperfect sales. Hence the clause introduced by *ideoque* is unlikely to be Ulpian's.<sup>7</sup>

Beseler undertook to restore this text<sup>8</sup> and excised not only the *ideoque* clause (probably, as Daube suggests, on the argument that *ideoque* is a sign of interpolation) but a great deal more. I have set out his emendations less conventionally to make clear the extent of his changes.<sup>9</sup>

Si quis fundum, qui defuncto donatus erat, iure hereditario sibi delatum ita vendiderit ~~vendidisset~~: "erit tibi emptus tanti, quanti a testatore emptus est," nulla est venditio ~~mox inveniatur non emptus, sed donatus testatori, videtur quasi sine pretio facta venditio, ideoque similis erit sub condicione factae venditioni, quae nulla est, si condicio defecerit~~.

If someone has sold a plot of land which had been given to one deceased (and which came to the seller by inheritance) with

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<sup>7</sup> Daube, "Certainty of Price" (note 2), 19.

<sup>8</sup> G. von Beseler, "Romanistische Studien," 8 *T. v. R.* 279, 291 (1927).

<sup>9</sup> *Id.*: — fundum <, qui defuncto donatus erat,> — <vendiderit> [vendidisset] — est, <nulla est venditio> [—].

the provision that "it shall be purchased for as much as it was purchased by the testator," there is no sale.

Daube mentions several inappropriate points of style in Beseler's text, but his main objection is that, on Beseler's view, the compilers have added a fact not in the original: that the parties have agreed on the condition in ignorance of the true state of affairs. Daube does not speculate on Beseler's reasons for this change (except to say that it was done as part of the addition of "ideoque . . ."). He marks his disagreement with Beseler by the following pointed statement:<sup>10</sup>

[O]ne can think of no plausible motive which might have induced anyone to turn Beseler's text into the present.

I postpone discussion of this statement for later: it expresses the kind of question which Rodger takes as the hallmark of Daube's method. For now it is enough to point out that this is the sort of question Beseler did not feel the need to ask.

On the other hand, "plausible motive" aside, we can guess at Beseler's reasoning without too much difficulty. He has begun with idea that *ideoque* signals an interpolation.<sup>11</sup> He has then noted that the nature of the interpolation is a misplaced analogy with conditions. He has then concluded that the introduction of the vendor's ignorance is of a piece with the misplaced analogy; that the compilers have misunderstood the thrust of the text (*certum pretium*) and attempted to make the text fit a rule about conditions; and that to accomplish this they have inserted a spurious discussion of ignorance and subsequent realization.<sup>12</sup> Be-

<sup>10</sup> Daube, "Certainty of Price" (note 2), 18.

<sup>11</sup> See G. Beseler, [Miscellen], 45 ZSS (rom. Abt.) 456, 456 (1925) (speaking of *ideoque*, among other expressions): "Diese Redeform ist in den Klassikertexten immer oder meistens unecht."

<sup>12</sup> The context gives another clue to Beseler's reasoning. He was responding to a discussion of risk in contracts of sale in E. Seckel and E. Levy, "Die Gefahrtragung beim Kauf im klassischen römischen Recht," 47 ZSS (rom. Abt.) 117 (1927). Seckel and Levy had discussed sales in which the price, though certain, was unknown to the buyer (see *D.18.1.7.1*, 2 (Ulpian 28 *Sab.*)). They commented that this sort of sale was valid but not perfect, and in the course of this discussion, at 162 n.4, directed the reader "Vgl. auch Ulp. D. eod. 37." Beseler refers to this footnote at the end of his restoration of *D.18.1.37*. He no doubt recognized that Seckel and Levy have miscited § 37: the parties' knowledge or lack of knowledge is, in that text, beside the point. And perhaps, in his enthusiasm to make this clear to readers of Seckel and Levy (and to take a dig at the authors), he simply dismissed the whole discussion of knowledge as interpolation.

Beseler has a good point, of course: the *max inveniatur* etc. is not strictly relevant. But Daube very plausibly says that the case is "no doubt

seler leaves us with a text that raises certain questions of style,<sup>13</sup> but there is no doubt that he has accounted for the interpolations (if I have reconstructed his thoughts correctly), and that his reasoning is entirely orthodox.

Daube's reasoning, on the other hand, is not orthodox. He asks how the text came to be changed, and then simply suggests an answer. Justinian, he says, was particularly proud of having settled a dispute among the classical jurists<sup>14</sup> regarding the validity of contracts of sale or hire whose price or charge was to be fixed by a third person. Justinian decided that this sort of contract was conditional and would fail if the third person, in the event, did not fix the price.<sup>15</sup> Daube suggests that, when the compilers turned their attention to *D.18.1.37*, it occurred to them that Justinian's decision might solve this controversy as well. Ulpian's choice of words may have reminded them of Justinian's decision; Ulpian says that, because the land had been given to the testator, the sale was *quasi sine pretio facta*, and these words perhaps called to mind Justinian's words, *quasi nullo pretio statuto* (*C.4.38.15.2*). The compilers, according to Daube, therefore applied Justinian's solution to this text, treating the case as one of an heir selling conditionally. Ulpian's text, of course, makes no mention of third persons; Daube's explanation is based solely on the supposed enthusiasm of the compilers for Justinian's decision and the way in which that enthusiasm may have left its mark on Ulpian's text.

Whatever the merits of Daube's argument, there is no denying its main appeal: if the compilers were enthusiastic in the way Daube describes, then the text would look just as we have it. But do we have any reason for accepting Daube's argument, other

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one which occurred in reality" and is set out as it happened. Daube, "Certainty of Price" (note 2), 19.

<sup>13</sup> Noted in Daube, "Certainty of Price" (note 2), 18.

<sup>14</sup> See Gaius 3.140 (sale), 143 (hire).

<sup>15</sup> *C.4.38.15* (AD 530):

Super rebus venundandis, si quis ita rem comparavit, ut res vendita esset, quanti Titius aestimaverit, magna dubitatio exorta est multis antiquae prudentiae cultoribus. 1. Quam decedentes censemus, cum huiusmodi conventio super venditione procedat "quanti ille aestimaverit," sub hac condicione stare venditionem, ut, si quidem ipse qui nominatus est pretium definierit, omnimodo secundum eius aestimationem et pretia persolvi et venditionem ad effectum pervenire . . . . 2. Sin autem ille vel noluerit vel non potuerit pretium definire, tunc pro nihilo esse venditionem quasi nullo pretio statuto . . . .

See also *D.19.2.25* pr. (Gaius 10 *ed. prov.*, but interpolated); *Institutes* 3.23.1, 3.24.1.

than this close fit between the explanation and the text? On this point some would prefer Beseler's argument, because the pattern of interpolation he identifies, uncertain as it is, nevertheless does support his argument: on the basis of comparable evidence (comparable instances of *ideoque*) he has formulated a rule ("*ideoque* signals an interpolation") which is applied to the text at hand and tells us by deduction that the text is interpolated. Even if people differ on how clearly the *ideoque*-rule is made out and how deeply the interpolation extends, the method of argument seems to be above reproach. Beseler has apparently achieved something lacking in Daube's argument: Daube has given us an answer, but no reason for accepting the answer. Some might even say that Beseler has given an argument, Daube only his opinion.

For purposes of understanding *D.18.1.37*, it makes a difference which argument one prefers. Both Beseler and Daube agree that the text is interpolated, and indeed both agree that "*ideoque*" here is not classical. But the arguments produce two very different texts. Beseler's argument produces a text in which the issue of the seller's knowledge is simply absent. Daube's argument produces a text in which the issue of the seller's knowledge (though irrelevant) is present, a text which therefore holds out to the would-be buyer some prospect of a remedy.<sup>16</sup>

## II. Objectivity and induction

For many, Beseler's explanation of *D.18.1.37* is the better one because Beseler's method is the more *objective*. The questions a romanist asks are usually regarded as questions with (theoretically) objective answers. This is presumably why the quality of argument matters in the first place. Even a question with a good deal of abstraction, e.g., whether the *ius honorarium* exerted greater influence on the law at the end of the Republic than thereafter, is regarded as a question with an objective answer.<sup>17</sup> And objectivity might seem to be best served by showing that the right explanation originates in a body of evidence, and not in the researcher's impressions of what the right explanation looks like.<sup>18</sup> That is Beseler's apparent advantage: he begins with ex-

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<sup>16</sup> That, at any rate, is what Daube believes: Daube, "Certainty of Price" (note 2), 18.

<sup>17</sup> See, for example, the treatment in W. Kunkel, *An Introduction to Roman Legal and Constitutional History*, 2nd ed. trans. J. M. Kelly (Oxford, 1973), 81–82.

<sup>18</sup> Daube himself seems to have doubted what he took to be the assumption of legal historians "daß der Wissenschaftler sich zu einer gewis-

amples, showing a pattern of usage of *ideoque* in ancient sources. A reader sees that the pattern lies in the sources, not in Beseler's head, and if he is satisfied that the sources say what they purport to say, then he demands nothing more from Beseler by way of objectivity.

The inductive power of Beseler's examples has certain obvious limits. We can accept, on the basis of the surviving evidence, that Beseler has made a very good case for the *ideoque*-pattern, and that the pattern is a reliable one for many purposes. But to speak strictly of "probability," and assert that "Beseler's explanation is more probable than Daube's," is not possible. There is no "text-transmission deity" who has guaranteed the survival of every usage and pattern, something a coherent sense of probability requires.<sup>19</sup> This, in fact, is the unstated foundation of Lenel's criticism of Beseler's philological method. Where Beseler notices that a word or expression is rare in juristic sources, Lenel reminds him, first, that rarity is a function of the underlying sample,<sup>20</sup> and then, that if a particular usage of a word is otherwise unattested in one's sample, there may be a good reason for that fact, unknown to us; it does not mean the usage is impossible or even unlikely.<sup>21</sup> In short, the state of the evidence precludes any genuine talk of probability.<sup>22</sup>

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sen Stufe von Objectivität aufschwingen kann." Daube, "Das Selbstverständliche" (note 2), 1.

<sup>19</sup> I am obviously skirting the far more difficult question, whether a multiplicity of examples *ever* makes any explanation "more probable." I do not discuss this below, but Popper's views on the question are summarized well in R. Corvi, *An Introduction to the Thought of Karl Popper* (London, 1997), 36–41, and D. Gillies, "Popper's Contribution to the Philosophy of Probability," in A. O'Hear (ed.), *Karl Popper: Philosophy and Problems [Royal Institute of Philosophy Supplement 39]* (Cambridge, 1995), 103–20.

<sup>20</sup> O. Lenel, "Kritisches und Antikritisches," 49 *ZSS* (rom. Abt.) 1, 18–23 (1929). Lenel's point is simply that some of the words and expressions that Beseler regards as justinianic are found in classical *literary* sources.

<sup>21</sup> For example, Lenel criticizes Beseler's rejection of *coniux* as justinianic on the basis of two doubtful occurrences in the *Vocabularium Iurisprudentiae Romanae*. Lenel (note 20), 20; see G. von Beseler, "Miscellanea Graecoromana," in *Studi in onore di Pietro Bonfante* (Milan, 1930), 2:63 n.6. Lenel suggests that the jurists perhaps often, but not always, preferred synonyms to *coniux*. This is all it takes to see off Beseler's argument.

<sup>22</sup> The probability issues that arise in philological interpolation-hunts are better recognized now than they once were. Albertario, like Beseler, argued with great specificity that certain words or expressions were peculiar to the compilers. E. Albertario, *Introduzione storica all studio del diritto romano Giustiniano* (Milan, 1935), 50–51. Buckland answers Albertario with perfect Daubean logic: even if they *are* not at-

This is not, of course, a rejection of induction *per se*, but only a rejection of the idea that, in comparing one explanation with another, many examples are necessarily more probative than few. Inductive reasoning, in fact, very much favors Beseler over Daube, simply because Beseler has given examples of what he believes has taken place in *D.18.1.37*, where Daube has given no examples whatsoever. In Beseler's examples, there is apparently some probative value, however small, to the meaning of *ideoque*, etc., in *D.18.1.37*. Daube's evidence (*C.4.38.15*, on prices fixed by third persons) has nothing in common with *D.18.1.37*: we recall that Ulpian's text makes no mention of third persons, and that there is simply no connection between *D.18.1.37* and *C.4.38.15*, other than the one Daube asks us to accept. The critic would say his explanation "is true if it's true." Induction also favors Beseler because his role as researcher seems more passive: he opens the *Vocabularium Iurisprudentiae Romanae*, and it gives up certain information *sua sponte*.

The issue here is whether Daube can make a case (as indeed he tries to) without any pretence to induction whatsoever.

### III. Karl Popper

Karl Popper (1902–1994) is probably the best known philosopher of science from the last century.<sup>23</sup> His principal writings are on epistemology, and his research led him to certain conclusions about the acquisition of knowledge in both the natural and social sciences.<sup>24</sup> Very little needs to be said here about Popper's broad epistemological theory, except that he argued tirelessly against

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tested in classical sources, why *should* they not be? W. W. Buckland, "Interpolations in the *Digest*: A Criticism of Criticism," 54 *Harv. L. Rev.* 1273, 1289 (1941). The problem of generalizing usage in this way is discussed in D. Johnston, "Justinian's *Digest*: The Interpretation of Interpolation," 9 *Oxford J. Leg. Studies* 149, 150 (1989).

<sup>23</sup> The principal work by which he made his scientific reputation was *Logik der Forschung*, first published in 1934, and first published in English in 1959: *The Logic of Scientific Discovery* (London, 1959; revised 1968). An earlier epistemological work, superseded by *Logik der Forschung*, was first published only in 1979: Karl R. Popper, *Die beiden Grundprobleme der Erkenntnistheorie [1930–33]*, ed. T. E. Hansen (Tübingen, 1979). A second edition appeared in 1994.

<sup>24</sup> In social relations he argued against a form of historicism he found in Plato, Hegel, and Marx, and in favor of what he called the "open society." His principal writings in this field are *The Open Society and its Enemies*, 5th ed. (London, 1966), and *The Poverty of Historicism* (London, 1961). Essays on the same themes were published in *In Search of a Better World* (London, 1992) and, after his death, in *Lesson of this Century* (London, 1997).

the claims of induction in all sciences. According to Popper, induction from examples cannot produce certainty, however many examples are brought forward, and however clear the observed regularity or "law" appears to be.<sup>25</sup> Certainty exists whenever a law is falsified by an observation, but no accumulation of observations will ever verify it.<sup>26</sup> None of this shakes Popper's faith in scientific method: he does not propose abandoning observation as the basis of research,<sup>27</sup> and even less would he reduce "science" to the subjective experience of the scientist.<sup>28</sup> His concern instead is with certain very practical issues that matter to any researcher in any field: how conclusions are expressed and criticized, and what makes one theory better than another.<sup>29</sup>

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<sup>25</sup> Popper argues that the problem was correctly identified, but incorrectly solved, by Hume. See Popper, "Two Faces of Common Sense" (note 28), 85–90; L. A. Selby-Bigge (ed.), *David Hume: Treatise of Human Nature*, 2nd ed. rev. P. H. Nidditch (Oxford, 1978), book I, part III, sec. xii. It is worth pointing out that Popper's "rejection of certainty" is by no means an eccentric view. See D. Miller, *Critical Rationalism: A Restatement and Defence* (Chicago, 1994), 2: "[A]lmost everyone these days is a fallibilist; almost no one now supposes that empirical statements, even simple observational ones, can be established with certainty." This consensus extends even to Thomas Kuhn, one of Popper's principal intellectual opponents. See B. Barnes, "Thomas Kuhn," in Q. Skinner (ed.), *The Return of Grand Theory in the Human Sciences* (Cambridge, 1985), 87: "Like the rationalists [Kuhn] cannot see how scientists reason securely from the data to the correct theory. But this leads him to ask whether the theory might not be accepted on some other basis."

<sup>26</sup> A concise treatment of Popper's mature views on falsification are in K. R. Popper, "Conjectural Knowledge: My Solution to the Problem of Induction," in *Objective Knowledge: An Evolutionary Approach*, rev. ed. (Oxford, 1979), 1–31.

<sup>27</sup> K. R. Popper, "Truth, Rationality, and the Growth of Scientific Knowledge," in *Conjectures and Refutations*, 5th ed. (London, 1989), 235; idem, *Poverty of Historicism* (note 24), 131–33.

<sup>28</sup> See K. R. Popper, "Two Faces of Common Sense: An Argument for Commonsense Realism and against the Commonsense Theory of Knowledge," in *Objective Knowledge: An Evolutionary Approach*, rev. ed. (Oxford, 1979), 35–37.

<sup>29</sup> The question of preference among theories is a contentious one among Popper's followers: see Miller (note 25), 113–14; I. Lakatos, "Falsification and the Methodology of Scientific Research Programmes," in I. Lakatos and A. Musgrave (edd.), *Criticism and the Growth of Knowledge* (Cambridge, 1970), 91–195, especially 95–103. Popper himself admitted that his solution was unsatisfactory in this respect. K. R. Popper, "Supplementary Remarks (1978)," in *Objective Knowledge: An Evolutionary Approach*, rev. ed. (Oxford, 1979), 367–74. Whatever its general merits, theory selection based on negative testing is not useful to the study of Roman legal texts, in my opinion: (1) the evidence is not complete enough to allow "rigorous testing"; (2) the time during which a given theory has been in currency is significant in Popper's theory but means

As I mentioned above, my interest here is not in Popper's so-called solution to the problem of induction, but in his more general discussions of induction and scientific method. Popper expresses certain anti-inductivist views that I believe Daube shared, and that explain very clearly and specifically Daube's exceptional creativity in reading texts. These views, generally speaking, reject the idea that a researcher may passively observe patterns in a body of evidence, and they offer an alternative description of how any researcher, including a historian, may explain an event — such as the condition of a text. These views are discussed below.

#### IV. Historical explanation

Popper was eager to discredit the idea of "pure observation." Some of his contemporaries in the early part of the last century<sup>30</sup> had assumed that an observer could watch a series of events, and that some regularity or "law" would emerge from the events and *proprio motu* make itself apparent to the observer. Scientific observation, according to this school, is passive, or at least uncorrupted by the observer's thoughts: the evidence speaks to the observer and the observer reports what he hears. One who held this view would, for example, accept that when a romanist like Beseler looks at a series of texts and identifies a word or phrase as "peculiar to the compilers," the texts have given up this conclusion without any substantive assistance from Beseler himself. In reply Popper makes a very practical observation: unless the conclusion is at least partly in the observer's mind at the outset, he will not know what evidence to look at.<sup>31</sup> The observer begins

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nothing in Roman law; and (3) to follow Popper in this respect would require us to favor theories which, by chance, have not been "falsified" by the survival of some piece of evidence, and to reject an entire theory which seems to explain a great deal about the Roman world but is contradicted by a single text. Similar objections may be found in Skagestad (note 4), 94.

<sup>30</sup> Popper's own account of his battles with the Vienna Circle, and logical positivism generally, are in his autobiography, K. Popper, *Unended Quest* (La Salle, 1982), 80–90. Many corrections to Popper's own account may be found in M. H. Hacothen, *Karl Popper: The Formative Years 1902–1945* (Cambridge, 2000), 208–13. There are also two very good popular accounts: B. Magee, *Confessions of a Philosopher* (London, 1997), 55–68; D. Edmonds and J. Eidinow, *Wittgenstein's Poker* (New York, 2001), 165–73. Popper believed strongly that positivism ("what is true is what is proven true") was hindering the proper understanding of scientific knowledge. On this brand of positivism see Lakatos (note 29), 91–92; Popper, "Truth, Rationality, and the Growth of Knowledge" (note 27), 228.

<sup>31</sup> K. R. Popper, "The Bucket and the Searchlight: Two Theories of Knowledge," in *Objective Knowledge: An Evolutionary Approach*, rev. ed.

with some manner of conclusion — a hypothesis — and uses the hypothesis to find his evidence. He is not overtly looking to shore up some preconceived answer, but only trying to isolate the kind of evidence which may prove helpful. Without a hypothesis, nothing is relevant, and hence nothing can be examined.<sup>32</sup> Accordingly on Popper's view, the justinianic words and phrases which Beseler identifies may be exactly what he says they are, but he nevertheless actively assisted in their discovery: unless he had first stopped to ask, e.g., whether *ideoque* was justinianic, he would not have examined texts with *ideoque*.

Popper's view, therefore, is that scientific observation is not passive but "theory-laden."<sup>33</sup> Every observer has in his head certain hypotheses which guide his observations and indeed interpret the evidence at the very moment the observation is made, and observation is impossible without such hypotheses.<sup>34</sup> Theory-laden observation is of course a very old idea;<sup>35</sup> Popper had the

(Oxford, 1979), 342, 344. See also K. R. Popper, "Towards a Rational Theory of Tradition," in *Conjectures and Refutations*, 5th ed. (London, 1989), 128 (note omitted):

Those among you who hold the opposite view and who believe that scientific theories are the result of observations, I challenge to start observing here and now and to give me the scientific results of your observations. . . . But even if you go on to the end of your lives, notebook in hand, writing down everything you observe, and if you finally bequeath this important notebook to the Royal Society, asking them to make science out of it, the Royal Society might preserve it as a curiosity, but decidedly not as a source of knowledge.

<sup>32</sup> In keeping with his own falsification solution, Popper regarded hypotheses as something to be retained but refined over time; conventionally, a hypothesis is replaced by a theory, after experiment has proved the hypothesis to be "true." See Popper, *Poverty of Historicism* (note 24), 131.

<sup>33</sup> Popper, *Open Society* (note 24), 2:259–60; idem, *Poverty of Historicism* (note 24), 134; idem, "On the Status of Science and of Metaphysics," in *Conjectures and Refutations*, 5th ed. (London, 1989), 184–93; idem, "Science: Problems, Aims, Responsibilities," in M. A. Notturmo (ed.), *The Myth of the Framework* (London, 1994), 86; idem, "The Pluralist Approach to History," in M. A. Notturmo (ed.), *The Myth of the Framework* (London, 1994), 145.

<sup>34</sup> See especially the quote reproduced above in note 31.

<sup>35</sup> It is an idea found in, among others, Vico, Locke, Hume, and of course Kant. On Vico's claim to be the first to develop the idea, see I. Berlin, "Vico's Theory of Knowledge and its Sources," in *Three Critics of the Enlightenment: Vico, Hamann, Herder*, ed. H. Hardy (London, 2000), 128, 130–32, and R. G. Alberoni, *Gli Esploratori del Tempo: Le Concezioni della Storia da Vico a Popper* (Milan, 1993), 31. Popper discusses this aspect of Kant's thought in K. Popper, "Immanuel Kant: The Philosopher of the Enlightenment," in *In Search of a Better World: Lectures and Essays*

bold aim to develop the idea for the natural sciences.<sup>36</sup> His hope was to debunk the pretension that scientists may be pure observers and reporters of truth.<sup>37</sup> He described the two opposing views in colorful but useful language: "the bucket and the searchlight."<sup>38</sup> The older view is the bucket view, because it envisages a researcher blindly accepting information into a bucket and then examining the information for patterns.<sup>39</sup> Popper's own view is the searchlight view, because it envisages the researcher selectively illuminating the particular evidence which, on his current hypothesis, needs examination.<sup>40</sup>

The "searchlight" is Popper's way of indicating to the reader that observation is possible only with the help of a hypothesis. But what exactly a hypothesis includes, and how the observer uses it to explain events, is part of a larger scheme: Popper's description of *scientific and historical explanation*.<sup>41</sup> What Popper

from *Thirty Years* (London, 1994), 131–32. It is a very common view today: see Skinner's introduction in Q. Skinner (ed.), *The Return of Grand Theory in the Human Sciences* (Cambridge, 1985), especially 10–12.

<sup>36</sup> He regarded himself as building on Kant, to whom he attributed the first assaults on induction from observation in the natural sciences: "Kant saw more clearly than anyone before or since how absurd it was to assume that Newton's theory could be derived from observations." Popper, "On the Status of Science" (note 33), 185. Newton: "This rule we must follow, that the argument of induction may not be evaded by hypotheses." I. Newton, *Mathematical Principles*, trans. A. Mott, rev. F. Cajori (Berkeley, 1962), 2:400.

<sup>37</sup> See J. Agassi, "Towards Honest Public Relations of Science," in S. Amsterdamski (ed.), *The Significance of Popper's Thought* (Amsterdam, 1996), 39–57.

<sup>38</sup> Popper, "The Bucket and the Searchlight" (note 31), 341–47; Popper, *Open Society* (note 24), 2:260–61.

<sup>39</sup> To insist that the observation and recording of facts be without recourse to *a priori* guesses of any kind is what Hempel (see below note 41) calls the "narrow inductivist conception of scientific enquiry." C. G. Hempel, *Philosophy of Natural Science* (Englewood Cliffs, 1966), 11. Popper calls it "observationism." Popper, "Science" (note 33), 84. It is illustrated in a recent article which purports to advise lawyers on how to do empirical research: L. Epstein and G. King, "Exchange: Empirical Research and the Goals of Legal Scholarship," 69 *U. Chi. L. Rev.* 1, 45 (2002): "[T]he author of the research is entirely irrelevant . . . . [S]entences that begin 'I think' or 'I believe' are beside the point."

<sup>40</sup> In some works Popper associated the idea of active observation with a biological desire of humans to solve problems by choosing and sometimes altering their surroundings. See, e.g., K. Popper, "Knowledge and the Shaping of Reality," in *In Search of a Better World* (London, 1992), 12–17.

<sup>41</sup> Whether the method is entirely Popper's own, or owes something to C. G. Hempel (1905–1997, professor of philosophy and author of *The Philosophy of Natural Science*, 1966), is a matter of debate. Compare Popper, *Poverty of Historicism* (note 24), 144–45 n.1, and idem, *Open*

describes is a model for explaining events without falling into the error of induction, and at the same time preserving conventional scientific practice.<sup>42</sup> It is a model that he applies equally to the activity of social scientists (including historians<sup>43</sup>) and natural scientists.<sup>44</sup> It is easily illustrated with an example from ordinary life.

A person sees a child falling ill in a candy shop, and concludes that the child became ill by eating too much candy. How did he come to this conclusion? The answer is that he made a causal link between the event he was trying to explain (the illness) and certain facts that he knew or supposed (the child had been eating candy). The crucial step in this procedure is in the way he makes this causal link; that a child has eaten candy, after all, does not spontaneously explain why the child became ill. This step is achieved, according to Popper, with the help of *universal laws*. Here, the observer has used several such laws, e.g., "too much candy makes people ill," and "children eat too much candy." With the help of universal laws like this — and indeed *only* with their help — the observer is able link the event to the facts and thereby deduce the cause of the child's illness. The process of deduction therefore looks like this:

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*Society* (note 24), 2:364, with A. Donagan, "Historical Explanation: The Popper-Hempel Theory Reconsidered," 4 *History and Theory* 3, 5–7 (1964), and K. Minogue, "Does Popper Explain Historical Explanation?," in A. O'Hear (ed.), *Karl Popper: Philosophy and Problems* [Royal Institute of Philosophy Supplement 39] (Cambridge, 1995), 229.

<sup>42</sup> That is, suggesting a law and testing its predictive power: Popper, *Poverty of Historicism* (note 24), 130–31.

<sup>43</sup> Popper treats historians (as distinct from other social scientists) in: *Poverty of Historicism* (note 24), 130–47; "The Bucket and the Searchlight" (note 31), 354–55; *Open Society* (note 24), 2:259–80; "On the Sources of Knowledge and Ignorance," in *Conjectures and Refutations*, 5th ed. (London, 1989), 23–24, 27; "Pluralist Approach to History" (note 33), 138–50; Letter from Karl Popper to the Editor, *New Scientist*, August 21, 1980, at 611; "Historical Explanation: An Interview with Sir Karl Popper," *University of Denver Magazine*, June 1966, at 4, 4–7.

<sup>44</sup> For what follows, see Popper, *Open Society* (note 24), 2:262–63; idem, "The Aim of Science," in *Objective Knowledge: An Evolutionary Approach*, rev. ed. (Oxford, 1979), 191–93, and the authorities cited above, note 43.

<i>Explicans</i>	{	Universal Law(s) (too much candy makes people ill, children eat too much candy)
		Conditions, both <ol style="list-style-type: none"> <li>1. known (the child was in the candy shop)</li> <li>2. supposed (the child ate candy)</li> </ol>
<i>Explicandum</i>		Event (the child fell ill)

The lesson of this syllogism is that, in Popper's view, the observer in the candy shop has played an important part in his own conclusion. Before entering the shop he had certain universal laws in the back of his mind. He then combined those universal laws with certain facts, both known and supposed, to create a hypothesis about what had happened.

Popper offers this as a model of causal explanation for all sciences, and as already mentioned, Popper expressly includes historians among the class of scientists. The main difference between the (conventional) scientist and the historian, he says, is that they are interested in different things: the scientist gives more of his attention to the universal laws, while the historian pays little if any attention to the laws, giving his attention instead to the conditions. Thus, the scientist tests his laws in the hope of discovering their accuracy, while the historian looks for the conditions which will explain why an event took place. The method, however, is the same.

What are these laws that the historian uses but does not pay attention to? Some of them are indeed physical laws; if a historian wants to explain the failure of Napoleon's Moscow campaign, he can make a causal link between the conditions and the event only with the help of certain meteorological laws, none of which he needs to mention. But the laws that Popper has principally in mind for historians are more human: they are the trivial, obvious, *psychological* laws which the historian uses without thinking.<sup>45</sup> They are essentially a body of routine expectations about human

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<sup>45</sup> See Popper, *Open Society* (note 24), 2:264–65; idem, "The Bucket and the Searchlight" (note 31), 354; idem, *Poverty of Historicism* (note 24), 145.

behaviour accepted by the historian and his audience, but passed over in silence; for this reason I prefer to call them "expectation/laws." Here is an example: a historian wishes to explain "why the Prime Minister of the United Kingdom resigned in 1990." In looking for the *conditions* which explain this *event*, he would almost certainly select this condition: "the prospect of failure on the forthcoming ballot."<sup>46</sup> This condition, however, would be a "cause" of the Prime Minister's resignation (and indeed would come to the historian's notice) only because the historian subscribed to a relatively trivial *expectation/law* of human behaviour, such as "people with no prospect of winning give up." This "law" is therefore logically indispensable for making the causal connection between the condition and the event, but it is not very interesting, and would almost certainly go unremarked. It serves the same purpose, and is as equally uninteresting, as "children eat too much candy" in the example I gave above.

What one notices right away, however, is that these expectation/laws are not really "laws." For example, the law I have just given, on people giving up, is obviously false: some people do not give up even when there is no hope of winning.<sup>47</sup> The problem is that the kinds of human laws Popper puts forward do not logically explain ("cover") the historical events they purport to explain,<sup>48</sup> and this seems to weaken substantially the argument that historical explanation is causal in the same way that (conventional)

<sup>46</sup> See M. Thatcher, *The Downing Street Years* (London, 1993), 855: "I had lost the Cabinet's support. I could not even muster a credible campaign team. It was the end."

<sup>47</sup> See, e.g., R. H. Bork, *The Tempting of America* (New York, 1990), 311–12. "Defeat was certain," says Bork of the vote on his confirmation to the Supreme Court, *id.* at 311, but a "simple emotional reaction" told him "it was better to fight than to run." *Id.* at 312.

<sup>48</sup> The problem, in brief, is that if a law is successfully to explain why certain conditions produced an event, then that law ought to be true, and it is very difficult to formulate any law of human action that is true in all its applications. Popper gives various examples of physical, economic, and sociological laws, see Popper, *Open Society* (note 24), 2:264–65; *idem*, *Poverty of Historicism* (note 24), 61–63, 145. Popper admits: "Nothing is here assumed about the strength of the available evidence in favour of these hypotheses whose formulations certainly leave much room for improvement." *Id.* at 63. Donagan shows that none of these examples are laws, such as would support Popper's view of causal explanation in history. Donagan (note 41), 14–17. (But see the quote in note 49 below.) Other critical literature is cited in G. Stokes, *Popper: Philosophy, Politics and Scientific Method* (Cambridge, 1998), 82, and C. Simkin, *Popper's Views on Natural and Social Science* (Leiden, 1993), 126.

scientific explanation is. This is a problem that Popper acknowledged but dismissed as unimportant,<sup>49</sup> for the following reasons.

Popper wanted historical explanation to be as objective and "testable" as possible, even while recognizing that permanent laws of human behavior, or laws of history, were unachievable.<sup>50</sup> The fact that such laws are unachievable does not prevent a historian from silently assuming that specific individuals acted in ordinary and predictable ways, and on that assumption explaining the causes of some specific event. There is no "law" that humans act in ordinary and predictable ways, but it is nevertheless sometimes useful to assume they do so, because it makes it possible to compare one historical explanation with another.<sup>51</sup> If there is broad agreement about the "laws" — how the historical players are expected to have acted — then one historian can make a *logi-*

<sup>49</sup> See "Interview with Karl Popper" (note 43), 5 (emphasis added):

"[M]y theory of historical explanation is [far removed] from what certain people have been discussing — the completely uninteresting, *though logically valid*, claims about universal laws. Really, I found the discussions which centered around my remarks on the question of the background of universal law so bad that I did not even answer my critics."

This rare statement, from an interview in a college magazine (!), is one of the few places where Popper makes clear his attitude to universal laws in historical explanation.

<sup>50</sup> Readers of Popper will know that in his political works he devoted much scholarly energy to banishing, so far as possible, generalizing statements about history, statements which ostensibly allow one to predict future events on the basis of rhythms, patterns, trends, and laws, but which, in Popper's view, are capable of doing no such thing. See Popper, *Poverty of Historicism* (note 24), 3; Popper, *Open Society* (note 24), 1:7–10; Skagestad (note 4), 17–19. This appears to cause some conflict with his claim that historians seek causal explanations, a conflict which he attempts to resolve by (1) a recommendation that historians use "situational analysis," discussed below, and (2) a claim that human behavior cannot be explained en masse, but only person by person, the so-called "methodological individualism." On the latter, see Popper, *Poverty of Historicism* (note 24), 136.

<sup>51</sup> Popper suggests what he calls the "zero method" or "rationality principle," where the historian assumes for the sake of argument that the human actors are behaving rationally and in full possession of all relevant information. Popper, *Poverty of Historicism* (note 24), 141–42; idem, "Models, Instruments, and Truth," in M. A. Notturmo (ed.), *The Myth of the Framework* (London, 1994), 169–70. The principle is false, but allows deviations to be observed: N. Koertge, "Popper's Metaphysical Research Program for the Human Sciences," 18 *Inquiry* 437, 440 (1975); A. O'Hear, *Karl Popper* (London, 1980), 163–64.

ally better case than another.<sup>52</sup> In the example I gave above, a historian might point out that the Prime Minister resigned, not only with an unfavorable ballot in prospect, but did so immediately after her colleagues withdrew their support. By bringing in this additional condition, the historian has explained more of the event: not only *why* the Prime Minister resigned, but *when*. In this respect it is a logically better explanation, and it is an acceptable explanation because no historian is likely to dispute, or even pay much attention to, the uninteresting psychological "laws" that link the conditions to the event.<sup>53</sup>

Popper calls this "situational logic" or "situational analysis." It follows the same deductive model as the natural and social sciences, the principal difference being that the historian's laws do not predict anything about the future.<sup>54</sup> They serve only to help explain the causes of a specific, past event, by showing how that event logically follows from certain conditions.<sup>55</sup> There is an undeniably subjective component to this exercise: the historian selects the event he wants to explain, and has some freedom also in selecting the relevant laws. But this does not prevent a group of historians with similar interests discussing whose conditions make more logical sense, and accordingly who has the best explanation.<sup>56</sup>

<sup>52</sup> See K. Popper, "The Logic of the Social Sciences," in *In Search of a Better World* (London, 1992), 79–80; idem, *Poverty of Historicism* (note 24), 149–51; idem, *Open Society* (note 24), 2:97, 265–68; idem, "On the Theory of the Objective Mind," in *Objective Knowledge: An Evolutionary Approach*, rev. ed. (Oxford, 1979), 178–80; idem, "Models" (note 51), 165–68; idem, "Pluralist Approach" (note 33), 146–50.

<sup>53</sup> In asserting these laws are uninteresting, one wonders if Popper is simply expressing his hope rather than his opinion. See A. Boyer, *Introduction à la Lecture de Karl Popper* (Paris, 1994), 228: "Popper insiste sur leur caractère le plus souvent *trivial*. . . . Peut-être Popper est-il sur ce point trop sensible au modèle de l'histoire politique dont il conteste par ailleurs le privilège." Minogue is bothered by the fact that, according to Popper, some events are explained causally, while others are unique ("brute facts"), an incoherence that suggests to Minogue that the scientific model is the wrong one for history. See Minogue (note 41), 230–32.

<sup>54</sup> Unlike the other sciences (including the social sciences), history is not, to Popper, a "theoretical" science. Popper, "The Bucket and the Searchlight" (note 31), 354–55; idem, *Poverty of Historicism* (note 24), 143–47. In the *Open Society*, the "theoretical" sciences are also the "generalizing" sciences, a term that reveals Popper's purposes somewhat more clearly. See Popper, *Open Society* (note 24), 2:263–64.

<sup>55</sup> Popper, *Poverty of Historicism* (note 24), 144.

<sup>56</sup> Popper's biographer notes: "No historical school regards Popper's situational logic as having inspired their work, but he elucidated wonderfully, if all too briefly, the premises underlying widely used practices in history and social science." Hacothen (note 30), 494.

## V. Summary, and two examples from Roman law

Popper's views on historical explanation can be summed up very briefly. He says that historians who believe their task is first to gather evidence, and then to examine the evidence for patterns, misunderstand not only historical explanation but scientific explanation generally. *They are imitating what they wrongly believe to be the method of science.*<sup>57</sup> An "evidence first" approach is an illusion: evidence that ostensibly explains some event is invisible unless illuminated by some hypothesis. The hypothesis may include some facts, but its main component is some sort of expectation/law which shows how the evidence explains the event. "Hypothesis first" is therefore the reality of historical explanation, as of scientific explanation generally.<sup>58</sup> I suggest below that Daube practiced this very method: he treated texts as *events* to be explained, as it were, causally; he brought to every text a body of *expectation/laws* about how jurists and others think; and with the help of evidence (and indeed sometimes without its help) he sought to explain as closely and accurately as possible the condition of the text.

Before giving examples from Daube's own work, I give two examples from the work of other authors, to illustrate how these views work with legal texts. The simplest example (for reasons which will become clear below) is an argument which uses two texts by the same author. Watson quotes Paul's famous definition of *furtum* in *D.47.2.1.3* (39 *ad edictum*):<sup>59</sup>

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<sup>57</sup> Popper, "On the Theory of the Objective Mind" (note 52), 186:

I . . . accuse at least some professional historians of "scientism": of trying to copy the method of natural science, *not as it actually is*, but as it is wrongly alleged to be. This alleged but non-existent method is that of collecting observations and then "drawing conclusions" from them. It is slavishly aped by some historians who believe that they can collect documentary evidence which, corresponding to the observations of natural science, forms the "empirical basis" for their conclusions.

Similarly: Popper, "Pluralist Approach" (note 33), 140.

<sup>58</sup> Among the problems in Popper's theory of historical explanation is the suggestion that causal explanation is somehow the beginning and end of the historian's job. Donagan defends Popper in this respect, saying that Popper intended to do nothing more than discuss causal explanation. Minogue has the opposite view: "Popper has generalized scientific method to a level at which it can cover any kind of rational attitude at all. He has not incorporated the element of meaningfulness which marks off the human world." Minogue (note 41), 227.

<sup>59</sup> A. Watson, "The Definition of *Furtum* and the Trichotomy," 28 *T. v. R.* 197, 197 (1960) [= A. Watson, *Studies in Roman Private Law* (London, 1991), 269].

Furtum est contrectatio rei fraudulosa lucri faciendi gratia vel ipsius rei vel etiam usus eius possessionisve, quod lege naturali prohibitum est admitttere.

Theft is the fraudulent interference with a thing for the sake of gain, whether in respect of the thing itself or of the use or possession of it. It is something natural law forbids.

Watson doubts "this is to any great extent the work of Paul."<sup>60</sup> He offers several arguments for the point, but one particular argument is based on the following text from Paul's *Sentences*, a later work:<sup>61</sup>

Fur est qui dolo malo rem alienam contrectat.

A thief is one who maliciously interferes with another's property.

If we accept that Paul is the author of the *Sentences*, and that the *Sentences* were written after the commentaries on the edict, then we have difficulty explaining why Paul has retreated from a good definition to an inferior one. Watson's conclusion: that the text in the commentaries on the edict are, to some degree, not Paul's (a matter Watson goes on to discuss in detail).<sup>62</sup>

The hypothesis-first view is the view that the text from Paul's *Sentences* does indeed support Watson's conclusion, but that it does not somehow spontaneously make its meaning known, rather that it has been selected by Watson on the basis of a certain hypothesis to which Watson subscribes. The hypothesis contains two assumed facts (that Paul is the author of the *Sentences*, and that the commentaries were written first), but its main component is a *tacit* expectation/law. In this example, as in most examples, the expectation/law cannot be stated free of all guesswork, but it is perhaps something like this:

A jurist is consistent in his views.

Unless a jurist is consistent in his views, the text from Paul's *Sentences* signifies nothing about the authenticity of the other definition.<sup>63</sup> This expectation/law is not, of course, a permanent

<sup>60</sup> *Id.*

<sup>61</sup> Paul's *Sentences* 2.31 (E. Seckel and B. Kuebler (edd.), *Iurisprudentiae Anteiustinianae Reliquias*, 6th ed. (Leipzig, 1911), 2:64).

<sup>62</sup> Watson (note 59), 197–98.

<sup>63</sup> To complete the explanation, the law must be able to be universalized, because otherwise it is only an *ad hoc* statement. Popper, "The Aim of Science" (note 44), 192–93. But in this example, many different

law of behaviour. It expresses only the expectation Watson brings to his problem, an expectation that draws his attention to the text from Paul's *Sentences* and gives the text its significance. Without the expectation/law, the text does not "mean" anything. This is how Popper would describe Watson's argument.<sup>64</sup>

In the study of Roman legal texts, hypotheses in Popper's sense are therefore most evident, and least remarked, when a person follows some obvious thread. A person who needs guidance on the meaning of a text may, for example, look for similar texts: texts from the same era, by the same writer, using the same expression, written in the same hand, found in the same manuscript, etc. He does not, however, look for texts written at the same altitude, or in the same color of ink. Why not? Because there is no expectation/law that says texts written, e.g., at the same altitude have anything to do with one another. A person is interested only in texts which explain the text under review, and what he perhaps takes without reflection to be "similar texts" are, Popper would say, particular texts which some particular hypothesis tells him will be helpful. The expectation/laws within these hypotheses are not, of course, permanent and invariable: they are provisional and are often altered or abandoned as evidence is gathered. An expectation/law that seems too obvious even to mention may turn out to be the argument's main vulnerability.<sup>65</sup> But without some expectation, a person will stand before a mass of evidence without a clue about what to look at.

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"universalizable" statements are possible, e.g., "a person is consistent in his views," and "Paul is consistent in his views."

<sup>64</sup> The "hypothesis first" view is epistemological, not logical, and therefore is not a view that "all statements are analytic" or "all knowledge is *a priori*." A hypothesis, on this view, is not a source of knowledge and does not need to be accepted as true by the observer. Thus, to say that Watson has selected evidence based on certain expectations is not to say that he is reasoning *a priori* from these expectations. Popper does seem to believe, however, that the mechanism for seeking knowledge this way is inborn, in a sense "psychologically *a priori*." See K. R. Popper, "Science: Conjectures and Refutations," in *Conjectures and Refutations*, 5th ed. (London, 1989), 47–48; idem, "Conjectural Knowledge" (note 26), 5 n.10; and especially Popper, "Two Faces of Common Sense" (note 28), 92: "The laws of nature *are* our invention, they are animal-made and man-made, genetically *a priori* though not *a priori* valid."

<sup>65</sup> Philological guidelines for interpolation provide easy examples of this vulnerability. Assumptions about the Latin of the second and third centuries are based on the obvious expectation/law that "jurists of a certain time have an affinity of style" but, as Buckland points out, are called into question by the fact that much of this juristic work was the work of provincials. W. W. Buckland, "Interpolations in the Digest," 33 *Yale L. J.* 343, 344 (1924).

The statement "a jurist is consistent in his views" is clearly the sort of trivial psychological law Popper had in mind in setting out his theory of historical explanation, and yet in this context the psychological law is anything but trivial. A critic points out one problem in the word trivial:<sup>66</sup>

[W]hile what Popper calls "the psychological part" of such explanations is very often "trivial" in the sense of "obvious," it is not "trivial" in the sense of "unimportant." The information that a man in traffic seeks to avoid injury and not to commit suicide is indispensable, even if it is often obvious, in explaining the way he moves.

The problem in fact goes deeper than this. An expectation like "a jurist is consistent in his views" is indeed an obvious one. But a romanist might just as soon rely on an unobvious expectation, if it helps to explain the text. It is not an unimportant point: as I discuss below, Daube often brings quite unobvious expectations to a text. Here is an example from Honoré.

The *actio de pauperie* was not available if the animal had been provoked by a person. Ulpian says: *Sed et si instigatu alterius fera damnum dederit, cessabit haec actio.*<sup>67</sup> But the word *fera* is out of place: in no event is there *pauperies* if the animal is *fera*, that is, an animal wild by nature.<sup>68</sup> Why is *fera* in this text? Part of the answer is not difficult: Honoré suggests that Ulpian probably did not give *dederit* a subject at all, leaving it to his readers to understand *quadrupes*.<sup>69</sup> The person who edited this text missed the subject and decided to supply one. But why *fera* specifically? That the editor, for example, "did not fully understand *pauperies*" might explain a slip, but why this very slip?

Explaining *fera* is difficult, not (Popper would say) because there is anything intrinsically difficult about the text, but because the presence of *fera* is mostly invulnerable to our usual arsenal of hypotheses. Honoré solves the problem by appealing to a very particular expectation/law. Like Watson's it is (colloquially speaking) a "psychological" expectation, and like Watson's it is one that a reader understands intuitively. But it is not one that a reader understands *tacitly*, so Honoré spells it out: "When some-

<sup>66</sup> Donagan (note 41), 18.

<sup>67</sup> *D.9.1.1.6* (Ulpian 18 *ed.*).

<sup>68</sup> *D.9.1.1.10* (Ulpian 18 *ed.*). This is presumably why the translator of this text in the Watson *Digest* gives "animal" for *fera*. See Watson (ed.), *Digest of Justinian*, 1:276 (at *D.9.1.1.6*).

<sup>69</sup> A. M. Honoré, "Liability for Animals: Ulpian and the Compilers," in J. A. Ankum, et al. (edd.), *Satura Roberto Feenstra* (Fribourg, 1985), 247.

one edits a text in a language which he can read but not compose in without strain, his best course is to copy words and phrases from the original."<sup>70</sup> Honoré's evidence is *D.9.1.1.7*, where the editor has read *fera*.<sup>71</sup> This text falls just after the text he is attempting to explain, and Honoré says the editor returned to the prior text and "copied" to that text the *fera* he believed had been accidentally omitted.

Honoré's expectation/law is clearly less obvious than Watson's, but on Popper's view of historical explanation, both writers are engaged in exactly the same exercise: finding evidence with the help of facts and some generalized expectation/law.

I now come back to our main text.

#### VI. Daube's treatment of *D.18.1.37*

To recall: both Beseler and Daube sought to explain the interpolated condition of *D.18.1.37*. Beseler did so by induction from examples, restoring the text by removing a clause with a plainly non-classical *ideoque*, and in the process removing other text associated with *ideoque*. Daube objected to Beseler's restoration because Beseler had assumed, for no apparent reason, that the compilers had added an entirely new fact (that when the parties agreed on the sale of the land for as much as the seller's testator had purchased it, they did so in ignorance of the fact that the property had been given to the testator). Daube's own solution offered no examples of what he believed had been done to the text; he relied instead on the explanation that the compilers were enthusiastic about Justinian's solution to a classical dispute about prices fixed by a third person, and that they clumsily applied it to this text.

The main point is that the hypothesis-first view draws no methodological distinction between Beseler's and Daube's explanation of *D.18.1.37*. Beseler's explanation, on this view, is not the more objective because he has examples, nor is Daube's the less objective because he has no examples. Beseler's explanation may be better than Daube's, but it is not more "scientific." While an inductivist might believe that Beseler's *ideoque*-texts leapt out of the *VIR* without his help, the hypothesis-first view says that he in fact sought them out with the help of certain obvious psychological expectation/laws, such as "a writer favors certain words and

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<sup>70</sup> *Id.*

<sup>71</sup> *Id.* The word *fera*, according to Honoré, is no more appropriate to *D.9.1.1.7* than to the prior text, and accordingly the evidence matches the hypothesis only if the editor actually read *fera* in his source material for *D.9.1.1.7*, as Honoré argues. See *id.* at 246–47.

expressions." Without this expectation, it would not even occur to him to look up *ideoque*, or any other word. Of course Daube's evidence (C.4.38.15), on this view, is no less pre-selected or "theory-laden" than Beseler's evidence. The expectation/laws that precede Daube's selection of C.4.38.15 are simply less obvious, and less often used, than Beseler's. Those that precede Daube's selection of C.4.38.15 are perhaps "the compilers were willing to alter texts out of sheer enthusiasm," or "the compilers were eager to advertise Justinian's reforms." Daube's eyes would have passed over C.4.38.15 without stopping unless an expectation/law, such as one of these, told them to stop. The hypothesis-first view is therefore not a rebuke to Beseler's method of argument. It challenges only the idea that Beseler's evidence speaks without the intervention of Beseler; or that Beseler has given "reasons" for accepting his view, while Daube has not; or that Beseler's view is supported by evidence, while Daube has only given his opinion.<sup>72</sup>

The real difference between Daube's and Beseler's arguments, on the hypothesis-first view, is in the nature of the respective expectation/laws. Beseler's expectation/law is the sort of workaday assumption that philological arguments often rely on. It is in the category of these: a writer has a limited vocabulary; he has a style; he conveys what he intends to convey; he is conscious of misapprehensions; a word shares meaning with etymologically related words; two inflections of the same word are, nevertheless, the same word. These kinds of generalizations make up an essential stock of assumptions. Their main shortcoming (Popper would say) is that, being so familiar, they may have a person reaching for "relevant" or "similar" texts without being aware that he is assuming anything in doing so. Daube's treatment of D.18.1.37 is different in this respect. His expectation/law, like Beseler's, makes a causal connection between the uninterpolated

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<sup>72</sup> Popper would make the even stronger point, that those who favour Beseler because he has given examples in support of his view are straying into subjectivity. See Popper, "On the Sources of Knowledge and Ignorance" (note 43), 25–27. They are trying to support their belief (Popper would say) by inquiring after the truth of a source. But inquiring after the truth of a source always requires a further inquiry into the sources of that source, an endlessly regressive exercise. His point is that one ought to be inquiring about the validity, not the pedigree, of one's beliefs. See also Miller (note 25), 37 ("[I]n science as in everyday life, it is whether our hypotheses are true or false that matters, not whether they are empirically supported . . ."). Popper acknowledges that a historian will always be interested in questions of pedigree, but argues that validity is nevertheless the ultimate aim. Popper, "On the Sources of Knowledge and Ignorance" (note 43), 27.

text and the interpolated text, but it is less intuitive and Daube must therefore put it in full view.

Does the hypothesis-first view offer any reasons for preferring either Beseler's or Daube's explanation of *D.18.1.37*? The answer is that it does, but only if the reader is willing to go along with the hypotheses. This means the first step for the critic is a subjective one: he agrees for the sake of argument to adopt the same hypotheses as Beseler and Daube. If he does not, none of their evidence will be even colorably relevant. But after he accepts the hypotheses for the sake of argument, a genuinely objective discussion can follow. It is then possible to discuss whether, *as a matter of logic*, the facts, the assumed facts, and the expectation/laws, adequately explain the condition of the text. This is Popper's "situational analysis," and it is a model that puts Daube's treatment of *D.18.1.37* in its best light. Daube criticizes Beseler for not fully and adequately explaining the condition of the text. That is the gist of his statement: "[O]ne can think of no plausible motive which might have induced anyone to turn Beseler's text into the present."<sup>73</sup> What he means is that his own hypothesis, right or wrong, does the better job of explaining why *D.18.1.37* looks as it does.<sup>74</sup> His criticism relies on the kind of causal explanation discussed above: initial conditions and laws, if well conceived, will explain an event more precisely, because logically they are better at "covering" the event they purport to explain. In this respect, Daube suggests, Beseler's argument falls short: his hypothesis does not cover the event. In particular, it does not explain why (on Beseler's view) the compilers added a new fact, that both parties embarked on the sale in ignorance of the true state of affairs. In short, if a reader accepts both writers' hypotheses at the outset, he may conclude that Daube's explanation is made out better than Beseler's.

### VII. Does Daube subscribe to Popper's views?

Many of Popper's views cannot be attributed to Daube. For example, Daube does not avoid induction,<sup>75</sup> and there is no reason to think that Daube equated his own method of research to a

<sup>73</sup> Daube, "Certainty of Price" (note 2), 18.

<sup>74</sup> Popper, in explaining why all historical interpretations are not of equal merit, gives the example: "[T]here are some [interpretations] that are unable to connect a number of facts which another interpretation can connect, and in so far 'explain'." Popper, *Open Society* (note 24), 2:266.

<sup>75</sup> Daube relies on examples to explain the presence of *tunc* in the apodosis of a condition, in D. Daube, "Si . . . tunc in D.19.2.22 pr.," 5 *RIDA* (3rd series) 427, 427-29 (1958) [= *Collected Studies*, 2:723-25].

scientist's. Where Daube and Popper do share views is on the matter of historical explanation. Daube often interprets texts "causally," within a framework which Popper would describe as situational analysis, as in the example just given. Daube puts together an account of facts and hypotheses, and asks us to judge his account, not principally for its support in the evidence, but for its power to explain the text. That this is Daube's model, and that he is not shy of putting his answers before the evidence, are apparent from the fact that his inquiry starts with the question: how did the text come to its present condition? The question itself is not part of any argument; it is instead a signal to the reader that Daube has looked at a text and noticed something which is not fortuitous. The question, moreover, cannot be answered by citing evidence. It can only be answered by a kind of story, giving facts and hypotheses which go to show how something which is not fortuitous found its way into the text. The argument is framed as a condition: if you accept these facts and hypotheses, then the text will look just as we have it. Supporting evidence is not strictly necessary.<sup>76</sup> What the argument cannot do without is a hypothesis, that is, facts that are assumed, and expectation/laws by which the facts explain the text under review.

Hypotheses of this kind are Daube's strength. He is very good at producing unexpected reasons to explain a text, and the reasons he produces do tend to explain the text closely. *Appreciating Daube's work means appreciating how difficult it is to do this well.* It is difficult because hypotheses (on the views discussed here) are not derived from observation, deduced, or produced by any deliberate method or mental process at all.<sup>77</sup> There is no way to determine in advance one's expectations of a text. The making of a hypothesis is an act of imagination: it may origi-

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<sup>76</sup> The text under review may itself be regarded as evidence for the hypothesis. In other words, Popper's causal explanation sometimes conflates the event being explained and the evidence explaining it. To some this will weigh against his explanation.

<sup>77</sup> "[M]y view of the matter, for what it is worth, is that there is no such thing as a logical method of having new ideas, or a logical reconstruction of this process." Popper, *Logic of Scientific Discovery* (note 23), 32. "[I]t is irrelevant from the point of view of science whether we have obtained our theories by jumping to unwarranted conclusions or merely by stumbling over them (that is, by 'intuition'), or else by some inductive procedure. The question, 'How did you first find your theory?' relates, as it were, to an entirely private matter . . ." Popper, *Poverty of Historicism* (note 24), 135. Popper's views and the views of his critics are summarized in Magee (note 30), 31–33.

nate in chance, error, or a flash of brilliance. Popper often cites<sup>78</sup> Albert Einstein's views on where theories come from, as in this letter he received from Einstein:<sup>79</sup>

Altogether I really do not at all like the now fashionable [*modische*] "positivistic" tendency of clinging to what is observable. . . . I think (like you, by the way) that theory cannot be fabricated out of the results of observation, but that it can only be invented.

The same view is expressed most clearly by Hempel,<sup>80</sup> the (possible) co-author of Popper's view of causal explanation:<sup>81</sup>

The transition from data to theory requires creative imagination. Scientific hypotheses and theories are not *derived* from observed facts, but *invented* in order to account for them. They constitute guesses at the connections that might obtain between the phenomena under study, at uniformities and patterns that might underlie their occurrence. "Happy guesses" of this kind require great ingenuity. . . .

It is a kind of ingenuity Daube has a great deal of. In the example just given, it is remarkable to have seen *D.18.1.37* as an example of the compilers' enthusiasm for Justinian's reforms. And this observation is a very typical one. Daube's hypotheses are often unique because he imagines that the actors are performing according to psychological laws other than the obvious ones. The laws are too remarkable to be passed over in silence, but not too remarkable to be believed. They are the kind of laws Daube expects the reader to understand intuitively, as in these three examples.

a) *D.12.5.5* (Julian 3 *ad Urseium Ferozem*). The text concerns a *condictio*, but which one?<sup>82</sup>

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<sup>78</sup> K. R. Popper, "Evolution and the Tree of Knowledge," in *Objective Knowledge: An Evolutionary Approach*, rev. ed. (Oxford, 1979), 257–58 & n.2; idem, *Logic of Scientific Discovery* (note 23), 32; idem, "Science" (note 33), 95–96 & n.6

<sup>79</sup> Letter from Albert Einstein to Karl Popper, 11 September 1935, reprinted in Popper, *Logic of Scientific Discovery* (note 23), 458 (translated by Popper). Einstein expresses the same opinion in his book of essays, A. Einstein, *The World as I See It*, trans. A. Harris (London, 1935), 125.

<sup>80</sup> See above note 41.

<sup>81</sup> Hempel (note 39), 15.

<sup>82</sup> See Daube, "Turpitude in Digest 12.5.5" (note 2), 33–36.

Si a servo meo pecuniam quis accepisset, ne furtum ab eo factum indicaret, sive indicasset sive non, repetitionem fore eius pecuniae Proculus respondit.

If someone received money from my slave to keep him from revealing a theft committed by the slave, Proculus gives the opinion that that money can be recovered, whether the receiver reveals or not.

Pflüger treats this as the case of a slave who steals from his master, and who passes on his master's property to another: the *condictio furtiva* is therefore, according to Pflüger, the action Proculus and Julian had in mind.<sup>83</sup> This solution is supported by what appears to be a very similar text, this one from Ulpian:<sup>84</sup>

Quod si a fugitivo meo acceperis ne eum indicares, condicere tibi hoc quasi furi possim . . . .

But if you receive something from my runaway slave not to betray him, I may bring a *condictio* against you for it, as if you were a thief.

Daube is unhappy with this solution for two reasons.<sup>85</sup> First, if Julian does have in mind the *condictio furtiva*, then the matter is not so complex that he would feel the need to cite Proculus. Second, the phrase *sive indicasset sive non* adds nothing of any interest if the slave's master is simply recovering his own stolen property. These are Daube's two "questions." He suggests that what Julian is not telling us is that the slave has stolen from a person other than his master. The agreement with the receiver is therefore a *datio ob rem*, one which, moreover, reflects badly on the receiver. This means that the action which Julian and Proculus have in mind is not the *condictio furtiva*, but the *condictio ob turpem causam*. And the receiver must restore the property notwithstanding *res secuta*, that is, even if he partly redeemed his character and kept his word, as Paul suggests:<sup>86</sup>

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<sup>83</sup> H. H. Pflüger, "Condictio ex iniusta causa," 32 ZSS (rom. Abt.) 168, 171 (1911).

<sup>84</sup> *D.12.5.4.4* (Ulpian 26 *ed.*).

<sup>85</sup> For the points below, see Daube, "Turpitude in Digest 12.5.5" (note 2), 33–35.

<sup>86</sup> *D.12.5.1.1–2* (Paul 10 *Sab.*). Daube does not actually cite this text specifically, but his reference to restoration "in spite of *res secuta*" means that he probably had it in mind: Daube, "Turpitude in Digest 12.5.5" (note 2), 35.

Ob rem igitur honestam datum ita repeti potest, si res, propter quam datum est, secuta non est. Quod si turpis causa accipientis fuerit, etiamsi res secuta sit, repeti potest.

So one may recover something given for an honest purpose if the purpose for which it was given does not ensue. But if it was received for an immoral reason, recovery is allowed even if the purpose ensued.

This explains, Daube says, why Proculus has added "sive indicasset sive non."

The novelty of Daube's explanation is clearer if the two explanations are expressed with the hypotheses foremost. Pflüger expects to find an explanation of Julian's text in Ulpian's treatment of the thieving slave in *D.12.5.4.4* because a certain hypothesis directs him there. The hypothesis includes an assumed fact, that the slave in Julian's text is passing on his master's property, and an expectation/law, something like:

On the same facts, jurists express the same opinions.

Pflüger's argument, from this point of view, is this: "If we assume that Julian and Ulpian are talking about the same facts, and that Julian's opinion is the same as Ulpian's, then the action is a *condictio furtiva*."<sup>87</sup> A very different hypothesis directs Daube to Paul's text on *res secuta*.<sup>88</sup> Daube's hypothesis, like Pflüger's, includes an assumed fact (that in Julian's text the slave is *not* passing on his master's property), but his expectation/laws (he uses two) are considerably more creative than Pflüger's:<sup>89</sup>

A jurist does not invoke another jurist for a trivial point.

A jurist does not insert a proviso for no reason.

Daube's argument, from this point of view, is this: "If we assume that the slave is not passing along his master's property, and that Julian has cited Proculus for a difficult point concerning *res secuta*, then the action is a *condictio ob turpem causam*."

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<sup>87</sup> This may seem like an unfair characterization of Pflüger's argument, but in fact the argument is no better in Pflüger's own words: "[K]ann über die Natur der *condictio* in den beiden Stellen eigentlich kein Zweifel sein. Natürlich die *condictio furtiva*." Pflüger (note 83), 171.

<sup>88</sup> See note 86 above.

<sup>89</sup> Daube does not state these rules; they are implicit in his questions. Daube asks (I am paraphrasing): "why does Julian invoke Proculus?" and "why does Proculus add 'sive indicasset sive non'?" Daube, "Turpitude in Digest 12.5.5" (note 2), 33–34.

This comparison between Pflüger and Daube does not, of course, show that Daube's argument is better than Pflüger's, only that his hypotheses are bolder. To compare the two on the merits, the reader must first agree to accept both writers' hypotheses. If he does so, he will see that Daube's argument explains more of the text than Pflüger's: why Julian has cited Proculus, why Julian has inserted the proviso.

b) *D.18.1.28 (Ulpian 41 ad Sabinum)*. The text concerns the disposal of a third person's property:

Rem alienam distrahere quem posse nulla dubitatio est: nam emptio est et venditio: sed res emptori auferri potest.

There is no doubt that a person is able to dispose of another's property, for there is indeed a sale, though it is possible to retrieve the thing from the purchaser.

Daube notices three oddities which he attributes to abbreviation by the compilers.<sup>90</sup> There is no reason to discuss the specifics except to say that, to Daube, these oddities are evidence that the text began as a text on the sale of a debtor's property by a pledge creditor, and that the compilers have used the text to make a general rule.<sup>91</sup> Daube's question concerns the compilers' reasons for making the general rule. That they desired general rules is taken for granted: but why did they do it in this instance? The rule they created made no change whatsoever from the classical law, and this is therefore different from the more familiar cases, where the compilers adapt an old rule to new circumstances, or generalize a rule that applies only in specific circumstances.<sup>92</sup> The explanation, Daube says, is that the compilers wanted a statement of the general rule, but were unable to find one: the classical jurists sometimes took a rule so much for granted that they never got around to stating it. The rule permitting the disposal of another's property is such a rule, and as evidence Daube gives several examples where the rule is implicit.<sup>93</sup>

The creation of a particular general rule by the compilers is not ordinarily treated as anything remarkable, and it is a sign of Daube's creativity that he is able to argue that this general rule is

<sup>90</sup> Daube, "Generalisations in D.18.1" (note 2), 186–92. The oddities he notes are (1) *distrahere quem posse*, which is clumsy; (2) *nam . . . venditio*, which adds nothing important; and (3) *sed . . . potest*, which is not sensitive enough to the time at which recovery is sought.

<sup>91</sup> See *Pal.*, 2:1167 (Ulpian 2874).

<sup>92</sup> Daube, "Generalisations" (note 2), 190–91.

<sup>93</sup> *Id.* at 186, 191. Daube takes up this idea more extensively in "Das Selbstverständliche in der Rechtsgeschichte" (note 2).

different from other general rules. He sees this as a special case, not because he has any evidence to that effect,<sup>94</sup> but because he brings certain expectations to his reading of the text. When he asks why the compilers took the trouble to generalize Ulpian's text instead of taking one ready-made, given that there was no change in the law,<sup>95</sup> he means that the fact that they did so is not an accident. Accordingly, the hypothesis with which he begins his argument comprises an assumed fact, that the classical jurists never got around to stating a general rule about the disposal of another's property, and an expectation/law, something like this:

The compilers would not exert themselves more than necessary.

The critic of course will point out that Daube has no evidence for his conclusion (i.e., the negative proposition that the classical jurists never stated a principle they took for granted), but for present purposes only the quality of the hypothesis is important.

c) Gaius, *Institutes* 1.5. Writing about the legal force of *constitutiones*, Gaius says: *nec umquam dubitatum est quin id legis vicem optineat*. Daube wonders why Gaius is so eager to put the matter beyond doubt.<sup>96</sup> Daube's conclusion is that there were indeed doubts about the force of *constitutiones*. The hypothesis he uses to reach this conclusion assumes one fact, that Gaius himself was in doubt about the force of *constitutiones*, and one expectation/law, that a person will exaggerate his confidence if, in fact, he is in doubt. Daube says: "Motivation of the type I ascribe to Gaius is perennial."<sup>97</sup> The argument relies entirely on the hypothesis; there is no supporting evidence.<sup>98</sup>

### VIII. The usefulness of putting hypotheses first

The general lesson of Daube's method I take to be this. There will often be a class of ready-to-hand evidence available to help a person understand some text. It is ready-to-hand, not because it

<sup>94</sup> Examples of texts where the rule is implicit (which Daube gives) are not evidence that the rule was never stated.

<sup>95</sup> Daube, "Generalisations" (note 2), 190–91.

<sup>96</sup> Daube, "Das Selbstverständliche" (note 2), 12–13.

<sup>97</sup> As in the translated text: Daube, "The Self-Understood" (note 2), 134.

<sup>98</sup> Or rather, the evidence he does cite, the Ghanan constitution, is not Roman evidence: Daube, "Das Selbstverständliche" (note 2), 13 n.48. Cf. A. M. Honoré, *Gaius* (Oxford, 1962), 118–21, who also takes Gaius' statement as untrue, but suggests that Gaius may be describing a point that was once disputed but no longer is.

is necessarily the best evidence, but because it has been selected by the researcher partly on the basis of certain common or tacit expectations, many concerning human behaviour. With enough imagination, one can read a text with different expectations, and in doing so perhaps bring a wider selection of evidence in tow. This wider evidence is not *a priori* inferior to the ready-to-hand evidence, only different. The hope is that different expectations will reveal evidence which explains the condition of the text more closely. And even if they reveal no evidence at all, different expectations alone may show in what direction a better answer lies.

This is the lesson, and certain objections aside<sup>99</sup> it is a useful one in several respects.

(1) It is useful to be reminded that evidence does not gain any probative value simply by being obvious. Less obvious evidence might explain something better, if one has the imagination to see it.<sup>100</sup>

(2) A method like Daube's makes discussions more fertile, even when it fails to prove its point. This is because (as Popper would put it) hypotheses have a value separate from their truth

<sup>99</sup> There are at least two serious objections that I do not address. Many would object that an argument that is not supported by evidence is not the equal of an argument that is. Cf. note 72 above. Also, many would object to the decisive role played by the expectation/law, which is neither proven true, nor even assumed to be true in most of its applications. Cf. note 101 below.

<sup>100</sup> Birks gives a good example of this. The second chapter of the *lex Aquilia* gave an action against an *adstipulator* who released the debt to the detriment of the *stipulator*, but the action fell out of use. See Gaius 3.216; D.9.2.27.4 (Ulpian 18 *ed.*). Why did it fall out of use? We have the "obvious evidence" in Gaius: he says that the action on mandate accomplishes nearly everything that an action on chapter two would accomplish. If we take our lead from Gaius we will look for an explanation in the rise of the action on mandate. See F. H. Lawson, *Negligence in the Civil Law* (Oxford, 1950), 4 n.5; J. A. C. Thomas, *Textbook of Roman Law* (Amsterdam, 1976), 334. But Gaius is not necessarily the best, the better, or even good, evidence on the subject. Birks looks for the answer in currency fluctuation, and in doing so exposes Gaius' text as potentially misleading. P. Birks, "Wrongful Loss by Co-Promisees," 22 *Index* 181, 181–88 (1994). The explanation may be, says Birks, not that chapter two fell out of use, but that it was, in the first place, a kind of *ad hoc* legislation, enacted to answer the problem of the devaluation of the *as*. The uncertainty of the value of the *as* provided the *adstipulator* an opportunity for deception that more stable times denied him. *Id.* at 184. Birks' solution is accomplished with a small but significant change of expectation: that those who wrote chapter two were eager to discourage, not a "wrong" *per se*, but a short-lived opportunity for abuse. This solution, correct or not, is invisible to one who unconsciously holds fast to certain familiar expectations, believing he has found the "best evidence" in texts directly on the *lex Aquilia*.

content.<sup>101</sup> One might, for example, reject Honoré's explanation of *D.9.1.1.6* discussed above,<sup>102</sup> but at the same time accept that his hypothesis may be true in some unknown number of other cases. It is plausible to believe that someone who cannot write a foreign language competently might resort to copying the words of others. The very fact that Honoré has made this hypothesis known is useful, because it can become part of a common fund of hypotheses and alert others to the possibility of reading texts from that point of view. Similarly a reader, unconvinced that the compilers altered our principal text out of a desire to advertise Justinian's reforms, will nevertheless go away with a new weapon to use on some other text. The better explanation may lie there, just as a difficult text may suddenly make sense if the reader considers the compilers' reluctance to exert themselves more than necessary, or a jurist's reluctance to cite other jurists for trivial points, or a jurist's tendency to exaggerate his opinion when he is in doubt, or any other plausible but unobvious explanation which Daube has proposed. Hypotheses like these are unlimited in number, and each one has the potential to explain a text, but only imagination can uncover them.

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<sup>101</sup> This is the notion of "explanatory power." Any hypothesis, Popper says, has a certain extent of empirical content, which is to say that whenever a person formulates a statement about the world, as a matter of sheer logic a certain number of events fall within that statement. See Popper, "Two Faces of Common Sense" (note 28), 81.

<sup>102</sup> See note 71 above.