

Two Empirical Approaches to the Study of Reasoning¹

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Abstract: David N. Perkins has studied everyday reasoning by an experimental-critical approach involving taped interviews during which subjects reflect on controversial issues and articulate their reasoning on both sides. The present author has studied scientific reasoning in natural language by an historical-textual approach involving the reconstruction and evaluation of the arguments in Galileo's *Two Chief World Systems*. They have, independently, reached the strikingly similar substantive conclusion that the most common flaw of informal reasoning is the failure to consider lines of argument supporting conclusions contrary to the one in fact reached. This article describes, compares, and contrasts their respective approaches, results, and theoretical frameworks.

1. Introduction

In a number of papers I have advocated a type of empirical approach to the study of reasoning which may be called the historical-textual or informal logic approach.² Here reasoning is conceived as a special type of thinking which consists of interrelating thoughts in such a way that some are dependent on and follow from others. To this abstract definition one may add something of an operational definition by saying that reasoning occurs paradigmatically in written or oral discourse which contains a high incidence of reasoning indicator terms such as therefore, thus, hence, consequently, since, because, and for. What this means is that, while all reasoning is thinking, not all thinking is reasoning, and hence the study of reasoning is only a part of the study of mental and

cognitive activities. I am not uninterested, of course, in the relationships between reasoning per se and thinking in general, but my own special focus is the former.

In this context the empirical is contrasted primarily to the apriorist approach, in regard to which I would give the example that, if and to the extent that we regard formal logic as a theory of reasoning, it would be a type of apriorist approach. On the other hand, I do not mean to contrast the empirical to the normative, and in fact the aim of the historical-textual approach is the formulation of normative and evaluative principles besides descriptive, analytical, and explanatory ones. Another proviso is that the empirical approach ought not to be regarded as empiricist, namely as pretending that it can study reasoning with a *tabula rasa*. I would have no difficulty admitting that various kinds of concepts are presupposed in any empirical investigation, though of course I would insist that the presupposed concepts are not the same ones which are at issue in the given empirical investigation, or at least the form or version of the concept or principle in question is not the same as what is presupposed.

Next, it is useful to say a few words about the variety of empirical approaches. We may distinguish at least three types of empirical orientations. One is the experimental approach, which consists of bringing human subjects (on a paid or volunteer basis) into a laboratory; explaining to them the questions to be answered, or problems to be solved, or tasks to be performed; arranging for them to respond usually by a yes or no, or with a multiple-choice selection, or in some other highly structured

fashion; recording such responses; analyzing such recorded data usually by statistical methods; and then drawing some more or less theoretical conclusion, either an inductive generalization extrapolated from the data or an explanatory hypothesis accounting for the data. What I have in mind here is the work of cognitive psychologists such as Johnson-Laird, Wason, Evans, Kahneman and Tversky, and Nisbett and Ross;³ I do not include Piaget simply because I want to emphasize that, as just explained, I am dealing with approaches to the study of reasoning and not to the study of thinking in general, and my impression is that Piaget's work deals with the more general topic, however suggestive it may be for reasoning as well. Another approach is the one analytical philosophers use when, on the basis of imagined examples, they reach generalizations about concepts such as existence, knowledge, belief, explanation, intentionality, justice, and morality; here I am thinking of the work of philosophers like Quine, Hintikka, Hempel, Scriven, Searle, Rawls, and Harman; this approach has been analyzed with great insight and originality by L. Jonathan Cohen⁴ and characterized as being inductive reasoning in which normative generalizations are based on particular intuitions. The third orientation is the historical-textual approach, which could be equated with informal logic⁵ and of which more presently. While sharing the general empirical orientation, I have criticized both the experimental-psychological approach and the inductive-intuitive approach for a number of reasons which cannot be repeated here but which include the argument that often both of these approaches exhibit inadequacies the overcoming of which requires moving in the direction of the historical-textual approach.⁶

With the background of these remarks, what I should like to do here is to focus on a subtype of the experimental-psychological approach which has been used with great skill and insight by David N. Perkins,⁷ and

which for reasons that will become apparent shortly I shall label the experimental-critical approach. What motivates me to discuss Perkins's work in detail is the surprising fact that some of his own substantive results are amazingly similar in content to conclusions I have reached through the study of very different material and following an historical-textual procedure rather than an experimental one. The similarity of these conclusions, and the fact that they have been arrived at independently of each other, will of course yield some mutual reinforcement, but it will also provide the occasion for exploring more deeply the methodological similarities between the two approaches. There are after all some obvious similarities between them, and perhaps they can be seen to be both special cases of some empirical approach yet to be characterized.

2. Examples of Reasoning Studied

Let us begin by describing what may be called the raw material that has been studied in the two cases. It will be immediately obvious that the subject matter of the reasoning studied could hardly have been more different. Perkins has studied arguments and reasoning which may be labeled everyday or informal, dealing with the following four issues which were relatively current in the United States at the time he conducted his experiment: whether or not a military draft in the United States would increase American influence in the world, whether or not the display of violence on television increases violence in real life, whether or not a deposit of five cents for bottles and cans of soft beverages like Coca Cola would reduce litter in streets and parks, and whether a stack of bricks created by a minimalist artist named Carl Andre is really art. As you can see these are also polemical issues.

On the other hand, I have studied arguments and reasoning about the motion of

the earth, its location in the universe, and the physical and chemical differences between the earth and the heavenly bodies; these are issues which were discussed at various phases of the Copernican Revolution, and which are recorded in Galileo Galilei's book entitled *Dialogue on the Two Chief World Systems, Ptolemaic and Copernican* (1632). In some obvious sense, therefore, these are scientific arguments, scientific in the sense of the history of science, and to be more exact prevalent during the formative period of modern science, if not in the sense of contemporary science.

2.1. *The Arguments in the Experimental-Critical Study*

It will be useful for our later analysis to give some examples. In regard to the five-cent deposit issue, the question was whether a law requiring a five-cent deposit on bottles and cans would reduce litter. The following argument conveys the idea of what we are dealing with:

The law wants people to return the bottles for the five cents, instead of littering them. But I don't think five cents is enough nowadays to get people to bother. But wait, it isn't just five cents at a blow, because people can accumulate cases of bottles or bags of cans in their basements and take them back all at once, so probably they would do that. Still, those probably aren't the bottles and cans that get littered anyway: it's the people out on picnics or kids hanging around the street and parks that litter bottles and cans, and they sure wouldn't bother to return them for a nickel. But someone else might—boy scout and girl scout troops and other community organizations very likely would collect the bottles and cans as a combined community service and fund-raising venture. I know they do that sort of thing. So litter would be reduced. [Perkins et al. (1983), p. 178]*

Consider now the question, "Would restoring the military draft significantly

increase America's ability to influence world events?" Here are some typical arguments:

Yes, because a draft would give the U.S. more manpower in the army. The U.S. would have a bigger stick to wave and foreign nations would be impressed.

Yes, because more manpower would put the U.S. in a better position to fight limited tactical wars. Since everyone is scared of world-wide nuclear war, small-scale wars are more likely and a military well-manned and ready to intervene should provide more influence.

No, because a draft would trigger widespread protests, as it did during the Vietnam war. This internal dissension would be seen as lack of unity and a sign of weakness by foreign observers.

No, because nowadays it's computers, missiles, and the people who design and maintain them that really count. If the U.S. needs anything in the army, it's more smart technical people. But a random draft won't net very many such people. [Perkins (1985), p. 22]

2.2. *The Arguments in the Historical-Textual Study*

Let us now get a glimpse of the Copernican and anti-Copernican arguments, found in the text of Galileo's *Two Chief World Systems*. One is the so-called a posteriori argument for the earth-heaven dichotomy. It reads quite simply: no heavenly changes have ever been observed; therefore, the heavenly region is unchangeable (Galilei 1953, pp. 46-48).

Galileo counters this argument in at least four ways. One is to point out that in his own time the premise is no longer true, in the light of, for example, the telescopic observations of sunspots and the naked-eye observation of novas. Here it is important to note that, though this criticism is in some obvious sense nonlogical, it is the one that takes Galileo the longest to articulate, since he has to argue every inch of the way through all sorts of controversial

issues in order to refute the premise (Galilei 1953, pp. 50-58).

Other Galilean criticisms are based on the contextual distinction of two meanings for the phrase "heavenly changes": a heavenly change can mean the generation or decay of a heavenly body as a whole, and it can mean a partial change within a heavenly body.

When interpreted holistically, the original argument amounts to the following: no one has ever observed any generation or decay of a heavenly body in the heavenly region; therefore, the heavenly region is unchangeable. It is then subject to the criticism that this way of reasoning would lead one to the following absurd argument: no one has ever observed any generation or decay of a terrestrial globe (i.e., the whole planet earth) in the terrestrial region; therefore, the terrestrial region is unchangeable (Galilei 1953, pp. 49-51).

When the argument is interpreted the other way, Galileo objects that it is still wrong for the following reason: no terrestrial changes would be noticeable to an observer on the moon before some particular very large terrestrial change had occurred, and yet terrestrial bodies are obviously changeable and would have been so even before that occurrence (Galilei 1953, pp. 49-50).

The fourth criticism Galileo makes is directed to the more plausible particularistic (second) version and amounts to the following argument: if there were changes in the heavenly bodies then most of them could not be observed from the earth, since the distances from the heavenly bodies to the earth are very great, and on earth changes can be observed only when they are relatively close to the observer; moreover, even if there were changes in the heavenly bodies large enough to be observable from the earth then they might not have been observed, since even large changes cannot be observed unless careful, systematic, exact, and continual observations are made, and no such observations have been

made, at least not by the argument's proponents (Galilei 1953, pp. 47-50).

The other Galilean example to be considered here mentions centrifugal force to show why the earth cannot rotate. It may be stated thus: if the earth were rotating, objects on its surface would be scattered away from it toward the heavens because rotation has the power of extruding objects lying on the surface of the rotating body; but objects on the earth's surface are not observed to be scattered toward the heavens; therefore the earth does not rotate (Galilei 1953, pp. 187-88).

Galileo counters in at least two ways. He objects that what follows at most from the principle of centrifugal force is not the scattering of objects on a rotating earth but rather either that (a) if the earth had always been rotating then there would not be any objects on its surface now, or that (b) if the earth were to start now rotating then objects already on its surface would be scattered; but the other premise as stated connects with (b) rather than (a); so the final conclusion that follows from the argument as stated is that the earth did not just start to rotate (Galilei 1953, pp. 188-90).

Continuing with the criticism, Galileo is well aware that one could reformulate the argument by restating the second premise to read: there obviously are now loose objects on the earth's surface; then it would connect with the first one of the consequences (a) of centrifugal force, to yield the conclusion derived by the argument's proponents.

Here we see the reason for the qualifying phrase "at most" in the previous criticism. For Galileo objects that a rotating earth would not really scatter its surface bodies toward the heavens, his argument being essentially a quantitative one: though it is true that the cause of scattering increases as the speed when the radius is constant, when speeds are equal the cause of scattering decreases as the radius increases, so that this cause increases directly with the speed and inversely with the

radius; thus, perhaps this cause remains constant when the speed increases as much as the radius, namely when equal numbers of rotations are made in equal times; hence, the earth's rotation would cause as much scattering as a wheel which rotated once in twenty-four hours; that is why there would be no scattering on a rotating earth (Galilei 1953, pp. 211-12 and 217-18).

3. The Experimental-Critical Case Study

These arguments were meant to convey an idea of the very different subject matter of the reasoning studied respectively by Perkins and by myself. But of course the fact that the content of the reasoning is so very different does not preclude a theoretical similarity in its structure and its evaluation, in terms of descriptive and normative principles at some level of generality; nor does it preclude a methodological similarity in the procedures followed for investigating it. The extent of these theoretical and methodological similarities will be seen presently. Let us begin with Perkins's experiment.

3.1. *The Experiment*

Perkins examined 320 subjects, divided into 8 groups of 40. They were mostly students, partly from high school, partly university undergraduates, and partly graduate students. They also included two groups of adult nonstudents, one with and one without university degrees.

His procedure was to have a one-hour interview with each subject, focusing on two issues. At the beginning of the hour, the experimenter selected one of the above issues and asked subjects to reflect on it with the aim of formulating and justifying a conclusion, or to give reasons for both sides if they could not decide in favor of one or the other conclusion. Then subjects were asked to articulate whatever reasoning

they did on either side of the case. Another important step was that, even when subjects had justified a given conclusion, they would be asked by the experimenter to formulate two objections against it. This process was then repeated for a second issue. Each interview was tape-recorded for later analysis.

I believe that Perkins's central interest was the determination of the difficulties in everyday reasoning, as the title of one of his papers explicitly suggests. That is, he wanted to determine what are the shortcomings of everyday reasoning, whether for example it is beset by formal fallacies like affirming the consequent, inductive fallacies like hasty generalization, or informal fallacies like *ad hominem* argument. One interesting and important aspect of Perkins's work is that, to use his own language, he decided to "operationalize" this problem by determining "to what sorts of objections are everyday arguments subject" (Perkins et al. 1983, p. 180). We might say that he decided to continue to follow an empirical approach in studying the evaluation of reasoning, in exploring how arguments are actually criticized.

We have already seen that experimental subjects were one source of these objections since each was asked to criticize his own conclusion in two ways. However, the data base of objections was expanded much beyond that as follows. One other source was the interviewer since "depending on certain decision points in the interview, the experimenter often made an objection, a standardized one, if applicable, or one conceived on the spot" (Perkins et al. 1983, p. 180). A third source was two of the three experimenters who listened to a large number of the taped interviews. Altogether this procedure generated about 2000 objections.

The analysis of these objections involved first devising a classification system, which was done by the third experimenter who had not listened systematically to the taped interviews. Then each

objection was classified by each of the experimenters, and those without concurrence by at least two judges were discarded. Although the system had 55 categories, 90% of the objections fell into 18 categories with at least 2% each, and a large majority (of much more than half) fell into just nine categories, which will be explained shortly.

3.2. Results

The two most striking first-level conclusions are the following. The first is that most objections (a simple majority of more than half) involve categories that are non-logical, in the traditional sense of either deductive logic, inductive logic, or the logic of informal fallacies. In other words, most difficulties with everyday reasoning are nonlogical. Of course, it remains to be seen what their nature is, or whether the notion of logic should be expanded to include them, given that we are dealing with reasoning after all. Second, the most common flaw of everyday reasoning, that is by far the single most frequent objection, was one which Perkins calls "contrary consequent."

3.3. Classification of Objections

Let us, therefore, begin our review of the most common flaws of everyday reasoning found by Perkins with this category. Contrary consequent is defined by him as the difficulty of "starting with the same situation . . . and reasoning to a 'contrary consequent', or one inconsistent with the supposed consequent" (Perkins et al. 1983, p. 181). In other words, "while the subject argued from A to B, one could alternatively argue from A to not-B . . . by a different line of argument" (Perkins 1989, p. 180). An example may be drawn from the issue of the American military draft and world influence: "a subject might argue, 'A military draft would strengthen the army and hence impress foreign nations into complying with our policies'. A contrary

consequent argument would be: 'But, on the other hand, a military draft might provoke widespread protest, displaying our disunity as a nation, and decreasing our influence on foreign nations'" (Perkins 1989, p. 180). It is obvious that Perkins is using the term "consequent" to mean "consequence" or "conclusion", and not to mean the second ("then") clause of a conditional proposition, which is the traditional logical meaning of the term "consequent"; nevertheless, his meaning is clear, and I see no problems with this category.

A second common difficulty unearthed by Perkins involves a category he labels "contrary antecedent" and defines as follows: "saying the supposed consequent of the argument is not, or not necessarily, a consequent of the argument at all, but instead a consequent of something else" (Perkins et al. 1983, p. 182). The example he gives is the following: "subjects occasionally argue, 'People are watching more and more television and violence is on the upswing, so television violence increases real world violence'. To this, there is the objection, 'There are many other possible causes for increasing violence—for instance a lax judicial system'" (Perkins et al. 1983, p. 182).

Some comments are in order here. First, it is obvious that the original argument is an excellent candidate for the classical inductive fallacy of *post hoc ergo propter hoc*, and therefore we must be careful when claiming that the difficulties studied do not involve the classical fallacies, for what may be happening is simply to give a different label for the traditional things. However, rather than pursuing this line of criticism, I should like to make what is perhaps a more constructive suggestion, moving in the direction of reducing this category to the previous one, thus increasing the frequency of contrary consequent even further. The disagreement in this type of polemic involves two causal claims, the one alleged in the conclusion of the original argument, and the one alleged

in the objection. That is, the objector is denying that television violence increases real-life violence, and proposing that perhaps the lax judicial system increases real-life violence; in the process, he is not denying the correlation stated in the premise of the original argument, but simply suggesting a different line of argument using additional available evidence. We thus have a case of arguing from A to not-B, in response to an argument from A to B, and this was precisely the so-called contrary consequent category. In other words, "contrary antecedent" is the special case of "contrary consequent" when there is a special relationship between the conclusion derived in the original argument and the contrary one derived in the objection: the two conclusions are "contrary" in the sense that one claims that *c* causes *e*, the other claims that *c'* causes *e*, and *c* and *c'* are different. This reduction of the second category to the first would explain why Perkins mentions contrary antecedent only in his first, more preliminary article (1983), and not in his later more definite one (1989).

Be that as it may, let us go on to the next category, which he calls "external factor." In Perkins's words, this "holds that another intervening factor blocks or vitiates the inference, which is not denied as a general tendency, but denied in the case of concern, because of the external factor" (Perkins et al. 1983, p. 182). His example is that "people arguing the draft issue sometime say, 'We have a large population that would pull through in any military crisis'. Objection: 'A large population used to help, but today modern nuclear weapons can make short work even of a large population'" (Perkins et al., p. 182). In regard to this category, Perkins himself points out that it is a special case of contrary consequent, namely the special case where the "contrary consequent" presumably follows due to an external factor, rather than to something intrinsic to the situation mentioned in the premise of the target argument. This explanation suggests to me that

we may take the categories of "external factor" and of "contrary antecedent" as special cases of "contrary consequent," namely cases where the contrary consequent is derived from additional considerations of special interest.

Another one of Perkins's categories is that of "neglected critical distinction." Such an objection amounts to charging the following: "That may be true in general, but in this situation you are not distinguishing between certain relevant subclasses. In fact, the critical subclass is one you've overlooked, and it turns out contrary to the generalization" (Perkins et al. 1983, p. 183). His example is taken from the five-cents deposit issue, in response to the idea that the deposit would motivate people to return the bottles and cans: "many people may do so. But you have to distinguish between people consuming beverages in their homes and people on picnics, or bumming around streets and parks. It's much less convenient for the latter to return the bottles, and they are the ones that do most of the littering anyway" (Perkins et al. 1983, p. 183). Again, we are dealing with a contrary consequent, in this case due to a so-called critical distinction.

A fifth common difficulty involves what Perkins calls "alternative argument." This is defined as "a certain kind of an objection to an objection. The new objection acknowledges the force of the original objection, but argues that the inference goes through on other grounds in any case" (Perkins et al. 1983, p. 183). For example, the original argument might claim that five cents will motivate people. An objection to this might be that five cents is not enough. And then one might object to this objection as follows: "Probably not by itself. But a bottle bill and the associated publicity will make people more environmentally conscious, so they will be more careful about litter" (Perkins et al. 1983, p. 183). It seems clear that this is a special case of contrary consequent when the target argument happens to be itself an objection.

These five categories account for at least 60% of Perkins's data base, and we may agree with him that they are relatively nonlogical, in the traditional sense of logic. We may also agree with him that these five categories have something in common, although the exact description and conceptualization of this common element is a more controversial matter. To give a flavor of his type of analysis, we may quote here his words to the effect that these five categories "all involve objections that introduce new causal chains or other major elements into the reasoner's model of the situation" (Perkins et al. 1983, p. 184), a point we shall discuss later. However, at a lower level of analysis, I believe my account above has already established that these five categories all reduce to the first one, in the sense that the last four are special cases of the first.

Let us now examine the four other categories discussed or mentioned by Perkins which are relatively more logical-sounding. They are labeled disconnection, scalar insufficiency, counterexample, and contradiction.⁹ Disconnection, which accounts for a sizable 13% of the data, is defined as a difficulty in reasoning "where the reasons seem to have nothing to do, or not enough to do, with the conclusion" (Perkins et al. 1983, p. 182). This is simply what one might expect from the label, and corresponds to what others would call *nonsequitur* or irrelevance.

Counterexample difficulties involved 11% of the objections, and the category is essentially what one might expect. Perkins adds, however, that objections stemming from individual cases as well as subsets of cases were treated as involving counterexamples, but that both individuals or subsets had to have some empirical basis in order to be treated as such.

Scalar insufficiency, for which Perkins does not give a percentage, occurs when "there is an insufficient degree of a factor for the consequent to follow, or for it to follow to the expressed degree" (Perkins et al.

1983, pp. 182-83). For example, in regard to the effectiveness of the five-cent deposit law, a common objection was that "five cents isn't enough for people to bother with" (Perkins et al. 1983, p. 183). Another example which could be subsumed under this category is Galileo's point that the centrifugal tendency on a rotating earth could not be sufficient to counteract the downward tendency due to weight. And in turn, this should be sufficient to suggest that scalar insufficiency is not necessarily a trivial affair.

The category of contradiction accounts for 7% of the cases and is defined as one might expect, namely as the charge that the target argument contains some kind of inconsistency (Perkins 1989, p. 180).

4. The Historical-Textual Case Study

Before proceeding with further analysis, it is best to present the data base and some of the conclusions of the historical-textual investigation¹⁰ to which I wish to compare the experimental study just discussed.

4.1. *The Data Base*

The historical-textual approach begins with the selection of some important book of the past, containing a suitably wide range and intense degree of reasoning. Many of the classics would fulfill this requirement, for example, Plato's *Republic*, Thomas Aquinas's *Summa Theologica*, Galileo Galilei's *Dialogue on the Two Chief World Systems*, David Hume's *Dialogues Concerning Natural Religion*, Charles Darwin's *Origin of Species*, perhaps Karl Marx's *Capital*. Not all classics would be appropriate; this is easy to see for works of poetry, fiction, and literature. Historical works such as those of Thucydides, Guicciardini, or Burckhardt do contain an occasional argument, but not sufficiently frequently. I do not think that certain philosophical classics would qualify either, if we are thinking of such works as Aristo-

tle's *Metaphysics*, Descartes's *Meditations*, Kant's *Critique*, or Hegel's *Phenomenology*. The problem with them would not be an insufficient degree of reasoning, but an insufficiently wide range of topics. In other words, they would make good case studies in philosophical reasoning, whereas our present concern is reasoning in general. Analogous remarks apply to mathematical classics such as Euclid's *Elements*. In some cases works other than the classics would serve the purpose, for example collections or selections of judicial opinions of bodies like the United States Supreme Court or the World Court in The Hague.

The book chosen was Galileo's *Dialogue on the Two Chief World Systems*, first published in 1632. This is the book that led, a year later, to the famous trial and condemnation for heresy by the Roman Inquisition, an episode which came to be described by some as "the greatest scandal in Christendom." From a scientific point of view this book represented Galileo's mature synthesis of the new astronomy revolutionized by Copernicus in 1543 and by his own telescopic discoveries, and of the new science of mechanics on which Galileo had been working for more than forty years. His *Dialogue* of 1632 should not be confused with his other famous book, the *Discourse on Two New Sciences*, which was published in 1638 and lays the foundations of mechanics and of engineering and avoids discussion of astronomical and cosmological topics.

In the present context, the important point is that Galileo's *Dialogue* is a 500-page work full of reasoning on all sorts of topics, ranging from astronomy, mathematics, and physics, to philosophy, common sense, and everyday life. There are arguments and counterarguments about such things as the perfection of the universe; the natural motion of bodies; the similarities and the differences between the earth and the heavenly bodies; the role of Aristotle's authority; the causes of the tides; and the location (in the atmosphere

or in the heavens) of the nova of 1572. Some of the arguments are about whether the earth's axial rotation is rendered impossible by the empirical evidence from vertical fall, from the experiment of dropping a rock from the top of the mast of a moving ship, from east-west gunshots, from north-south gunshots, from vertical gunshots, from point-blank gunshots, from the flight of birds, and from the extruding power of whirling. Other arguments are about whether the same idea can be refuted by the thought-experiment of dropping a ball from the moon to the earth, by the epistemological principles that all natural phenomena must be explicable and that the senses cannot deceive us, and by the metaphysical principles that each simple body must have one and only one natural motion, that similar substances must have similar motions, and that motion cannot last forever. Still other arguments are about whether the earth's orbital revolution around the sun conflicts with Biblical passages and with available astronomical observations concerning the dimensions and distance of the stars, the elevation of the celestial pole and of the stars, the lack of stellar parallax, and the seasonal changes in the sun's apparent motions. Finally, there are arguments about whether the earth's motion is rendered probable by the principle of simplicity, whether it is confirmed by evidence from the relationship of orbital periods and sizes and from the heliocentrism of planetary motions, and whether it is the best explanation of retrograde planetary motion and of the apparent annual motion of sunspots.

By one count there are seventeen main arguments that Galileo gives in support of conclusions he favors, and twenty-nine critiques of arguments he opposes. And I am referring here to main arguments and main subdivisions of the book, and not to the various subarguments that are parts of these; counting the latter would yield a much greater number. Moreover, it is possible to show that all these forty-six main

discussions can be integrated into a single argument, since the seventeen main positive conclusions are all parts of or steps toward the single cosmological thesis that the earth moves, while the twenty-nine critiques support negative conclusions that undermine the opposite thesis that the earth stands still at the center of the universe.

All this is still relatively preliminary, in the sense that it is the sort of fact that justifies the selection of such a work for a historical-textual case study in the theory of reasoning. In a sense this fact is immediately obvious even to a casual reader of the *Dialogue*, as long as he has the proper appreciation for reasoning. The articulation of the details of this fact is, of course, another story, and a long story at that; it is part of the spadework required as a precondition for a fruitful investigation.

The next step was to elaborate a relatively a priori element of my historical-textual approach, for as mentioned above, by empirical I do not mean empiricist. It was to find and articulate some principles for the systematic collection of the data. Four ideas were of paramount importance here. First, all data should consist of reconstructed arguments, that is, interpretations of arguments contained in the text, restated in natural language in such a way as to portray them as clearly and accurately as possible, by ignoring extraneous material and adding as many reasoning indicator terms as needed. Second, an argument should be reconstructed with the primary aim of exhibiting its propositional structure, that is, the inferential and ratiocinative interrelations among the various statements or propositions that are its constituent parts; in this context a proposition is taken as the basic unit of acceptance or rejection or as the basic carrier of truth or falsehood, and so the portrayal of propositional structure is an intermediate level of analysis, since it disregards the internal structure of propositions, or to be more exact, it leaves this finer structure to a secondary or subsequent stage of inquiry.¹¹ Third, the most useful

method of describing the propositional structure of an argument turned out to be the technique of root or inverted-tree diagrams, coupled with a numbering system that assigns to each proposition a sequence of numbers which uniquely defines its place in the network: for example, "1-1" and "1-2" would be respectively the first and second premises supporting proposition "1", while "1-2-1" would be a proposition supporting "1-2" and "1-2-1-1" would support "1-2-1."¹² Fourth, the normative and evaluative aim of the project should be addressed by interpreting the book's critiques of arguments as arguments about arguments and reconstructing them in the manner just sketched. This step is in accordance with another point mentioned earlier to the effect that by empirical I do not mean "value-free."

When the text of Galileo's *Dialogue* is studied in accordance with these principles, the forty-six main arguments mentioned earlier generate several hundred reconstructed subarguments, each of which may to some extent be examined by itself. This constitutes a rich and varied data base which one may then analyze in an empirical fashion to determine what interesting conclusions it supports. These theoretical implications will be discussed presently, but first I should like to stress that this data base is intended to be neutral vis-a-vis these conclusions, and hence it is to be hoped that other researchers will subject it to their own analysis to test their own theories. In other words, the construction of such data bases, by using other appropriate classics, is a valuable element of the empirical study of reasoning, and I believe that the one I have constructed for the case of Galileo's *Dialogue* will be found to have value independently of the conclusions I shall be drawing from it.

4.2. Conclusions

In the present context the most telling conclusions involve what I call "evaluation

methods," namely techniques used for determining whether a particular instance of reasoning is correct or incorrect. I labeled the six most frequently occurring ones as follows: method of alternative conclusion, active evaluation, *ad hominem* argument, method of counterexample, principle of charity, and explanation of error in reasoning. This list is partly overlapping and partly open-ended, and the three most relevant methods are alternative conclusion, active evaluation, and *ad hominem* argument.

The method of alternative conclusion tries to show that a conclusion does not follow from the premises because some other different conclusion follows instead; here the alternative conclusion is meant to be different in the sense that there is some conflict (though not necessarily a strict formal inconsistency) between it and the original one. This is not to say that one criticizes an argument by simply giving a counterargument supporting a conclusion conflicting with the original one, for this might be a species of irrelevant criticism (if the premises of the counterargument were completely new and had nothing in common with those of the original argument). Nor is it to say that one shows simply that the exactly same original premises imply a proposition conflicting with the original conclusion, for this situation would represent only a special case. Rather, one starts with the same original premises, but then adds other contextually appropriate propositions (perhaps in the process subtracting some from the original set), in order to arrive at a conflicting conclusion in such a way as to utilize some of the original premises in the new counterargument.

The other two methods are related. Active evaluation is the procedure of testing inferential relationships among propositions by becoming actively engaged in the reasoning being evaluated, that is by arguing at the level of, and largely in terms of, the argument being evaluated and checking whether what follows from its premises is the conclusion it draws or some other

proposition. *Ad hominem* argument is not meant in the present-day sense of the informal fallacy of attempting to discredit an argument by discrediting the character or motives of the person advancing the argument; rather it is meant in the seventeenth-century sense, namely as reasoning where the arguer derives a conclusion not acceptable to an opponent from premises accepted by the opponent, but not necessarily generally acceptable.¹³

Here I have focused on these three, repeating the original terminology used in the original study, for several reasons. First, all three seem to relate in some obvious sense to Perkins's key category of contrary consequent.¹⁴ Second, although I did not do a specific quantitative analysis, there is no question that these three methods were collectively the most frequent ones. Finally, all three seem to be variations on a common theme, in the sense that both active evaluation and *ad hominem* argument may be thought of as special cases of the method of alternative conclusion.

That is, *ad hominem* argument is the special case where the alternative conclusion is not acceptable to the proponent of the original argument and where the additional premises that yield it are acceptable to him; and active evaluation is the special case where the alternative conclusion is grounded on additional considerations that remain, at least relatively, within the spirit of the framework of the original argument. Using symbols, the explanation might be as follows. Suppose we have the target argument, C because R1 and R2. The alternative-conclusion criticism tries to show that starting with R1 and R2, what really follows is not-C rather than C, because of R3, R4, ..., and R-n, where the additional propositions R had been overlooked by the original argument. In the general case the additional propositions are simply propositions which are justifiable in the context, whether or not they were known to the original arguer; therefore, the full critical counterargument would have to contain a

justification of these additional propositions. What makes such criticism particularly effective is the fact that the critic begins by agreeing with the initial premises of the original argument, but then goes on to point out things the consideration of which leads into a different direction. When the additional propositions are accepted by the original arguer, but are not generally acceptable, we have the case of an *ad hominem* argument in the seventeenth-century sense. When the additional propositions are either relatively uncontroversial, or relatively immediate consequences of the original premises R1 and R2, then we have the case of active evaluation; however, the notion of active evaluation also calls attention to the fact that the criticism is a counterargument, and so in this kind of criticism one becomes actively engaged in reasoning.

4.3. *Galilean Illustrations*

For some illustrations, consider the following anti-Copernican argument,¹⁵ called the contrariety argument in favor of the earth-heaven dichotomy: (12) bodies do not change unless there is contrariety; (11) there is no contrariety among heavenly bodies; therefore, (1) heavenly bodies are unchangeable. (The “contrariety” mentioned in this Aristotelian argument refers to such relationships as hot versus cold, light versus heavy, and dry versus wet.)

One of Galileo’s objections to this is that the anti-Copernicans should not stop here but go on as follows: (21) bodies which have contraries are changeable, since (211) bodies do not change unless there is contrariety; but (22) heavenly bodies have contraries, since (221) heavenly bodies are unchangeable, (222) terrestrial bodies are changeable, and (223) changeability and unchangeability are contraries; therefore (2) heavenly bodies are changeable. This is an *ad hominem* criticism of the original argument, *ad hominem* in the precise sense defined here; and it is also a case

of active evaluation because the criticism is adding very little beyond what the original argument asserted.

Another Galilean objection is the following. (3) It is questionable whether contrariety is absent even within the heavenly region since (31) Aristotle would regard heavenly bodies as the denser parts of the heavens, (32) if heavenly bodies are regarded as the denser parts of the heavens then differences of rarity and density exist in the heavens, and (33) if differences of rarity and density exist in the heavens then a change-producing contrariety exists in the heavens; this is so because (3311) differences of rarity and density give rise to the light/heavy contrariety in the terrestrial region, (3312) this contrariety gives rise to the upward and downward spontaneous motions, and (3313) these motions are allegedly the source of terrestrial changes, and hence (331) differences of rarity and density may be regarded as the cause of terrestrial changes; moreover, (3321) the cause of terrestrial as well as celestial differences of rarity and density is the quantitative difference of more or less matter in a given space, (3322) the cause of terrestrial differences of rarity and density is not the qualitative difference of heat and cold (since (33221) the density of solid substances changes little when their degree of heat changes significantly), and hence (332) the cause of terrestrial differences of rarity and density is the same as the cause of celestial differences of rarity and density.

The *ad hominem* element here is the fact that the anti-Copernicans are obliged to admit the presence of contrariety in the heavens (conclusion unacceptable to them), and the fact that they themselves would have to say that heavenly bodies are the denser parts of the heavens (premise accepted by them but not necessarily acceptable). The argument also has an element of active evaluation in the sense that it requires some immersion in the Aristotelian framework to justify the point made in the objection that celestial differences of

rarity and density are a change-producing contrariety (proposition 33). And the objection also has an element of general alternative conclusion in the sense that it justifies a number of needed propositions as part of the counterargument.

Referring to the a posteriori justification of the earth-heaven dichotomy, mentioned earlier, we have an example of active evaluation in Galileo's third critical point; it amounts to saying that if otherwise correct, the conclusion to draw would be that the heavenly bodies are unchanged so far (and not that they are unchangeable). His fourth objection to the same argument provides an example of alternative conclusion, since it amounts to saying that what follows is that perhaps the heavens have not been observed carefully and systematically enough (rather than that the observed lack of changes corresponds to reality).

A final example is provided by Galileo's first criticism of the centrifugal force argument, also mentioned earlier; the criticism amounts to saying that the argument as stated only implies that the earth has not just changed its state from rest to rotation (rather than that the earth is and has always been in a state of rotation).

5. Comparative Analysis of the Two Case Studies

What are we to make of this convergence? Let us begin by trying to characterize it more precisely, and then we can examine its implications. Three points should be noted about the convergence, namely that it is partly substantive and partly methodological, and that it is not total insofar as the above mentioned common conclusions are embedded in different theoretical frameworks.

5.1. Substantive Similarities

In saying that the convergence is partly substantive I mean that both Perkins's experimental-critical study and my

historical-textual one have arrived at generalizations which make very similar claims. Let us analyze this similarity. One way of stating Perkins's conclusion is that the most common flaw of everyday reasoning is the failure to consider contrary lines of reasoning, that is lines that would arrive at contrary conclusions. And one way of stating the conclusion of my historical-textual study is to say that the most effective way of criticizing reasoning is to apply the method of alternative conclusion, that is to justify a conclusion conflicting with the one under consideration largely on the basis of the same premises. Aside from the question of the proper domain, the two statements are two aspects of the same point. The first stresses a property belonging to reasoning, but the property is a negatively evaluated trait, and so there is a corresponding method of establishing such a trait, which is what the second statement focuses on. We may also say that the first is an evaluative claim about the phenomenon of reasoning, the second is a normative principle about what to do in order to detect the phenomenon or when faced with the phenomenon. Or again, the first is an evaluative judgment upon the situation, the second is a normative principle about what one ought to do. However, there is a correspondence between the two.

By the question of the proper domain I mean the issue of exactly what phenomenon these conclusions deal with. Perkins speaks of everyday or informal reasoning, and contrasts it to formal reasoning best exemplified in mathematics. In my investigation I have dealt with a classic scientific controversy, but I would hesitate to speak of scientific reasoning without qualification because my aim was to focus on some universal and fundamental aspects of the scientific reasoning in question. Here one could speak of revolutionary scientific reasoning, in the sense of Thomas Kuhn's distinction between normal and revolutionary science (Kuhn 1962), but I am not sure I would want to convey all the connotations

that such phraseology would give. A common property of both Perkins's everyday reasoning and the scientific reasoning I have studied is controversy, and so one could speak of controversial reasoning and say that both conclusions apply to controversial reasoning, namely reasoning in controversial situations. One problem with such a focus is that the common result then seems much less striking, for the result reached is really part of the nature of controversy and so was to be expected. Perhaps it would be best to take the domain to be that of informal reasoning, as Perkins himself suggests; but then we must not equate it with everyday reasoning and instead allow it to include scientific reasoning.

5.2. *Methodological Similarities*

In regard to the respective approaches, aside from the empirical orientation, it might appear at first look that doing psychological experiments with human subjects and reading texts in published books are as far apart as any two procedures could be. However, if we scratch under the surface, we begin to see the similarities. To begin with, the experiments were primarily the means to get the subjects to engage in reasoning, and the most immediate product was audiotapes whose content was then studied and analyzed. Thus, it seems to me that the heart of Perkins's data too was texts, though delivered orally and recorded in tapes. More importantly, unlike the traditional experiments in cognitive psychology, Perkins's experiment was designed to produce not merely yes or no answers, or multiple choice decisions, but rather reasons for the answers; in fact, the actual answers were relatively unimportant since the subjects were instructed to explain the reasons on both sides in case they could not decide in favor of one, and in any case they were also asked to formulate objections to two aspects of their own arguments. Moreover, they were given some time to reflect on the issue before explaining their

reasoning. Finally, the collection of objections originates in part from two of the three experimenters who listened to a large sample of the tapes. We thus have elements of reflection, ratiocination, and critical discussion of the type that is the heart and soul of scientific and other scholarly research and is then written up in books and articles. Of course, even such experimental data are still a long way from the complexity, richness, and polish of a published scholarly or scientific book, but the difference is merely one of degree, whereas my impression is that the contrast to the data of the usual experiments of cognitive psychology is one of kind. Therefore, besides noting the similarity between the experimental-critical approach practiced by Perkins and what I have called the historical-textual approach, I would also argue that Perkins's approach is much more valid than the usual experiments and provides the only effective experimental means of getting in touch and coming to grips with the phenomenon of reasoning.¹⁶

In short, the experimental approach of the type practiced by Perkins and the historical-textual approach I have practiced are indeed two variants of the empirical orientation, but what they have in common is much more significant than what distinguishes them. Here, I am making a purely methodological point, for in regard to theoretical framework the situation is different and certainly not so simple. To this we now turn.

5.3. *Theoretical Frameworks and Differences*

By theoretical framework I mean the network of conclusions, ideas, concepts, and principles which Perkins derives from his experiment, but also from other sources, and in the context of which he holds the substantive conclusion mentioned earlier (that the key difficulty in everyday reasoning relates to contrary consequent and its cognates).

Let me begin by saying that I do not disagree with Perkins that even this conclusion is strictly speaking a theoretical claim because each of his classification categories is in a sense a theoretical construct. Nevertheless, I would add that this conclusion, and its mirror image in my account, is relatively low level and relatively close to the data, and so the theoretical leap is rather small.

After this Perkins goes on to argue that most objections "extend the reasoner's current model of the situation" (Perkins et al. 1983, p. 183) and that the essential difficulties are underexploration and bias in "situation modeling" (Perkins 1989, pp. 178-79), a notion he adapts from Johnson-Laird (1983); and in line with this type of consideration, he advocates a "critical epistemology" as a solution, and then does another experiment suggesting that an intrusive, maieutic, Socratic technique can effectively teach this epistemology and can lead to significant improvement in reasoning (Perkins 1989, pp. 185-88). He also argues that, on the other hand, the impact of conventional education on informal reasoning is negligible (Perkins 1989, pp. 181-83); and that the imagination has an essential role to play in reasoning, informal and everyday, as well as mathematical (Perkins 1985). Another important conclusion he elaborates is that almost all objections involve context-specific considerations (Perkins et al. 1983, pp. 179, 184), and that, as mentioned earlier, most do not involve traditional logical considerations. And in regard to the nature of informal reasoning and its differences from formal, Perkins argues that whereas in informal reasoning premises are not fixed, they are in formal reasoning; that whereas in informal reasoning inferential links are not perfectly reliable, in formal reasoning they are meant to be; that whereas informal reasoning involves many lines of argument, formal reasoning is one-lined;¹⁷ that, whereas in informal reasoning each line of argument addresses both sides of the issue,

formal reasoning is one-sided;¹⁸ and that, whereas in informal reasoning each line of argument is short, formal reasoning consists of long linked chains of argument (Perkins et al. 1983, pp. 177-79; Perkins 1985, pp. 20-21; Perkins 1989, pp. 176-78).

Much of this is interesting, insightful, and acceptable. Moreover, some of these conclusions are similar to ones I have drawn, as is the case, for example, in regard to the importance of context in reasoning and in its evaluation.¹⁹ However, most of these conclusions are not the ones I have drawn from my data. In this context, I can only focus on one possible dissimilarity, disagreement, or divergence. It regards the question of whether or not, or the sense in which, the major difficulties of informal reasoning are logical.

Part of this issue may be a verbal disagreement; that is, the issue hinges in part on what we mean by logic. As mentioned earlier, there is no doubt that both his data and mine show that the faults of informal reasoning are not primarily the fallacies of traditional deductive or inductive logic, or the traditional logic of informal fallacies. On the other hand, if we take logic to be the theory of reasoning, a conception which can be argued to go back to the father of the science of logic, Aristotle (Johnson 1987), then any theoretically significant phenomenon in this domain would to that extent be of logical interest. Therefore, it may be preferable to approach this issue from another angle.

In my discussion above, for the sake of incisiveness and in order to stress an important substantive similarity, I said nothing about a cluster of concepts suggested by my investigation which are in one sense more closely analogous to Perkins's categories for classifying objections. I call them evaluation categories, and they enable me to classify criticisms of arguments. The cluster is not meant to be exhaustive, and the items are not meant to be mutually exclusive. In alphabetical order, they are the following: circularity, equivocation,

fallacy of composition, groundlessness, incompleteness, infinite progression, invalidity, irrelevant conclusion, question-begging, self-contradiction, and uselessness. Especially revealing for some additional similarities with Perkins's work would be the categories of groundlessness, incompleteness, irrelevant conclusion, and uselessness. But that is not what I wish to elaborate here.

Rather the point to reflect on is that the single most frequent category is invalidity, which is reminiscent of Perkins's notion of disconnection. I do not mean invalidity simply in the sense of deductive formal logic, but rather as something of a generalization of it. That is, I wanted a label to refer to a flaw in reasoning which involves the failure to connect properly premises and conclusion, namely a flaw in the inferential link between premises and conclusion. I believe this sense of invalidity to correspond more or less to the ordinary language meaning of the word *invalid* when applied to arguments, but that is another story. It should be stressed that, although such a notion of invalidity overlaps with formal invalidity insofar as it refers to challenges to the premise/conclusion connection which may very well involve bringing in other premises, it still retains the contrast with direct challenges to the simple truth of the explicit premises of an argument.

Now, taking the hint from this empirical fact about the pervasiveness of invalidity in my data, I began a later stage of analysis²⁰ with this general notion. That is, I began by defining invalidity as the failure of one proposition to follow from others, so that an inferential step is invalid if and only if the conclusion does not follow from the premises.²¹ This is understood in a general sense such that formal deductive invalidity is the special case when this failure comes about for the reason that a counterexample argument exists, namely another argument of the same form with clearly true premises and clearly false conclusion.

The inductive incorrectness of inductive arguments turns out to be in large measure the special case of invalidity when the reason why the conclusion does not follow from the premises is that it does not follow any more likely than some other specifiable proposition. In other words, in such a situation the critic produces another argument which has (or at least includes) the same premises as the original argument but a different conclusion, and which appears to be of equal strength as the original. This occurs primarily with explanatory (or causal) arguments whose conclusion is an explanation of what is stated in the premises, and the criticism amounts to providing an alternative explanation (or cause).

There are four other special cases of invalidity, due again to special situations, involving difficulties either with the logic of some of the informal fallacies, or with rhetorical matters, or with the justification of premises, or with the justification of presuppositions. However, there is no space here even to summarize them. Instead, let me say that it also turns out (or so I argue) that all other ten of my evaluation categories can be interpreted in terms of invalidity, in one or another of its six special cases. Moreover, what I call the evaluation methods, the three discussed above and three others I have not discussed here, also can be connected to this notion of invalidity.²²

As mentioned earlier, all this involves a later stage of analysis when an attempt is made to interpret the lower-level concepts and generalizations in a theoretically more interesting way. Such theorizing is not meant to undermine or undo the empirical reality of the phenomenon revealed earlier simply in terms of the methods of alternative conclusion, active evaluation, and ad hominem argument. However, this does suggest that the similarity with Perkins's work may be rather limited, because it does not extend to the theoretical framework in which the lower-level common generalization is embedded. And this

possible theoretical divergence, in the presence of an undeniable substantive similarity, was the main thing I wanted to explain in this context.

One final comment is in order to bring us back to the issue of whether or not the difficulties of informal reasoning are "logical." Whether my theoretical framework built on invalidity is acceptable or not, I believe it could not be denied that it deals with the logic of reasoning because it deals with matters involving the *relationships* of propositions with each other, as contrasted to issues of the relationship between propositions and the world, for example. So, while it is worth repeating that we are not dealing with traditional logic, I would want to claim that there is a way of interpreting the difficulties of informal reasoning as "logical" errors. One other point that should not be forgotten is that these are not "logical" errors in the sense that they are invented or conceived a priori, for the empirical credentials of this type of investigation should by now be beyond dispute.²³

This comparison of Perkins's experimental-critical approach and of my historical-textual approach has revealed that there are important theoretical differences underlying the common substantive conclusion, as well as significant methodological similarities underlying the *prima facie* different approaches. It would be arbitrary to say whether the methodological similarities are more important than the methodological differences, as it would be to say whether the substantive-theoretical differences are more important than the substantive-theoretical similarities. Nevertheless, by contrast to the traditional experimental-psychological and to philosophers' inductive-intuitive approaches, not to mention the apriorist approach, the substantive and methodological commonalities between the two approaches discussed here give them a research potential which is far from negligible.

6. Recapitulation

In conclusion, reasoning may be defined abstractly as the interrelating of thoughts in such a way that some follow from others, and operationally in terms of linguistic expressions with a high incidence of words like therefore, because, and consequently. The empirical approach is to be contrasted with an apriorist and an empiricist orientation, but not with a normative aim, and so the empirical approach advocated here aims to study mental processes which exist independent of the investigator. There are at least four varieties of the empirical approach, namely the traditional experimental method of cognitive psychologists, the intuitive-inductive method of analytical philosophers, the experimental-critical approach pioneered by David N. Perkins, and the historical-textual approach attempted by the present author. Perkins has studied primarily everyday informal reasoning involving such issues as the advisability of a small deposit on the purchase of bottled and canned beverages in an attempt to reduce litter and whether compulsory military service in the United States would increase American world influence. The present author has studied primarily informal but scientific reasoning, as it occurred during the Copernican Revolution, and as recorded in Galileo's book *Two Chief World Systems*.

Perkins's experiment interviewed 320 subjects and produced a collection of 2000 objections, 90% of which could be classified into nine categories. Of these, the most common one was that of contrary consequent, which then together with four other related categories accounted for a large majority of the data. These categories are nonlogical in the traditional sense, and so it seems to be a well-established conclusion that most difficulties with everyday informal reasoning involve a failure to appreciate contrary arguments.

The historical-textual study by the present author was a critical examination of Galileo's *Two Chief World Systems*,

which created a data base consisting of hundreds of reconstructed arguments about natural phenomena as well as criticisms of other arguments. Here the inescapable conclusion is that the most effective way of criticizing reasoning is to use what I call the method of alternative conclusion, and its variants active evaluation and ad hominem argument in the seventeenth-century sense of this term.

The substantive similarity between the two respective conclusions is striking, and provides additional mutual reinforcement. This coincidence also strengthens the viability and effectiveness of the two

respective methods, the experimental-critical approach and the historical-textual approach, and it also points to the large overlap between these two methods. However, neither these methods nor the respective results are identical, and in fact the respective theoretical frameworks, while containing other similarities, contain also important differences. One of the most important of these theoretical differences is the issue of the nature of the concept "logic," and in what sense and to what extent the flaws of informal reasoning are logical. The paper ended with an elaboration of this difference.

Notes

¹ This paper was first published in German in *Zeitschrift fuer Semiotik*, vol. 15, 1993. An earlier version was presented at the Summer Institute on Argumentation, University of Amsterdam, Holland, 25-26 June 1990; I thank Frans van Eemeren and Rob Grootendorst for the invitation and David Hitchcock for some valuable comments, which I hope to be able to take into account on some other occasion. Another version was presented at the Interdisciplinary Research Project on "Cognition and Context," Technical University of Berlin, Germany, 4 May 1992; I thank Professors Roland Posner and Bernd Mahr for the invitation and encouragement. I also thank the referees of *Informal Logic* for valuable comments, and its editors for being willing to consider an article whose right of first publication belonged to *Zeitschrift fuer Semiotik*.

² See the works listed under my name in the references. This is also in the tradition of Barth (1985a, 1985b, 1987), Barth and Martens (1982), Barth *et al.* (1992), Johnson (1987), Johnson and Blair (1980, 1985), Naess (1966, 1982a, 1982b), and Scriven (1987).

³ Again, see the works listed in the References.

⁴ See Cohen (1986) and its references to the works of these and other analytical philosophers; cf. Finocchiaro (1991).

⁵ See Johnson and Blair (1980, 1985), and Finocchiaro (1984).

⁶ See Finocchiaro (1991) for the details of the criticism (but also appreciation) of the inductive-intuitive approach, and Finocchiaro (1979; 1980, pp. 256-72; 1989) for the details about the experimental-psychological approach.

⁷ See his works listed in the References.

⁸ As Perkins himself clarifies, this is not an argument that was given by any one of the experimental subjects, but rather each step was given by many.

⁹ Contradiction, which accounts for 7% of the objections, was not mentioned in Perkins *et al.* (1983), but is introduced in Perkins (1989, p. 180).

¹⁰ This was originally reported in Finocchiaro (1980), but see the other references for additional clarifications and elaborations.

¹¹ Here, I make the same distinction made by Freeman (1991) between the macrostructure and the microstructure of reasoning.

¹² The first work where I saw this technique explicitly discussed is Angell (1964) and I adapted it from him. Nowadays it is very common, though various authors add various twists to the basic idea; see, for example, Scriven (1976, p. 41-43) and van Eemeren and Grootendorst (1992, pp. 73-89). The essential idea was introduced much earlier by Naess in a book first published in Norwegian in 1941, and then translated into English (Naess 1966, especially pp. 106-15).

- ¹³ This is also John Locke's meaning; see Finocchiaro (1974).
- ¹⁴ To be more exact, his classification categories correspond more directly and formally to what I call evaluation categories, but these are in turn related to the methods in the way indicated above, namely that the methods are the techniques to be used to generate evaluations in terms of various specific categories. Moreover, as sketched below, a later stage of theoretical analysis reveals more interconnections among them.
- ¹⁵ For this argument and the ensuing objections to it, see Galilei (1953, pp. 38-47) and Finocchiaro (1980, pp. 357-72). Note that the propositions are here numbered in accordance with the principles stated earlier.
- ¹⁶ See especially Finocchiaro (1979, 1989).
- ¹⁷ E. M. Barth has objected that this is questionable even for axiomatic deduction, given that the latter can be shown to be algorithmically equivalent to both Beth's semantic-tableau method and the dialogical method, and given that the dialogical method involves lines of attack and lines of defense and thus interprets a formal proof as an interplay between *lines* of attack and *lines* of defense (Barth and Krabbe 1982). I agree that one should not accept uncritically the claim that formal reasoning is "one-lined," and that in the light of Barth and Krabbe's work such a claim may well be untenable. To resolve this difficulty, I would want to clarify the notion of a "line of argument": one question would be whether Barth and Krabbe's concept is the same as the one intended by Perkins in this claim; another question would be the relationship between the notion of a "line of argument" and that of the "side" of an issue, mentioned in the next claim. This problem is both interesting and important, but I cannot resolve it here.
- ¹⁸ Once again, note that there is a question about the exact meaning of "one-sided," especially in relation to the notion of "one-lined" mentioned in the previous claim.
- ¹⁹ See, for example, Finocchiaro (1980, pp. 145-66, 305) and Finocchiaro (1984, 1987).
- ²⁰ One may usefully distinguish the stages of data collection (or construction of the data base, if you will), concept formation, generalization, and theorizing or theory articulation; cf. Finocchiaro (1980, pp. 424-31; 1987a; 1987b).
- ²¹ David Hitchcock has suggested that this should be modified to read "if and only if the conclusion is not justified by the premises," an interesting suggestion I have not been able to incorporate in this paper. In any case, some of his own work is relevant and important, especially Hitchcock (1987, 1989).
- ²² I do not wish to give the impression that there is a one-to-one correspondence between the six cases of invalidity and the six evaluation methods; the occurrence of the number six in both clusters is fortuitous.
- ²³ We are dealing with "empirical logic," as it were; cf. Barth (1985a, 1985b, 1987), Barth and Martens (1982), Barth *et al.* (1992), and Naess (1982a, 1982b).

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