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STATISTICAL LANGUAGE, STATISTICAL TRUTH AND STATISTICAL REASON: THE SELF-AUTHENTIFICATION OF A STYLE OF SCIENTIFIC REASONING

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It is a philosophical task in our times to connect:

(1) Social studies of knowledge, of the sort pioneered by David Bloor and Barry Barnes in Edinburgh, but now quite common in Europe, especially in the United Kingdom.

(2) Metaphysics, particularly the debates that resulted from Hilary Putnam's series of revised positions, beginning with the scientific realism founded on his theory of reference, but proceeding to his rejection of such metaphysical realism, and his advocacy of internal realism, recently the focus of attention in the United States.

(3) The Braudelian aspects of science, that is, the long-term slow-moving, persistent, and accumulating aspects of the growth of knowledge. Braudel, in caricature, wrote of the Mediterranean as a Sea around which nothing much happens besides shifts in climate and topography; the chief effect of civilization in Greece was to turn a forested peninsula into a rockheap.[1]

The task of connecting (1)–(3) was trivial when the whole of science was thought of as gradual Braudelian accumulation. We had (2) a metaphysics of a real world to which true propositions correspond, (3) a permanent, if sometimes subdued, will to find out the truth, and (1) types of civilization or social order—ours—that fostered ingenuity, honesty, innovation, and the growth of knowledge. That vision has fallen from favor. It may seem odd that I do not even mention a fourth item that needs reconciliation with the three that I do list—the structure of scientific revolutions. I omit it because (1)–(3) are contraries precisely because of Kuhn's work. My paper discusses a problem that takes Kuhn for granted in the background. Since he published in 1962, three types of inquiry have almost ceased to speak to each other, namely:

A. Newly gained analyses of, and case-studies of, the fleeting "microsocial" interactions of knowers and discoverers, their "macrosocial" relationship to larger communities, and the material conditions and objects in which the discoveries are made and which they are about. At this level, the relevant events last a week or at most a few decades.

B. Current philosophical conceptions of truth, being, logic, meaning, and knowledge.

C. Models of relatively permanent, growing, self-modulating, revisable features of science. Such features might begin in a lot of delicate interplays of needs, interest, and power struggles that cry out for detailed examination of a microsocial sort. Their persistence demands another analysis. The result of their persistence is a body of what is counted as objective ways of determining the truth, of settling belief, of understanding meanings, a body of nothing less than logic itself.

Philosophers, historians, and sociologists have brilliantly energized studies of (1) and (2). Putnam's metaphysics has redrawn the contours of discussion, while the plethora of perhaps misleadingly titled "social" studies of science has opened new vistas. The social construction of scientific facts school does not mention Putnam nor does Putnam mention it. Neither has much use for big units of philosophico-historical reflections. The mighty have fallen. Paradigms are distinctly out. Putnam, following Peirce, speaks grandly of what would ideally be known late in the day as a result of unceasing honorable inquiry. But this is not a vision of any actual science in the long term, for unlike Peirce, Putnam says little enough about how the inquiry is conducted. Putnam is concerned not with reasoning but with pure reason. The increasingly common references to Kant in his work are no accident. They reveal the extent to which he has lost interest in how we find out, in the details of how scientists actually carry on. Instead his papers are full of fables, science fictions used to make philosophical points, delightful to read, but the very opposite of factual microsociology.

The constructionalists form a different contrast. They study the first shift at the factory of facts. Quitting work early in the day, they leave us in the lurch with a feeling of absolute contingency. They give little sense of what holds the constructions together beyond the networks of the moment, abetted by human complacency. We now need to examine something in between timeless metaphysics and the momentary social conjunctures. What will serve?

I want something both social and metaphysical and propose my concept of a "style of reasoning."[2] It is an irrevocably metaphysical idea, yet styles, like all else human, come into being through little local interactions. So styles help fulfill the task of bridging (1) and (2). And what are "styles"? I took the name "style of reasoning" from A. C. Crombie, who, in a paper of 1978, listed six very familiar items. The only thing unusual was that he took them to be the core list of "styles of scientific thinking in the European tradition."

(a) The simple postulation established in the mathematical sciences.
(b) Experimental exploration and measurement of more complex observable relations.
(c) Hypothetical construction of analogical models.
(d) Ordering of variety by comparison and taxonomy.
(e) Statistical analysis of regularities of populations and the calculus of probabilities.
(f) Historical derivation of genetic development.[3]

These "styles" did not stay in one community or wither away. They run the world. A style of scientific reasoning is put in place in a network of people, answering to the needs, interests, ideology, or curiosity of some of its members, defended by bluster or insidious patience. But when it becomes fixed as a new way to truth, it needs no support or rhetoric, for as it assumes self-confidence it generates its own standard of objectivity and its own ideology. It starts by being pushed and shaped by social vectors of every sort; we end with a self-sustaining mode of knowledge. It becomes less something molded by interests, and more an unquestioned resource upon which any interest must draw, if it ever hopes for the accolade of objectivity. And it further determines how people conceive of themselves and their world, opening new horizons, but also constraining the possible forms of knowledge.

The relatively slow-moving, curiously permanent evolution of ways in which we know, find out, and evolve skills of thinking, asking, and investigating is eminently Braudelian. For example, once people began to reason like Euclid, they continued to do so, off and on, and always can do so, once they see how it goes. When (to use Althusser's catchy phrase) a legendary Thales "discovered the continent of mathematics" we began postulational reasoning, the deduction of or speculation about the consequences of precise assumptions. That way of thinking has grown, reconceived itself, abandoned old aspirations and achieved new heights. It is the accumulation not so much of knowledge (which even in mathematics is commonly superseded) as of ways of finding out. I am inclined to say much the same of laboratory science, which, whatever its antecedents, be-

gan to assume its present power in the world only in the "scientific revolution." But here my example is Crombie's (e), especially the fixation of the statistical style of reasoning during the nineteenth and early twentieth centuries. It has the great merit of being relatively recent so that we can almost see new kinds of objectivity emerging before our very eyes. It has also, over the past decade, benefitted from a great deal of new scholarship.

The following essay is in two parts. In Part I ("The Metaphysics of Styles") I sketch out the general idea of a style of reasoning, and its connection with truth, meaning, and verification. Then in Part II ("The Styles of Statistics") I show how the brazen metaphysical claims that I make are exemplified in a surprisingly unadventuresome way by statistical reasoning. In passing, I shall observe that as this style of thought evolved, every social dimension is on show. If you want interests, we have interests. If you want rhetorical devices, we have those. And institutions, modes of legitimation, takeover battles, constructions, uses of power, networks, intimations of control, and much, much more. Yet as the style becomes increasingly secure, these are decreasingly relevant to its status. The style ends as an autonomous way of being objective about a wide class of facts, armed with its own authority, and available as a neutral tool for any project or ideology that seeks to deploy it. It provides new criteria of truth, new grounds for belief, new objects about which there can be knowledge. It generates the very stuff about which we do metaphysics. Thus do I address the ask of connecting (1) social dimensions, (2) metaphysics and (3) a long-term, "Braudelian" aspect of science, namely styles of reasoning.

1: The Metaphysics of Styles

We can tell a good deal without much speculation, reasoning, or active reordering of and intervention in the world. That means: we do not need *any* style of reasoning to find out lots of things, because we do not need, literally, to reason. We can just go and look, and find out whether some sentences do in fact correspond to the way the world is. The banal fact that there are such "observation statements" does not imply that our remarks about what we notice or check out by looking and listening are in some way privileged, basic, or foundational. Of course what we see is affected by what we expect, by our neighbors, by our education, and by our past experience. But I say this not to defend the idea of observation, whose real-life complexities I have amply examined elsewhere, but instead to address correspondence theories of truth. And I do *that* not because I have any investment in them, but because *I do not think styles of reasoning come into play for sentences to which a correspondence theory applies*.

It is now rather fashionable to decry correspondence theories, often using that wonderful canard of William James: "copy theory of truth." Philosophers neatly divide into two camps: those who say correspondence is worthless, and those who say that a correspondence theory, accompanied by a sound theory of reference, is the only one that makes sense. I may for the moment be unique in holding the only commonsense view, that correspondence theories are on the right track for some but by no means all declarative sentences. I stated this obliquely some time ago, but the point may need recapitulation.[4]

The core objection to a correspondence theory is that there is no way in which to identify the facts to which a statement corresponds, independently of the statement itself. That is true in general, but not of a lot of the run-of-the-mill sentences of the sort beloved by logicians—subject-predicate, and subject-relation-object—the kind codified in first-order logic, and using commonplace "observational" common nouns and verbs. In a debate with P. F. Strawson, J. L. Austin had a tidy way of overcoming the standard objection, insisting that we do often have independent ways of telling what a subject term refers to and what a predicate term denotes. We can then identify the fact to which "my shoes are black" refers by independently identifying the shoes and the blackness. That is Austin's doctrine of cap-fitting.[5]

That idea depends upon a supposedly outmoded classification of "observational" terms. It is outmoded in philosophy of science, maybe, but not among progressive psycho-anthropologists, who contend that there are "basic-level" concepts that are relatively stable among languages. They are expressed by short words. They have fairly standard prototypical examples that are elicited in standard testing of virtually all speakers of a language. George Lakoff has provided an excellent resumé of these ideas.[6] I have no trouble with a correspondence theory for sentences whose terms designate basic-level concepts. Those are the sentences that, even though we sometimes deduce them from evidence, we also can tell to be true or false, on occasion, just by looking. That is the humdrum fact that provides the sound core to the idea of observation sentences.

In contrast, there are many typically complex questions that can be answered only by a process of reasoning. Indeed, it makes sense to ask them only against a background of ways of acceptably reasoning towards their answer. The answers make sense only in the context of a style of reasoning. Many ways of reasoning have been developed, "discovered." A style of reasoning grows, together with questions that it can help answer, and with the truth-conditions of the sentences on which it bears. In these cases, Moritz Schlick's motto, that "the meaning of a sentence is its method of verification," points in the right direction. This implies a radically noncorrespondence theory of truth. I have no wish to discuss theories of truth here, but these excellent if hackneyed models, correspondence and verificationism, serve my purpose of establishing that I do not think there is one theory of truth, or one semantics, that applies to all contingent empirical sentences investigated in the sciences.

Schlick did not think of "methods of verification" as having histories, and he seems to have had little sense of the motley of methods that we use. Our methods of verification have different historical trajectories, each within its own timeframe. My own study of the statistical style illustrates this. Putting such considerations aside for a moment, let us suppose that the truth conditions of some sentences are determined by the ways in which we reason to them. And suppose that a style becomes a standard of objectivity because, to use Peirce's phrase, it has the "truth-producing virtue." There then arises a suspicion of circularity. I embrace it, I welcome it. For there *is* an odd way in which a style of reasoning and truth-conditions of some sentences are mutually self-authenticating. The truth is what we find out in such and such a way. We recognize it as truth because of how we find it out. And how do we know that the method is good? Because it gets at the truth.

The style of reasoning dictates constraints on the truth and establishment of the sentences that it defines. The actual truth value of those sentences is external to the style: what is true in no way depends upon the style of reasoning. The truth does not depend on how we think. But that a certain complex sentence is a candidate for the truth may depend upon there being a style of reasoning, because there is no truth-or-falsehood in the matter, independent of the style of reasoning. There is not a prior truth, deeper, original, independent of reason, dwelling in the very interstices of the world, and which is discovered by reasoning ("correctly") according to some style. Nor do we discover the styles that then enable us to unearth and finally state the hitherto unstatable but pre-existing truth. The truth-or-falsehood and the style grow together. And as I shall show in Part II, this abstract metaphysics becomes rather modest common sense when we begin to look at a historical example.

This body of doctrine is not as relativist as it may sound. A style of reasoning, once in place, is not relative to anything. It does not determine the standard of objective truth. It is the standard.

1.1. META-CONCEPTS

I should begin by comparing and contrasting my idea of a style of reasoning with more familiar notions. We have just passed through a quarter century in which philosophical discussions about science were couched in terms of governing meta-concepts that are now disdained by the micro-sociologists. Kuhn's paradigms, Lakatos's programs, Holton's themata: extending our net beyond mere science, there are Foucault's discursive formations and Wittgenstein's language games. Do they have a common trait, aside from their flamboyant generality and abstractness? I shall discuss this under four heads, *possibility, exclusivity, historicity,* and *exemplification*.

Possibility. All the meta-concepts have to do with possibilities and constraints. All are Kantian. My "style of reasoning" is from the same bag. We should not give all the credit for this family of ideas to a faceless Kant. The more immediate filiation of Kuhn, Holton, and Foucault is with Alexandre Koyré. Koyré's conception of a dominating Platonic idea constraining the possibilities of thought and structuring experience had an extraordinary power on his audience and readers. Herbert Butterfield's bluff English metaphors, "picking up the other end of the stick" and "putting on a new thinking cap," capture some of Koyré's immediate attraction.[7] I know from discussions with him that Crombie's styles have the same filiation as do some of the ideas of Holton and Foucault.

Another meta-concept might seem in the offing here: "conceptual scheme." And so it is, in the sense in which one immediately takes the phrase. What is a conceptual scheme but an end of the stick, a thinking cap? But Quine preempted the phrase and took it to mean a set of sentences held for true. That is not in the realm of possibility but of actuality. If I were to try to reclaim the phrase for common usage, I would say that a conceptual scheme is a set of sentences that is up for grabs as true or false, a set of sentences that might be true. I think of a style as determining just such a set.

Exclusivity. Kuhn presented competing paradigms and Lakatos described rival programs as mutually exclusive, not as a matter of logic (as would be the case for incompatible Quinean schemes) but as a matter of thought and action. A generation after those authors, we are not in need of their hyperbole, and admit that a person can in a single mind entertain two "incommensurable" paradigms, and that a laboratory director may support two conflicting research programs within the same building. Nevertheless, competing paradigms or research programs do tend to drive each other out. They constrain possibilities: not only do they open doors, but they also close them. That is less true of Holton's themata. One can contemplate the possibility that the world is atomistic, and also that it is continuous, and apply one type of analysis to one problem, applying the other to a closely related difficulty. But in the grandest sense that Holton has in mind, themata do tend to be hegemonous, so that one cannot wholly subscribe to contrary themata: when Boyle believed that the world is made up of atoms and the void, a world of continuous variation and plenitude did not make sense to him.

Here is one respect in which my "styles" are quite different from more famous meta-concepts. Styles may have to fight to become established, but once they are mature and confident they do not even tend to exclude each other. The six that I have listed above are interwoven. They are not contraries but simply different, and can all be called upon in a single research project.

When it comes to possibility and constraint, the meta-concept of a language game is trickier than the others. Readers of Wittgenstein will quarrel little with the following unambitious remark: for speakers who participate in a language game, some speech acts are possible and some not; a language game has to do with what makes sense. Language games are the unstatable boundaries of the possible. Perhaps somebody has already called them the post-modern version of the scaffolding of which we read in the *Tractatus*. But I do not believe that from within the text of Wittgenstein himself we will find much support for the idea of competing language games.

Michel Foucault's sketched archaeology of knowledge is concerned with the way in which within a "discursive formation" certain sentences attain positivity, i.e., can be determined as true or false within the procedures authorized by the formation. My discussion of styles appropriates much Foucault, as I understand him. There is a big difference, too. His archaeology is self-consciously non-Braudelian.[8] His *epistemes* come into being and later perish at two moments of transformation. My styles are evolutionary, and might be with us evermore. Thus exclusivity is one dimension on which to range the meta-concepts under discussion, and my styles are the least exclusive of all.

Historicity. Aside from language games, all these meta-concepts historicize Kant. Georges Canguilhem was precise when he spoke of Foucault's "historical a priori." Paradigms and research programs are historical objects. To test the assertions of Kuhn or Lakatos, you must dig out some historical detail. Paradigms flourish for brief and glorious moments. By comparison Holton's themata seem timeless. Has not atomism been with us always? Yet Holton's interest is above all historical, to see how an ongoing theme is deployed in successive problem situations, in successive eras of science. Styles of scientific reasoning are equally historical entities, with a past, present, and future. They can also just die.

Wittgenstein alone had no lust for historicity. His language games are not in time. In this as in many other domains that vex contemporary philosophers, he is the odd man out, the restless Cartesian of the twentieth century. As part of the normal rewriting of our day, he must of course be historicized; people attempt this by appropriating his phrase "form of life" and regarding forms of life as historical entities. That is not Wittgenstein, but it does suggest another item to add to my roster of historiographic meta-concepts. Just as we have historicized Kant, so people are beginning to historicize Wittgenstein. Tim Lenoir does this in his discussion of German physiology in this volume, and David Bloor does so under the title "Left and Right Wittgensteinians."[9]

Exemplification, not definition. Paradigms, research programs, themata, discursive formations, language games, forms of life—these have been powerful words. They are used for vigorous metaphysical, epistemological, and methodological theses. But the concepts are never exactly defined. Instead their authors present plentiful examples together with some characteristics and differentia. I must now do the same for styles, begging for the same indulgence as my predecessors. I provide paradigmatic examples, illustrations, commentary, and application, but no precise definition.

1.2. STYLES

Crombie spoke of "styles of scientific thinking in the European tradition." I choose "reasoning" over "thinking" because I am more concerned with what is said than with what is thought. More importantly, reasoning is not, as I understand it, a purely sedentary art. It includes a lot of doing, not just arguing or thinking.

The pedigree of the phrase "style of thinking" is by no means immaculate. Precedents are found in the writings of memorable German thinkers, including Oswald Spengler, Edmund Husserl, and Ludwig Fleck. The English "style of thinking" is a term for intellectuals to toy with, whereas Fleck's term *Denkstil* was part of more common speech, loosely

employed, and in little need of definition. *Jüdisch Denkstil* was a handy epithet of the Nazis. At the time that I encountered Crombie's use of "style of thinking" I also found the phrase in essays by the high energy physicist and cosmologist Steven Weinberg, the linguist Noam Chomsky, and the historian Winifred Wisan.[10] During 1980, I read the draft of an enormous book by Crombie, *Styles of Scientific Thinking in the European Tradition*. It had six parts, corresponding to the six styles already mentioned, the fifth of which was statistical and probabilistic. The trajectory of each style was laboriously traced, with myriad citations, as far back as there are any records of "the European tradition."[11] Crombie's list of styles is, *ex cathedra,* the set of standard examples for the use of the word 'style', just as Holton's examples form the paradigms for 'themata'. We may want to divide, combine, recluster, or supplement his examples, but we know where to start.

I am not happy with the word 'style'. I would not want to hang anything on the distinctions about style made by critics of art or literature, or by practitioners of aesthetics. Nor should we be tempted to, for there is at least this great difference: the *style empire* or *Jugendstil* flourish for short periods of time after which they cease and can at most be imitated ("nurtured in tragedy and perished amid disaster" says my faithful *Encyclopedia* of the "empire style" of furniture, meaning that it began in the years of terror and ended with the defeat of Napoleon I). Crombie's styles are long-lived and cumulative. We live in a world where his six are inextricable from scientific enterprises. We could say that fads and fashions in apparel take the word 'style' in the direction of ultimate transience, while Crombie is chasing it in the opposite direction in the hope (which I do not share) of finding an irrevocable teleology.

There is a further disadvantage to the word, that we have the idea of personal style. We have management style, style when doing the Australian crawl. That leads at once to seedy jokes or tawdry irony. And what style of reasoning did Ronald Reagan use? What style are you using yourself? "O my! In this very paper you have given us a new style of reasoning!" Crombie's styles are completely impersonal, anonymous, just like Foucault's discursive formations. They became, like a language, there to be used, canons of objectivity. They were indeed formed and fixed in social traffic. We can find spokesmen for a style, a Hobbes or a Boyle, say, but we shall not find an author. We shall find authorities, but oddly enough, once the style is fixed, the experts get their authority from the style.

2: *The Styles of Statistics*

2.1. USES OF PROBABILITY

"Statistics" has three importantly different roles: descriptive, inferential, and modeling. We can simply enumerate and report how many of this, that, and the other fall under various classifications. That is description. Then there is statistical inference in which we reduce data, infer generalizations, or decide what is to be done in the light of data and goals. Thirdly, we build mathematical models, using the concept of probability, to represent some structure we dimly perceive in reality. The probabilities of modeling are often called objective probabilities; those of inference, inductive or subjective. They are inextricably related. Many types of statistical inference rely on probability models. In the old days, statistical descriptions were digests of enumerations and had little enough to do with modeling. In this century, they are derived from initial data by means of a host of inferential technologies.

If we include statistical descriptions, it is obvious that "the statistical style" is ancient. Since I shall be talking almost entirely about the nineteenth century, it is well to make this point clear. The fourth book of the Pentateuch is called Numbers because it is about a census of Israel. Numbering is about as deep in the Jewish and the Christian tradition as could be. King David commenced the Temple to atone for his census of Israel and Judah.[12] And Jesus was born in a manger because his parents were en route to be counted and taxed in their home town.[13] Statistical description is so common among civilizations that it can be called a universal of human governance, a product of those two other universals of orderly society, recruitment and taxation.

But does statistical description, the result of mere enumeration, deserve to be called "reasoning"? Yes. As I understand reasoning, it is not just sentential, not just thinking and mulling. The doing in the case of a census description is also extraordinarily complex. When General Joab numbered Israel and Judah he did not "observe" in the way we observe that there are three people in the corner, nor count as when we count the people in the room. He had to organize, choose marshalls, devise a coding method, make tests to find out which marshalls were faking numbers; every one of these activities was integral to the reasoning. The data were not passive, awaiting collection; they were moved, ordered, coerced. The operation took nine months and twenty days and, like many a modern census, gave incompatible answers.[14]

2.2. THE TRAJECTORY OF THE STATISTICAL STYLE

I am able to use the statistical style as an example because there have been so many recent and excellent publications on the topic. My own version of events, on which of course I draw in what follows, is to be found in *The Taming of Chance.*[15] In order to follow subsequent sections we nevertheless require here the briefest outline of the development of the statistical style. The story of Joab makes plain that it did not begin in 1820 or whatever. But it was of little importance—certainly unworthy of Crombie's canonization among the top six styles—until a sequence of events during the nineteenth and early twentieth century. My own periodization goes as follows. I hope that the dates are so exact that no one will take them seriously. They are markers. I shall very briefly explain my names.

1640–1693 the emergence of probability
1693–1756 the doctrine of chances
1756–1821 the theory of error, and moral sciences I.
1821–1844 the avalanche of printed numbers, and moral sciences II.
1844–1875 the creation of statistical objects
1875–1897 the autonomy of statistical law
1897–1933 the era of modeling and fitting.

My names are idiosyncratic but my periods are not. The avalanche of printed numbers coincides with Harald Westergaard's "Era of Enthusiasm" for statistics, 1820–1848. He emphasized that the events of 1848 drew to a close a period of fetishistic counting prompted by belief in utilitarian reform. Lorraine Daston rightly groups the period covered by my first four units as "Classical Probability" terminating about 1840. Stephen Stigler with equal good reason divides his "History of Statistics" in two at 1827, the death of Laplace. That conveniently separates what Stigler calls the "Gauss-Laplace synthesis," providing an essentially complete theory of errors, from the new era in which the problem was the assessment of information about mass phenomena, primarily social phenomena.[16]

Those authors have well explained the phenomenon they wish to highlight with their dates. I use signal events to indicate my interests. I have described the events around 1660 in *The Emergence of Probability.* In 1693 or thereabouts Bernoulli began the work that culminated in his celebrated theorem, the first central limit theorem—the next being due to De Moivre. In 1756 came the last edition of De Moivre's *Doctrine of Chances,* and the beginning of Lambert's studies of error. That is at the same time as the start of the rationalist conception of what were called moral sciences and whose noblest advocate was Condorcet. From the point of view of mathematics, this was the time of the theory of errors; but it was also the period of Enlightenment moral science. The year 1821 marks the first of the statistical publications about Paris and the Seine department. From then, or a little before, the printing of *public* statistics (as opposed to those privy to the government) ran rampant. Moral science and moral analysis became the names not of what we now call "rational choice theory" in the style of Condorcet, but of the statistics of moral deviancy, also called moral science, but in a completely new sense of the words, which is why I speak of moral sciences II, 1821–1844.

In 1844 Quetelet objectified the mean of a population. I shall describe this in a little detail below in the section titled "New Objects." In 1875 statistical laws were used not only to describe but also to explain phenomena, as I discuss under "New Explanations." By 1897 chance (as Peirce had written in 1892) was pouring in at every avenue of sense, Durkheim's *Suicide* to right and Mallarmé's most celebrated poem ("A throw of the die will never annul chance"), to left. Those great works were two culminations of 1897, which also marked the beginnings of Karl Pearson's chi-squared, published in full detail in 1900.

These periods single out a series of distinct stages in the fixation of the statistical style of reasoning. It is there that we find the material mesh with my metaphysics. This is because we can exhibit, without any exaggeration, the classes of sentences that became possible, sentences that had no clear sense, certainly no defined truth value, until the time span indicated. Very commonly, the sentences quite literally did not exist; neither they nor their translations are to be found among the entities uttered, inscribed, or even thought. When they had existed earlier, the conditions of their truth changed. I shall speak not in metaphors but with citations. Indeed, were I not propounding metaphysics, no one would think twice before assenting to my claim, "these are new sentences with new meanings, new truth-conditions, new objects, new classifications, and new criteria for verification."

2.3. NEW SENTENCES

It is a trifling matter to check that most statistical sentences on view in textbooks, laboratory handbooks and notebooks, internal company audits, research papers, gallup poll results, TV commercials, sports broadcasts, expert testimony on risk, fault trees, stockbroker's reports, sex manuals, parapsychology, agricultural gazettes, catalogues of quasar and other astrophysical objects, dispositions made by the World Health Organization, and the official statistics of every nation just did not exist at the beginning of the period under scrutiny, 1821. Not only were the sentences not uttered, but also they could not have been understood. We take for granted that most of the sentences are either true or false. No one will dispute the fact that sentences such as these were not inscribed in 1821. I urge that they did not have truth values. I do not mean that a sentence uttered now, say "the gross national product of Württemberg in 1817 was 76.3 million adjusted 1820 crowns" has no truth value. I mean that such a sentence uttered then would have had no truth value, not only because "gross national product" was not defined, but because there was no procedure of reasoning about the relevant ideas.

But surely some such sentences had truth values! For example, "the population of New York City in 1820 was 123,706" (as determined by the census). Of course, but I hope I have laid grounds for doubt above, in mentioning the biblical censuses. In America there was indeed a rather ramshackle procedure that led to that very sentence about New York in 1820, a procedure constituted in a rush with funds voted by Congress in the nick of time, and census takers hired entirely on grounds of political patronage. Forms were completed, knockers knocked, answers given, sums tallied, and the end product was this number, 123,706. We are inclined to say, that was the procedure, in which we have no exact trust. We conclude that the population was only somewhere in the neighborhood of 124,000. We do not and never will know the exact figure. But at least there was a fact of the matter, was there not?

There were some facts of the matter. For example, having recalculated many a sum I am completely confident that some errors of addition were made in the course of enumerating New York City. People "always" made mistakes; moreover, these mistakes in arithmetic would have been acknowledged then, had they been brought to anyone's attention (well, I ignore graft). The truth conditions for the arithmetical sentences had long been in place. That is less clear about the sentences stating the number of people. The population is not the number of living human bodies inside a certain perimeter at a certain moment, but rather the number of people who inhabit New York on a certain day. Transients are to be excluded—yet it will not even have been a fact, of many a person, that they were or were not "transient" until much later.

I do not believe that there is a sequence of sentences, "the population of New York on 1st January, 1820, was 100,000" [100,001, . . . , 159,999], exactly one of which was true. But surely I know the population was between 100,000 and 160,000! Of course. But that is not like saying that the number of people in this room is between 44 and 59. With those small numbers I believe that one sentence in the sequence "44 people in the room" {45 . . . 59} is true. The truth-conditions of "between 44 and 59" is a disjunction of the truth-conditions of "44 in the room" . . . "59 in the room," and each of those typically has definite truth-conditions defined by counting and the like. But none of the sentences "There were exactly *n* people in New York City, on 1st Jan., 1820" has such a truth condition.

It was for just such a reason that the economist J. B. Say urged, around 1820, that there is no such thing as the population of France. Of course, he had a political agenda as well. His position is no longer viable. But this is not because there is a number, the population of France on a certain day, that we have circumscribed with greater care than in 1820, and have better means of determining. We have, as it happens, almost entirely lost interest in total population. We direct attention to subgroups by age, origin, occupation, interest, income, and inclination. The standard view at present is that populations and subpopulations are not most accurately determined by exhaustive enumeration every decade, but by quite small stratified samples. The very idea of representative sampling was a novelty a century ago. But do we not at least know now that stratified sampling yields the most accurate estimates of subpopulations? The technique was introduced in the late 1930s, in the United States, when Jerzy Neyman was brought over from London in order to help work out a technique for cheaply avoiding errors in the census and the like.[17] And we now believe these techniques work well, i.e., are on the whole accurate. But what is the measure of accuracy? Correspondence with a true number known independently of any statistical method? No. Reliability itself determined statistically. We have probabilities of probabilities, or more sophisticated tools such as variance and confidence intervals.

I do not claim that this procedure is circular. There is no logical error that impugns its validity. On the contrary, it is precisely what is objective. It is what the statistical style teaches as valid assessment. This is bootstrapping. The statistical style of reasoning improves upon itself, where improvement is judged by its own standards. I believe that something like this happens with every style of reasoning. The self-authentification of the statistical style differs only in its unusual transparency.

2.4. NEW CLASSES

I have said that even the census sentences of the form "The number in class *C* is *n*" acquire truth value within a system of reasoning. That sup-

poses that the class *C* exists and is waiting to be counted. We must add to the doing in statistical reasoning the creation of new classes. In almost every decade, every country with an active statistical bureau produces a new set of classifications. These are, of course, partly a response to internal changes in the population or in the interests of authorities. New trades develop, new kinds of objects are owned, and new groups become "social problems" whose extent must be determined. Functionaries decide what classes shall be counted and how they shall be defined. Commonly they are well in advance of popular distinctions, rather literally bringing the distinctions into being. In an earlier era, Karl Marx read the statistical reports of the factory inspectorate and the like. It is a small joke that they, rather than he, contributed the most to class consciousness. For they devised the classes and obtained data from the mill-owners, who had not thought of their workers falling into those classes. Thereafter they and their workers conceived themselves as within that frame of work within the factory, and factories were redesigned and trade unions organized to accommodate these differences in the classes of employees.[18] If we see a style of reasoning as inseparable from the institutions that deploy it, we find new and complex relations between the style and the sentences that it brings into being.

2.5. NEW LAW-LIKE SENTENCES

Throughout the nineteenth century, German statisticians resisted the very existence of statistical laws of social groups, but these were readily embraced in the more atomistic and individualistic west. In France, it was laws of misbehavior that seemed to leap from the pages of official statistics. A whole range of phenomena now seemed subject to inexorable law, which had hitherto seemed the province of free choice: crime, suicide, and the like. That created a famous problem of statistical determinism.[19] Let us consider, however, not the mawkish question of freedom but continue with the arid matter of new laws of nature and society. Two new kinds of fact emerged: first, the number of suicides, sorted by region, age, sex, cause, health, marital state, social class, time of day, time of year, existence of suicide notes. Secondly, dispositions and regular tendencies to suicide according to the preceding classifications, expressed as probability distributions. Sentences able to state this second tier of "facts" came into being, in a new modality. They were the expression of social laws, laws hitherto unimagined.

The most fascinating laws (especially in France) were moral, but laws of other kinds of deviation, such as physical infirmity, may have had more consequence. We know exactly when and how and why some of these "came into being." Two sets of biological regularities had been well known from 1660, namely birth and death. These were spoken of in terms of law, the law of mortality, or the law that slightly more boys were born than girls. They were couched in terms of probability. Biostatistics stopped exactly there until 1825. In that year, a Select Committee of the House of Commons addressed the problem of sickness premiums for Friendly Societies, small mutual-benefit clubs of working men. As always at the start of a new class of sentences, there was immediate political motivation. The societies were suspect as covers for illegal Combinations (trade unions) but the primary interest was more philanthropic (viz. concerned with the worker's good but acting so as to maintain moral fiber and the social structure). What were actuarially sound sickness premiums? The national actuary John Finlaison asserted in testimony that "life and death are subject to a known law, but . . . sickness is not, so that the occurrence of the one may be ascertained, but not so the other."[20] He made clear that not only did he know no such laws, but that they do not exist. The Select Committee searched Europe for contrary belief but found none—except in one document compiled by the Highland Society of agricultural reformers in Scotland, completed in 1824. Suddenly it seemed that there could be laws of sickness, and the worlds of medicine and sickness insurance never looked back. Finlaison protested the Scottish figures had to be wrong, because the sickness rate was so much less than shown at musters of the British Army stationed at home—the beginning of a lesson not really enforced before that great statistical reformer Florence Nightingale. But probabilistic laws of disease did come into being almost literally at once; in the 1830s, medical studies were full of them. I do not here imply that disease had not secretly been following its allotted rates of spread, decline, and fall. I mean that law-like sentences about disease rates did not exist, and had even, by some of the most scrupulous observers, been excluded as stemming from a false analogy between disease and death.

I need hardly emphasize that the sequence of events is susceptible of almost every type of microsociological analysis ever proposed. At the banal level of interests of an overt material or political sort, we find the entrenched concerns of employers fearing strikes, the military fearing exposure for incompetence, the utilitarians seeking stability in the laboring force, the resistance of the insurance companies to reduced premiums, and so on and on. In Latour's account of actants, networks, and alliances, we are particularly struck by the alliance between specifically Scottish working men, with their concerns for good health, and the London administrators. The story can be elaborated for chapters. Here we find the proximate causes of the emergence of a new type of medical law. Nevertheless, once this kind of law is in place, it is largely independent of the proximate causes, and becomes a new standard of objective fact of which we can have objective knowledge. The same remark may be made at the end of each of the following sections; I shall not repeat it.

2.6. NEW OBJECTS

The coordinates of an archipelago, the position of a planet at a moment, the velocity of light in a vacuum, the atomic weight of an isotope of chlorine, the gravitational constant: given a scale for measurement, these are all definite numbers given (or so we say when not in a skeptical mood) by nature. The theory of errors was devised for such quantities. Its immediate application is a theory about the best estimate, based on a number of slightly discrepant measurements, and a measure of the dispersion of the readings. In the standard theory, for which Gauss and others provided elegant motivations, the best estimate is usually the mean and a measure of dispersion (the probable error, or, later, the standard deviation) is used as an indicator of accuracy. All this was in place by the early 1800s, and all subsequent theory of error is only a set of more or less ingenious footnotes to the work of Gauss and Laplace.

Tables of deviancy seemed to show that averages—of conviction rates for crimes against the person, of suicides classified according to region, season, sex, and method—were pretty constant. The average was the arithmetic mean, but it was not "natural" to transfer the theory of error to social statistics. Scholars have asked why it took so long, but they have done so only because it later *seemed* "natural."[21] The former manifest difference between geodesy or astronomy, on the one hand, and social or biometric statistics, on the other, was simply erased. That was in 1844.

In a few brief pages published that year, Adolphe Quetelet gave three examples. First he tabulated readings at Greenwich, followed by computations illustrating the mean and the probable error. Secondly, he suggested that if one could on plausible nonstatistical grounds divide a set of readings into two groups, with different means and smaller probable errors, one could conjecture that there were two distinct quantities under measurement.[22] Thus one would find that the readings were not homogeneous. Third, he directed us to measurements made on a large number of different individuals, to wit, the chest diameters of 5,738 Scottish soldiers. These are so distributed about their average, he said, that it is just as if one typical Highlander had been measured by an incompetent tailor with a definite probable error. The distribution is that of the law of error, or, as we say now, the normal distribution or bell-shaped curve. At that time he had so few data upon which to draw for purposes of illustration that he used a digest, prepared perhaps by a student, of a summary of information collected by a contractor and published in Edinburgh over a quarter century before.[23] Before Quetelet's kind of inquiry, such data were of scant anecdotal interest.

Quetelet brought into being a new kind of object: not the average of the diameters of these 5,738 chests, for that type of number had been around for quite some time. His new object was the population characterized by a mean and a standardized dispersion. The mean and dispersion are now thought of as objective properties of some part of the world, as "out there" as the location of a planet. Conversely, the old-fashioned kind of population—Scotsmen, or Highland crofters, or whatever—was replaced or at any rate paralleled by a more abstract concept of a group of individuals whose attributes are represented by the law of error. Populations can be split into two more homogeneous lots by the formal technique of distinguishing two means and smaller dispersions around each mean. Quetelet was not a eugenicist, but one of his immediate aims was the characterizing of the subpopulations of Europe, groups that would not necessarily be separated by any traditional boundary.

Philosophical talk of creating new objects, populations and phenomena is tricky. There is a spectrum of philosophical opinion. To start at one end, consider the population of "homeless" camped on the streets of major American cities. Whatever its causes, this population is a distinct one that did not exist a decade ago, even if its members were mostly members of other populations that shifted to this one. There is no hint of nominalism in saying that this is newly created. Moving along the spectrum, I myself am happy to say that people created lasers and also the phenomenon of lasing—nothing lased until people made it do so. Many more conservative philosophers of science resist what I say, but such statements do not reveal me as a closet constructionalist. Going further along the spectrum, some have said that a new object, the solar system, and its center, the sun, came into being after Copernicus. That is clearly a more radical use of "new object" than my commonplace and commonsense remark about lasers. And there are more radical versions of "new object" than the remark about the sun, versions that tend toward what I once called linguistic idealism.[24]

Where, on this spectrum of philosophical radicalism, should we place remarks about the mean of an attribute of a social or biological population? To say that it is a new kind of object, presented to the world in 1844, is to be more radical than to say that lasers are a new kind of object, and lasing a new kind of phenomenon. It is less radical than saying that the solar system and the sun were new kinds of objects. They were, to a conservative mind, old objects reclassified. That is not true of the new kind of population and its statistical parameters. There were no such objects under *any* description.

Quetelet's move in 1844 was a decisive advance for the statistical style of reasoning, because it created discourse about a new class of entities and their measurements. This discourse could not exist without the importation of probabilities and the Gaussian law of error. Had it not been for this move, there might have been no such thing as Crombie's "the statistical analysis of populations and the calculus of probabilities." There might have been two distinct things, statistics and probability.

2.7. NEW EXPLANATIONS

In daily life, we commonly try to explain unexpected events or puzzling occurrences. The target is the particular. In the sciences we explain phenomena, what happens or can be made to happen as a rule. There are overlaps, as when a particular surprise is shown to be an instance of an explicable phenomenon, and also when the phenomenon is singular, as the extinction of the dinosaurs. Philosophers of science have recently been sidetracked into analyses of explanation of the particular. They ponder puzzles that arise when an individual event falls under a merely statistical law. Here I attend instead only to "scientific" explanation of a phenomenon by showing how it arises from known laws or facts of a general kind.

The statistical style furnished no such statistical explanations until 1875. There were descriptions and predictions. There was the trap of statistical determinism and the associated devolution of morality. If Tom Gradgrind is one of a class of whom a fixed law-like proportion steal, was not there bound to be theft by many of these miscreants? Is not Tom's crime explained and thereby excused? Such were the preoccupations of the 1850s, made memorable in Dickens's parodying, in *Hard Times*, of the statistical style or "S-s-stutterers" as Cissy called it.

Galton furnished the first statistical explanation of a phenomenon (as opposed to a singular fact). Donald MacKenzie wrote one of the first, and best, "social construction of scientific knowledge" books around the

Galtonian tradition of biometrics.[25] That reminds us once again how the proximate causes of an event in the trajectory of a style of reasoning are subject to social analysis. Here I attend to a feature slightly different from that studied by MacKenzie. Galton knew that gifted and retarded families produce unusual offspring, but also that children of the outstanding parents tend to be less exceptional than their parents. This "regression towards mediocrity," as he called it (both from above and from below) was a phenomenon that he established by descriptive statistics. It baffled him—until he showed that it was a mathematical consequence of the fact that the relevant attributes in the population were distributed according to the law of error. As we now say, regression toward the mean is deducible from the supposition that the population has a normal distribution. Many more explanations followed. The statistical style had created a matrix from which a whole generation of new, complex, sentences would be born. The sentences that expressed the explanations simply did not exist until the person whom we call Quetelet had brought into being discourse about these new objects, populations with means, dispersions. The use of these sentences in the sentence or sentence sequence of the form "explanandum explains explanans" required further developments which we call the invention of regression analysis. A few years later correlations were added to the body of technique that could, among other things, furnish a new kind of explanatory paragraph.

2.8. NEW CRITERIA

Few of the populations studied by Quetelet "really" had a normal distribution. Like all other styles the statistical one leap-frogged along on the backs of propaganda-exemplars based on optimism, error, exaggeration, or sometimes deceit. Why do I speak of error? Because we now have criteria for goodness of fit that became settled at a later state in the trajectory of the statistical style. Quetelet's table of Scottish girths only very roughly fit the normal distribution, and his other examples tend to be worse. Quetelet did have a technique for comparing an empirical distribution (say of heights of Union soldiers in the American war) with a mathematical curve. But it was a matter of comparison, not of testing. Good tests (by modern criteria) did develop in Germany around 1875 at the hand of the economist Lexis and others. These were long ignored by the Anglo-Americans, who thought that statistical laws had the form of equations containing a number of constants, called parameters, that were fixed by nature. For example, the mean of a normal distribution, and a measure of dispersion, determine precisely the normal law attributed to a phenomenon. The German measures in contrast were typically nonparametric (in today's terminology) because the Germans did not believe in the new objective laws of what they called *Queteletismus*. One of their aims, in line with their total skepticism about statistical law, was to show how much irregularity is to be found in empirical statistics. The ecological niche for nonparametric theory was located in Eastern Europe, in Berlin, and St. Petersburg. London was the locale for parametric approaches.

Parametric theory—that is, much standard "Anglo-American statistics" until recently—can be regarded in two ways, realistic and positivist. The realist says that in describing a population as normally distributed, with mean mu and variance sigma, one is making exactly the same sort of statement as in saying that the latitude of the tip of the archipelago is 57° 37′ 26″ S, or that the atomic weight of chlorine is 34.651. The positivist view is that one is only representing the population *as if* its attributes were the product of a stochastic device whose results are normally distributed. The difference was not felt keenly. Those who were inclined to the positivist understanding were positivist about all quantities—Karl Pearson, for example. In either interpretation, statistical hypotheses—an increasingly well-defined class of sentences—stated that certain quantities associated with a population were distributed according to a mathematical law in which there were free parameters such as the standard deviation or the correlation coefficient. The statisticians saw themselves as estimating these unknown parameters, assessing the fit of an empirical distribution to a family of curves, of testing the significance of a treatment upon a population whose distribution and range of parameters was specified in a model, and, in the late stages, as assessing the "operating characteristics" of a decision procedure for accepting or rejecting statistical hypotheses. All of these procedures were themselves couched in terms of probabilities. In broad outline, the ideas can be perceived in earlier writings—the theory of confidence-intervals in Laplace or A. A. Cournot (1843) for example. But the self-conscious general application of the ideas came later: the conditions of assertibility of statistical hypotheses are themselves to be determined by using the statistical style of reasoning, and in terms of yet a new layer of sentences that themselves are statistical.

Thus theories of statistical testing and estimation conform all too readily to my thesis about statistical language, statistical truth, and statistical reason. I claim that testing procedures, which provide criteria for the acceptance and rejection of statistical hypotheses and statistical models, determine the meaning of the sentences expressing the hypotheses and the models. This is an overt verification theory of the meaning of statistical sentences. I did not inaugurate it: it is full-fledged in R. B. Braithwaite's *Scientific Explanation*. I did follow that approach in *Logic of Statistical Inference* a quarter century ago.[26] There is, then, the danger that my example of a style of reasoning will be defeated by its own success. "Yes, of course we see the introduction of new criteria for the assertibility of statistical statements, and that assertions about probability are themselves assessed using probabilities. The statistical style has indeed evolved by bootstrapping, but the example is unique!" Such are the risks of proving a point. I do not claim that the argument will go in the same way for Crombie's other styles of reasoning. I claim that it will go in different ways, which require as much detailed elaboration for those cases as the ones that I draw upon for the present essay.

2.9. NEW INTERSUBJECTIVITY

Within an objective theory of probability such as that favored by Jerzy Neyman, there is only one type of meaning for statements of probability, whether they occur in assessments of a method of testing ("the probability of wrongly accepting an hypothesis is only 0.05") or in statements about a part of the world under investigation ("the probability of infection after three weeks decreases to 0.05"). The meaning may be the same (for example, on a verification theory, always given in terms of meta-statements of acceptance and rejection) but the use and role is very different. The infection statement tells us something about transmission of disease; the statement about the test seems to tell us something about the test. But its role is primarily not to say something about this test but to provide a protocol for the intersubjective comparison of tests. Reflect on the litany of statistical chants, "5 percent significance level," "95 percent confidence," "P value in a chi-squared test of 3," and what the newspapers now give us in reporting opinion polls, "These results are considered to be accurate to within 3 percentage points 19 times out of 20." The last is closest to plain English, but people are too scared to ask what it means. It is not literally true that if this poll were taken many times, 19 out of 20 of the results would be within 3 percent of the true value (whatever "true value" is supposed to mean). Instead, these numbers indicate that a general protocol has been used, and provide a method for qualitative interpoll comparisons.

A technology of intersubjectivity has come into play. I suspect that this is a uniform feature of all styles of reasoning, although the protocols of intersubjectivity will differ at different epochs within one style, and will be different for different styles. The difference for different styles is perhaps a tautology: we could use the mode of intersubjectivity to characterize the style. We have proof procedures in mathematics, experimental controls in the laboratory sciences. In statistics we have these measures that are essential to legitimation, to publication, to authority: the confidence level, for example.

The technology of statistical intersubjectivity is now, if not hard-wired, thoroughly softwared. You buy a program such that if you use it, and do not cheat elsewhere, you are assessing data in an objective way. That is now what it means to be objective about statistical data. I am not denying that this is objective. To do so (as some, alas, do) is to suggest that there is some other standard of objectivity for statistical sentences. On the contrary, these sentences get their meaning precisely within this technology of objectivity.

I shall not pause to consider conceptual disagreements about the foundations of statistics, fierce battles between "Bayesians" and "orthodox statisticians." This is often presented as a difference between subjective and objective ideas about probability. Do they matter? It is true that different schools will give you different advice about how to design experiments, but for any given body of data they agree almost everywhere. The disputes are of measure zero in the space of intersubjective analysis of actual data. There is an important story to be told here about the ways in which foundational issues have been used to mask the regimentation of reason that is so characteristic of that great epistemological and metaphysical success story of our century, the calculus and language of probabilities. But that philosophical exercise demands both mathematical and historical exposition beyond the scope of this discussion. I shall only warn against words. The existence of a subjective school may seem to count against my claim that statistics has provided criteria of objectivity. In fact, the most dogmatic statisticians have been Bayesian. After the Second World War some writers, such as the French mathematician Allais, saw adolescent Bayesianism as the march of American stormtroopers across the European mind.[27] Where "objective" statisticians, such as that authoritarian giant R. A. Fisher, said that statistics could only present objective comparisons of data, leaving free-thinkers to judge as they wanted, the subjective theory was explicitly intended by that mild and generous soul, L. J. Savage, as a mode for disciplining your own mind.

2.10. THE TASK RECALLED

We should try to connect, I said at the beginning, social studies of knowledge, metaphysics, and the Braudelian aspects of science. Styles are Braudelian. The seeds of statistical analyses of population are as ancient as counting; I took biblical examples only for reasons of familiarity. Since 1821, there has been a lot of activity on the probability-and-statistics front, yet in earlier writings I found it entirely natural to describe even that in geological terms ("the avalanche of numbers," yes, but also "the erosion of determinism"). When we look at any individual incident we shall see a mass of social detail to which I have merely alluded (e.g., the worry about weekly premiums for Friendly Societies of working men). We also see certain global characteristics (e.g, the fascination with deviancy and the drive towards not just normalization but the very concept of normalcy). There is also a phenomenon of different statistical ideas developing in different "ecological niches"—compare Paul Forman's famous thesis that Weimar Germany was peculiarly suited to the advent of the new quantum mechanics. I have illustrated at length in *The Taming of Chance* how French culture was curiously receptive to the idea of statistical law, while this was thoroughly resisted in Prussia—a fact of great importance, once, to the sociology of statistical knowledge, but now irrelevant to modern statistical practice.

A style of reasoning becomes largely independent of all these early proximate causes of different sorts, ranging from ecological niches spanning more than half a century of development to meetings of a Parliamentary committee lasting a couple of weeks. The extent to which a style retains the more global characteristics associated with its maturation is a matter for open and ongoing inquiry. Is statistical thought intrinsically dedicated to normalization and control of people? Must it be so, as part of a historical a priori resulting from its initial conditions of possibility? Those are questions for future reflection.

The intersection of the proximate social causes and the Braudelian thrust of a style of reasoning should not, I think, create any philosophical perplexity. How could things be otherwise, if there is any stability at all in our patterns of reasoning? Little incidents and global needs bring ideas into being. Certain of them become part of the fabric of our thought, our very canons of objectivity. That is not surprising.

I have been driven to provocative statements only at the level of metaphysics. There the interplay between the proximate social causes and the long-range organization of reasoning becomes vital. This is because I claim that sentences get truth-conditions at definite moments of time, and those moments are the product of the social. At the same time, those sentences and their modes of verification become taken for granted within the grand march of the style of reasoning. As soon as one starts talking in this way, or in my annoying section titled "Metaphysics" early in the essay, one seems on the edge of speculative gobbledygook. On the contrary, I have illustrated how all the talk about styles and sentences and truth-

conditions and verification-theory-of-meaning is to be taken in a literal and unadventuresome way. The assertion that the statistical style of reasoning is self-authenticating turns out to be correct, a rather old-fashioned conclusion given heightened significance.

Is the result some kind of "relativisim" about truth and styles of reasoning, some kind of "anti-realism"? No. That by which we investigate reality is not relative to anything, and the aspects that we call the real determine what is true or false according to our criteria. Yet our styles and our truths do not exist until we bring them into being. Objectivity is not the less massive, impenetrable, resistant, because it is the product of our history. But when we get close enough to run our hands across this rock—or rather, conglomerate—we shall feel its fissures and notice how different is its texture from that smooth surface that we seem to observe from afar, before we attend to the innumerable details that are its only origin and which constitute its substance.

NOTES

1. I owe the Braudel metaphor to Peter Galison, *How Experiments End* (Chicago: Chicago University Press, 1987), 246.

2. The title of the present paper alludes to Ian Hacking, "Language, Truth and Reason," in M. Hollis and S. Lukes, *Rationality and Relativism* (Oxford: Blackwell, 1982), 48–66. See "The Accumulation of Styles of Scientific Reasoning" in Dieter Henrich (ed.), *Kant Oder Hegel* (Stuttgart: Klett-Cotta, 1983), 453–462. These two papers are combined and condensed in "Styles of scientific reasoning," in John Rajchman and Cornell West (eds.), *Postanalytic Philosophy* (New York: Columbia University Press, 1985), 145–163.

3. A. C. Crombie, "Philosophical presuppositions and shifting interpretations of Galileo," in J. Hintikka, D. Gruender, and E. Agazzi (eds.), *Theory Change, Ancient Axiomatics and Galileo's Methodology, Proceedings of the 1978 Pisa Conference on the History and Philosophy of Science* (Dordrecht: Reidel, 1981), vol. I, p. 284. He has more recently published an account of the concept of style in general and of these six styles in particular in "Designed in the mind," *History of Science* 26 (1988): 1–12.

4. I. Hacking, *Representing and Intervening* Cambridge: Cambridge University Press, 1983), 145. My discussion of observation, alluded to in the preceding paragraph above, is found in chs. 10 and 11.

5. J. L. Austin, "Truth" and "Unfair to Facts," in his *Philosophical Papers* (Oxford: Clarendon Press, 1961), 85–122.

6. George Lakoff, *Women, Fire and Dangerous Things: What Categories Teach About the Human Mind* (Chicago: Chicago University Press, 1986).

7. Herbert Butterfield, *The Origins of Modern Science* (London: Bell, 2nd ed., 1957), 1–4. The book is based on lectures at Cambridge commenced in 1948, and is written under the spell of Koyré.

8. *The Archaeology of Knowledge* (London: Tavistock, 1972) begins by saying just that, on pp. 3–21 of the English edition.

9. Michael Lynch, "Extending Wittgenstein: The Pivotal Move from Epistemology to the Sociology of Science" and David Bloor, "Left- and Right-Wittgensteinians," in Andrew Pickering, *Science as Practice and Culture* (Chicago: Chicago University Press, 1991).

10. For references to these authors, see my first paper cited in note 2 above.

11. The book took longer to complete than expected, but from July 1990 has been with the publishers (Duckworth, London), in three volumes. I am very glad of this, for Crombie has fleshed out the idea of a style with a rich plenitude of historical example.

12. He had been "moved" or "provoked" to take the census by either God or Satan (depending on whether you read 2 Sam. 24:1 or 1 Chron. 21:1). On either story, God was furious with David, and offered him a choice of (a) 3 months famine, (b) 3 months of constant defeat by enemies or (c) 3 days of pestilence. David chose the third as the least evil, but after the plague had killed 70,000 men, God repented and made David build the Temple. The "Satan" of the King James Bible is for the difficult Hebrew word also rendered "adversary."

13. The "taxed" of the Authorized Version is better rendered "enrolled," which means counted and taxed; Augustus wanted not only revenue, but also a head count for military purposes.

14. How many men able to bear arms? 800,000 in Israel and 500,000 in Judah? (2 Sam. 24:9). Or 1,100,000 in Israel and 470,000 in Judah (1 Chron. 21:5). It does not help that in Chronicles we are told that Joab refused to count two tribes, Levi and Benjamin. The Chronicles numbers obviously cannot be the Samuel numbers, less Levi (in Israel) and Benjamin (in Judah). The same census was referred to. The disagreement does not show that the Bible is inconsistent but that the reasoning around a census is tricky.

15. Ian Hacking, *The Taming of Chance* (Cambridge: Cambridge University Press, 1990). For a collective study, containing work by authors of individual books cited below, see Lorenz Krüger et al. (eds.), *The Probabilistic Revolution*, 2 vols., (Cambridge, Mass.: MIT Press, 1987).

16. Harald Westergaard, *Contributions to the History of Statistics* (London: P. S. King, 1932). Stephen Stigler, *The History of Statistics: The Measurement of Uncertainty before 1900* (Cambridge, Mass: Belknap Press, 1986). Lorraine Daston, *Classical Probability in the Enlightenment* (Princeton: Princeton University Press, 1988).

17. See Margo Conk (Anderson), "The 1980 Census in Historical Perspective," in William Alonso and Paul Starr (eds.), *The Politics of Numbers* (New York: Russell Sage Foundation, 1987), 155–186.

18. I began to argue this in "Biopower and the Avalanche of Printed Numbers," *Humanities in Society* 5 (1982): 272–295. A much more detailed examination of the effects is given by Alain Desrosières and Laurent Thevenot, *Les Catégories socioprofessionelles*, (Paris, 1988), and other work by these two authors.

19. See Theodore M. Porter, *The Rise of Statistical Thinking, 1820–1900* (Princeton: Princeton University Press, 1986), ch. 6, and Ian Hacking, *The Taming of Chance* (Cambridge: Cambridge University Press, 1990), chs. 14, 15.

20. Full information is found in my *Taming*, ch. 5.

21. Hilary Seal, authority on the mathematics of error, is an example of someone who presents the slow transfer as a mystery. See his essay, "The Historical Development of the Gauss Linear Model," reprinted in E. S. Pearson and M. J. Kendall (eds.), *Studies in the History of Statistics and Probability* (London: Charles Griffin, 1970), vol. 1, 207–230.

22. Quetelet actually is closer to the case of social statistics than my statement here suggests.

23. The digest is reproduced and recalculated by Stigler, pp. 207–8. The original summary was intended to show that different regions of Scotland have different biometric characteristics, that is, that the population was inhomogeneous—that is why I charitably suppose that Quetelet never saw the data, *Taming*, ch. 13.

24. In *Why Does Language Matter to Philosophy?* (Cambridge: Cambridge University Press, 1975), 182. The phrase has since been used by both G. E. M. Anscombe and Hilary Putnam with slightly different meanings.

25. Donald MacKenzie, *Statistics in Britain, 1865–1930: The Social Construction of Scientific Knowledge* (Edinburgh: Edinburgh University Press, 1981).

26. R. B. Braithwaite, *Scientific Explanation* (Cambridge: Cambridge University Press, 1953). I. Hacking, *Logic of Statistical Inference* (Cambridge: Cambridge University Press, 1965). Isaac Levi was the first to show me, many years ago, how incredibly "verificationist" these books are.

27. I shall not cite other twentieth-century statistical works referred to, such as those by Savage or Fisher, on the grounds that they are well-known to philosophers, but it is worth giving the title of one of the most vigorous critical papers here: Maurice Allais, "Le comportement de l'homme rationnel devant le risque: Critique des postulats et axiomes de l'école americaine," *Econometrica* 21 (1953) 503–546.