# 1

# The argument

The most decisive conceptual event of twentieth century physics has been the discovery that the world is not deterministic. Causality, long the bastion of metaphysics, was toppled, or at least tilted: the past does not determine exactly what happens next. This event was preceded by a more gradual transformation. During the nineteenth century it became possible to see that the world might be regular and yet not subject to universal laws of nature. A space was cleared for chance.

This erosion of determinism made little immediate difference to anyone. Few were aware of it. Something else was pervasive and everybody came to know about it: the enumeration of people and their habits. Society became statistical. A new type of law came into being, analogous to the laws of nature, but pertaining to people. These new laws were expressed in terms of probability. They carried with them the connotations of normalcy and of deviations from the norm. The cardinal concept of the psychology of the Enlightenment had been, simply, human nature. By the end of the nineteenth century, it was being replaced by something different: normal people.

I argue that these two transformations are connected. Most of the events to be described took place in the social arena, not that of the natural sciences, but the consequences were momentous for both.

Throughout the Age of Reason, chance had been called the superstition of the vulgar. Chance, superstition, vulgarity, unreason were of one piece. The rational man, averting his eyes from such things, could cover chaos with a veil of inexorable laws. The world, it was said, might often look haphazard, but only because we do not know the inevitable workings of its inner springs. As for probabilities – whose mathematics was called the doctrine of chances – they were merely the defective but necessary tools of people who know too little.

There were plenty of sceptics about determinism in those days: those who needed room for freedom of the will, or those who insisted on the individual character of organic and living processes. None of these thought for a moment that laws of chance would provide an alternative to strictly causal laws. Yet by 1900 that was a real possibility, urged as fact by an adventurous few. The stage was set for ultimate indeterminism. How did that happen?

This is not a question about some sort of decay in knowledge or management. The erosion of determinism is not the creation of disorder and ignorance – quite the contrary. In 1889 Francis Galton, founder of the biometric school of statistical research, not to mention eugenics, wrote that the chief law of probability 'reigns with serenity and in complete effacement amidst the wildest confusion'.[1] By the end of the century chance had attained the respectability of a Victorian valet, ready to be the loyal servant of the natural, biological and social sciences.

There is a seeming paradox: the more the indeterminism, the more the control. This is obvious in the physical sciences. Quantum physics takes for granted that nature is at bottom irreducibly stochastic. Precisely that discovery has immeasurably enhanced our ability to interfere with and alter the course of nature. A moment's reflection shows that a similar statement may be attempted in connection with people. The parallel was noticed quite early. Wilhelm Wundt, one of the founding fathers of quantitative psychology, wrote as early as 1862: 'It is statistics that first demonstrated that love follows psychological laws.'[2]

Such social and personal laws were to be a matter of probabilities, of chances. Statistical in nature, these laws were nonetheless inexorable; they could even be self-regulating. People are normal if they conform to the central tendency of such laws, while those at the extremes are pathological. Few of us fancy being pathological, so 'most of us' try to make ourselves normal, which in turn affects what is normal. Atoms have no such inclinations. The human sciences display a feedback effect not to be found in physics.

The transformations that I shall describe are closely connected with an event so all-embracing that we seldom pause to notice it: an avalanche of printed numbers. The nation-states classified, counted and tabulated their subjects anew. Enumerations in some form have been with us always, if only for the two chief purposes of government, namely taxation and military recruitment. Before the Napoleonic era most official counting had been kept privy to administrators. After it, a vast amount was printed and published.

The enthusiasm for numerical data is reflected by the United States census. The first American census asked four questions of each household. The tenth decennial census posed 13,010 questions on various schedules addressed to people, firms, farms, hospitals, churches and so forth. This 3,000-fold increase is striking, but vastly understates the rate of growth of printed numbers: 300,000 would be a better estimate.

The printing of numbers was a surface effect. Behind it lay new technologies for classifying and enumerating, and new bureaucracies with the authority and continuity to deploy the technology. There is a sense in which many of the facts presented by the bureaucracies did not even exist ahead of time. Categories had to be invented into which people could conveniently fall in order to be counted. The systematic collection of data about people has affected not only the ways in which we conceive of a society, but also the ways in which we describe our neighbour. It has profoundly transformed what we choose to do, who we try to be, and what we think of ourselves. Marx read the minutiae of official statistics, the reports from the factory inspectorate and the like. One can ask: who had more effect on class consciousness, Marx or the authors of the official reports which created the classifications into which people came to recognize themselves? These are examples of questions about what I call 'making up people'. This book touches on them only indirectly.[3]

What has the avalanche of printed numbers to do with my chief topic, the erosion of determinism? One answer is immediate. Determinism was subverted by laws of chance. To believe there were such laws one needed law-like statistical regularities in large populations. How else could a civilization hooked on universal causality get the idea of some alternative kind of law of nature or social behaviour? Games of chance furnished initial illustrations of chance processes, as did birth and mortality data. Those became an object of mathematical scrutiny in the seventeenth century. Without them we would not have anything much like our modern idea of probability. But it is easy for the determinist to assume that the fall of a die or the spin of a roulette work out according to the simple and immutable laws of mechanics. Newtonian science had no need of probabilities, except as a tool for locating underlying causes. Statistical laws that look like brute, irreducible facts were first found in human affairs, but they could be noticed only after social phenomena had been enumerated, tabulated and made public. That role was well served by the avalanche of printed numbers at the start of the nineteenth century.

On closer inspection we find that not any numbers served the purpose. Most of the law-like regularities were first perceived in connection with deviancy: suicide, crime, vagrancy, madness, prostitution, disease. This fact is instructive. It is now common to speak of information and control as a neutral term embracing decision theory, operations research, risk analysis and the broader but less well specified domains of statistical inference. We shall find that the roots of the idea lie in the notion that one can improve – control – a deviant subpopulation by enumeration and classification.

We also find that routinely gathering numerical data was not enough to make statistical laws rise to the surface. The laws had in the beginning to be read into the data. They were not simply read off them. Throughout this book I make a contrast of a rough and ready sort between Prussian (and other east European) attitudes to numerical data, and those that flourished in Britain, France, and other nations of western Europe. Statistical laws were found in social data in the West, where libertarian, individualistic and atomistic conceptions of the person and the state were rampant. This did not happen in the East, where collectivist and holistic attitudes were more prevalent. Thus the transformations that I describe are to be understood only within a larger context of what an individual is, and of what a society is.

I shall say very little about mathematical conceptions of probability. The events to be described are, nevertheless, ingredients for understanding probability and for grasping why it has been such an incredible success story. Success story? A quadruple success: metaphysical, epistemological, logical and ethical.

Metaphysics is the science of the ultimate states of the universe. There, the probabilities of quantum mechanics have displaced universal Cartesian causation.

Epistemology is the theory of knowledge and belief. Nowadays we use evidence, analyse data, design experiments and assess credibility in terms of probabilities.

Logic is the theory of inference and argument. For this purpose we use the deductive and often tautological unravelling of axioms provided by pure mathematics, but also, and for most practical affairs, we now employ – sometimes precisely, sometimes informally – the logic of statistical inference.

Ethics is in part the study of what to do. Probability cannot dictate values, but it now lies at the basis of all reasonable choice made by officials. No public decision, no risk analysis, no environmental impact, no military strategy can be conducted without decision theory couched in terms of probabilities. By covering opinion with a veneer of objectivity, we replace judgement by computation.

Probability is, then, *the* philosophical success story of the first half of the twentieth century. To speak of philosophical success will seem the exaggeration of a scholar. Turn then to the most worldly affairs. Probability and statistics crowd in upon us. The statistics of our pleasures and our vices are relentlessly tabulated. Sports, sex, drink, drugs, travel, sleep, friends – nothing escapes. There are more explicit statements of probabilities presented on American prime time television than explicit acts of violence (I'm counting the ads). Our public fears are endlessly debated in terms of probabilities: chances of meltdowns, cancers, muggings, earthquakes, nuclear winters, AIDS, global greenhouses, what next? There is nothing to fear (it may seem) but the probabilities themselves. This obsession with the chances of danger, and with treatments for changing the odds, descends directly from the forgotten annals of nineteenth century information and control.

This imperialism of probabilities could occur only as the world itself became numerical. We have gained a fundamentally quantitative feel for nature, how it is and how it ought to be. This has happened in part for banal reasons. We have trained people to use numerals. The ability to process even quite small numbers was, until recently, the prerogative of a few. Today we hold numeracy to be at least as important as literacy.

But even compared with the numerate of old there have been remarkable changes. Galileo taught that God wrote the world in the language of mathematics. To learn to read this language we would have to measure as well as calculate. Yet measurement was long mostly confined to the classical sciences of astronomy, geometry, optics, music, plus the new mechanics. T.S. Kuhn has iconoclastically claimed that measurement did not play much of a role in the 'Baconian' sciences that came to be called chemistry and physics.[4] He urged that measurement found its place in physics – the study of light, sound, heat, electricity, energy, matter – during the nineteenth century. Only around 1840 did the practice of measurement become fully established. In due course measuring became the only experimental thing to do.

Measurement and positivism are close kin. Auguste Comte coined the word 'positivism' as the name of his philosophy, holding that in all the European languages the word 'positive' had good connotations. His own philosophy did not fare especially well, but the word caught on. Positive science meant numerical science. Nothing better typified a positive science than a statistical one – an irony, for Comte himself despised merely statistical inquiries.

The avalanche of numbers, the erosion of determinism, and the invention of normalcy are embedded in the grander topics of the Industrial Revolution. The acquisition of numbers by the populace, and the professional lust for precision in measurement, were driven by familiar themes of manufacture, mining, trade, health, railways, war, empire. Similarly the idea of a norm became codified in these domains. Just as the railways demanded timekeeping and the mass-produced pocket watch, they also mandated standards, not only of obvious things such as the gauge of the lines but also of the height of the buffers of successive cars in a train. It is a mere decision, in this book, to focus on the more narrow aspects that I have mentioned, a decision that is wilful but not arbitrary. My project is philosophical: to grasp the conditions that made possible our present organization of concepts in two domains. One is that of physical indeterminism; the other is that of statistical information developed for purposes of social control.

This study can be used to illustrate a number of more general philosophical themes. I have mentioned one above: the idea of making up people. I claim that enumeration requires categorization, and that defining new classes of people for the purposes of statistics has consequences for the ways in which we conceive of others and think of our own possibilities and potentialities.

Another philosophical theme is reasoning. In thinking about science we have become familiar with a number of analytic concepts such as T.S. Kuhn's paradigms, Imre Lakatos's research programmes and Gerald Holton's themata. Following A.C. Crombie I have thought it useful to employ the idea of a style of reasoning.[5] Crombie had in mind enduring ways of thinking such as (a) the simple postulation and deduction in the mathematical sciences, (b) experimental exploration, (c) hypothetical construction of models by analogy, (d) ordering of variety by comparison and taxonomy, (e) statistical analysis of regularities of populations, and (f) historical derivation of genetic development.[6]

Each of these styles has its own sources and its own pace. Those who envisage continuity in the growth of knowledge see each style evolving at its own rate. Catastrophists see sharp beginnings and radical mutations. One need not dogmatically adhere to either extreme in order to see styles of reasoning coming together. Each contributed to what Crombie calls 'the growth of a research mentality in European society'.

My topic is Crombie's style (e) which, of the six that he distinguishes, is quite the most recent. Despite various discernible precursors and anticipations, our idea of probability came into being only around 1660, and the great spurt of statistical thinking did not occur until the nineteenth century. The statistical example makes plain that the growth of a style of reasoning is a matter not only of thought but of action. Take so seemingly unproblematic a topic as population. We have become used to a picture: the number of people in a city or in a nation is determinate, like the number of people in a room at noon, and not like the number of people in a riot, or the number of suicides in the world last year. But even the very notion of an exact population is one which has little sense until there are institutions for establishing and defining what 'population' means. Equally there must be ways of reasoning in order to pass from cumbersome data to sentences with a clear sense about how many were such and such. Most professionals now believe that representative sampling gives more accurate information about a population than an exhaustive census. This was unthinkable during most of the nineteenth century.[7] The very thought of being representative has had to come into being. This has required techniques of thinking together with technologies of data collection. An entire style of scientific reasoning has had to evolve.

Its development was intimately connected with larger questions about what a society is, and thus leads to speculation and historical study of the formation of the western concept of a community.[8] But it also invites more abstract analytical philosophy, because styles of reasoning are curiously self-authenticating. A proposition can be assessed as true-or-false only when there is some style of reasoning and investigation that helps determine its truth value. What the proposition means depends upon the ways in which we might settle its truth. That innocent observation verges nervously on circularity. We cannot justify the style as the way best to discover the truth of the proposition, because the sense of the proposition itself depends upon the style of reasoning by which its truth is settled. A style of thinking, it seems, cannot be straightforwardly wrong, once it has achieved a status by which it fixes the sense of what it investigates. Such thoughts call in question the idea of an independent world-given criterion of truth. So the seemingly innocent notion of a style of reasoning can lead to deep waters, and it is wiser to enter them by wading into examples than by a high dive into abstraction. The development of statistical thinking may be our best example available – because most recent and enduring and now pervasive.

Historians will see at once that what follows is not history. One may pursue past knowledge for purposes other than history of science or history of ideas. A noncommittal account of what I am attempting might be: an epistemological study of the social and behavioural sciences, with consequences for the concept of causality in the natural sciences. I prefer a less expected description. This book is a piece of philosophical analysis. Philosophical analysis is the investigation of concepts. Concepts are words in their sites. Their sites are sentences and institutions. I regret that I have said too little about institutions, and too much about sentences and how they are arranged.

But what sentences? I use only the printed word, a minuscule fraction of what was said. The distinguished statistician I. J. Good noted in a review that 'the true history of probability or of science in general will never be written because so much depends on unrecorded oral communication, and also because writers often do not cite their sources'.[9] The true historian of science is well able to solve the second problem, but not the first. One may nevertheless make a good stab at it by consulting the ample Victorian troves of notebooks, letters and other ephemera. I do not do so, for I am concerned with the public life of concepts and the ways in which they gain authority. My data are published sentences.

But which ones? I omit many pertinent words because one cannot do everything. I leave out Malthus and Mendel, for example, A.A. Cournot, Gustav Fechner, Florence Nightingale and ever so many more modest participants in the taming of chance. Very well: but I say nothing of Maxwell, Bolzmann or Gibbs, although statistical mechanics is critical to the spread of chance and probability not only into physics but also into metaphysics. I say nothing of Charles Darwin, although evolutionary theorizing was to import chance into biology. I say nothing of Karl Marx fabricating an iron necessity out of the very same numerals, the identical official statistics, that I have incorporated into an account of the taming of chance.

There is an uncontroversial good reason for silence about these figures. Scholars and teams of scholars dedicate their lives to the study of one or another. It would be folly to venture a short story here, a mere chapter. But it is not only prudence and respect, but also method, that makes me hold my tongue. Transformations in concepts and in styles of reasoning are the product of countless trickles rather than the intervention of single individuals. Marx, Darwin and Maxwell worked in a space in which there was something to find out. That means: in which various possibilities for truth-or-falsehood could already be formulated. This book is about that space. So although a lot of sentences are reproduced in this book, they are the words not of heroes, but of the mildly distinguished in their day, the stuff of the more impersonal parts of our lives.

Sentences have two powers. They are eternal, and they are uttered at a moment. They are anonymous, and yet they are spoken by flesh and blood. I have tried to answer to these two facts. On the one hand, I do regard the sentences as mere material objects, inscriptions. But to do that, and only that, is to become lost in vain abstraction. As counterbalance, my epigraphs to each chapter are dated, to recall that on a real day important to the speaker, those very words were uttered, or are said to have been uttered. My footnotes (marked with asterisks) are anecdotes that would be improper in the more solemn text.* They give some tiny glimpse of who the speakers were. But there is seldom anything personal about the footnotes. They address the individual as official, as public writer, even if his behaviour may strike us, so much later, as strange.

Thus although many chapters have a central character or text, it is not because Salomon Neumann, A.-M. Guerry or John Finlaison is 'important'. They are convenient and exemplary anchors for a particular organization of sentences. I use the antistatistical method, that of Frédéric Le Play, topic of chapter 16. After having interminably trekked across the

* Notes at the end of the book provide references, and, rarely, numerical formulae. They are marked with numerals. A numeral after an asterisk (as *3) indicates that note 3 at the end of the book bears on the material in the footnote marked *.

written equivalent of his Hartz mountains, I take what I think is the best example of one speaker. Much like Le Play, I include a few stories, but the personages whom I use are in some ways like his household budgets, if, alas, less thorough.

There is one exception among these chapters. The final one is twice as long as the others, and is a rather full account of one side of one writer, namely C.S. Peirce. He really did believe in a universe of absolute irreducible chance. His words fittingly end this book, for as he wrote, that thought had become possible. But I argue that it became possible because Peirce now lived a life that was permeated with probability and statistics, so that his conception of chance was oddly inevitable. He had reached the twentieth century. I use Peirce as a philosophical witness in something like the way that I used Leibniz in *The Emergence of Probability*.[10] But Leibniz was a witness to the transformation that I was there describing, namely the emergence of probability around 1660 and just afterwards. Here Peirce is the witness to something that had already happened by the time that he was mature. That is why he is the topic of the last chapter, whereas in *Emergence* the name of Leibniz recurred throughout.

Although other philosophers are mentioned in the two books, only Leibniz and Peirce play a significant part. The two works do, however, differ in structure in other ways. *Emergence* is about a radical mutation that took place very quickly. Doubtless, as Sandy Zabell and Daniel Garber have shown in an exemplary way, the book underestimated various kinds of precursors.[11] My central claim was, however, that many of our philosophical conceptions of probability were formed by the nature of the transition from immediately preceding Renaissance conceptions. Accounts of the methodology have been given elsewhere.[12] *Taming*, in contrast is about a gradual change. Hence the geological metaphors: avalanches, yes, but also erosion.

Most of my selections and omissions – such as my long treatment of Peirce and my neglect of any other philosopher – have been deliberate. But sloth and good fortune have also played their part. When I began work there was hardly any recent secondary material; now there is a great deal. I am particularly glad of new books by my friends Lorraine Daston, Ted Porter and Stephen Stigler, and of earlier ones by William Coleman and Donald MacKenzie. We all participated in a collective inspired and guided by Lorenz Krüger. The joint work of that group has also appeared. Hence there is now a number of brilliant and often definitive accounts of many matters that overlap with mine.[13] They have made it unnecessary for me to examine a good many matters. And aside from specific histories, there are also points of great generality that I have allowed myself to gloss over in the light of that collective work. For example, another virtue of my geological metaphor is that the erosion of determinism took place at markedly different rates on different terrains. Not uncommonly the least deterministic of disciplines most fiercely resisted indeterminism – economics is typical. This phenomenon emerges from the individual studies of the research group, and is further emphasized in a recent summing up of some of its results.[14]

I have mentioned a number of more specific topics on which I have only touched, or have entirely avoided: making up people; styles of reasoning; great scientists; philosophers; mathematical probability. There is a more glaring omission. I write of the taming of chance, that is, of the way in which apparently chance or irregular events have been brought under the control of natural or social law. The world became not more chancy, but far less so. Chance, which was once the superstition of the vulgar, became the centrepiece of natural and social science, or so genteel and rational people are led to believe. But how can chance ever be tamed? Parallel to the taming of chance of which I speak, there arose a self-conscious conception of pure irregularity, of something wilder than the kinds of chance that had been excluded by the Age of Reason. It harked back, in part, to something ancient or vestigial. It also looked into the future, to new, and often darker, visions of the person than any that I discuss below. Its most passionate spokesman was Nietzsche. Its most subtle and many-layered expression was Mallarmé's poem, 'Un Coup de dés'.[15] That graphic work, whose words are more displayed than printed, began by stating that we 'NEVER ... will annul chance'. The images are of shipwreck, of a pilot whose exact mathematical navigation comes to naught. But the final page is a picture of the heavens, with the word 'constellation' at its centre. The last words are, 'Une pensée émet un coup de dés', words that speak of the poem itself and which, although they do not imagine taming chance, try to transcend it.

# 23

# A universe of chance

> Chance itself pours in at every avenue of sense: it is of all things the most obtrusive. That it is absolute is the most manifest of all intellectual perceptions. That it is a being, living and conscious, is what all the dullness that belongs to ratiocination's self can scarce muster the hardihood to deny.*[1]

The Age of Reason, of ratiocination, had seen things differently. Peirce reversed Hume's dictum, 'that chance, when strictly examined, is a mere negative word, and means not any real power which has anywhere a being in nature'.[2] It wasn't easy. Peirce had tried to settle on half measures.

> For a long time I myself strove to make chance that diversity in the universe which laws leave room for, instead of a violation of law, or lawlessness. That was truly believing in chance that was not absolute chance. It was recognizing that chance does play a part in the real world, apart from what we may know or be ignorant of. But it was a transitional belief which I have passed through.[3]

Peirce denied determinism. He also doubted that the world is a determinate given. He laboured in a community seeking to establish the true values of Babbage's constants of nature; he said there aren't any, over and above those numbers upon which we increasingly settle. He explained inductive learning and reasoning in terms of merely statistical stability. At the level of technique, he made the first self-conscious use of randomization in the design of experiments: that is, he used the law-like character of artificial chances in order to pose sharper questions and to elicit more informative answers. He provided one of the standard rationalia for statistical inference – one that, named after other and later workers, is still with us. He had an objective, frequentist approach to probability, but also

* C.S. Peirce, writing in early 1893 a 'Reply to the Necessitarians'. Peirce had 'attacked the doctrine that every event is determined by law ... At the end of my second paper, the partisans of the doctrine of necessity were courteously challenged and besought to answer my arguments. This, so far as I can learn, Dr. Carus alone, in *The Monist* of July and October 1892, has publicly vouchsafed to do.' Peirce's papers did provoke one other immediate response: in the April 1893 issue we read John Dewey on 'The Superstition of Necessity'.

pioneered a measure of the subjective weight of evidence (the log odds). In epistemology and metaphysics, his pragmatic conception of reality made truth a matter of what we find out in the long run. But above all, he conceived of a universe that is irreducibly stochastic.

I end with Peirce because he believed in absolute chance, but that is not my focus. His denial of the doctrine of necessity was incidental to a life permeated by statistics and probabilities. Somebody had to make a first leap to indeterminism. Maybe it was Peirce, perhaps a predecessor. It does not matter. He 'rejoiced to find' himself in the company of others, including Renouvier.[4] He did argue against the doctrine of necessity, but it was not an argument that convinced him that chance is an irreducible element of reality. *He opened his eyes, and chance poured in* – from a world which, in all its small details, he was seeing in a probabilistic way. In this respect, although he was very much a nineteenth-century man, he was already living in a twentieth-century environment. His working days of experimental routine, and his voyages of the mind, took place in a new kind of world that his century had been manufacturing: a world made of probabilities.

Peirce is the strongest possible indicator that certain things which could not be expressed at the end of the eighteenth century were said at the end of the nineteenth. I do not use him here because he is the happy upshot of preceding chapters, the point at which groping events finally led to the truth as we now see it. Not at all: some of what he wrote strikes me as false and much of it is obscure. I use him instead to exemplify a new field of possibilities, the one that we still inhabit. Chance poured in at every avenue of sense because he was living in a new probabilistic world. One can't grasp that just by reading him on the romantic subject of absolute chance. You have to glimpse the almost innumerable ways in which his world had become constructed out of probabilities, just like ours.

This chapter is twice as long as preceding ones, and differs in other ways as well. It breaks down into sections:

1. A measurer at the Coast Survey (biographical)
2. Necessity examined
3. Errors of observation
4. Psychophysics and randomization
5. Induction and hypothesis
6. Disposition and relative frequency
7. The truth-preserving virtue
8. Probable error
9. Induction and the weight of evidence
10. Community

## 1 A MEASURER AT THE COAST SURVEY

Philosophers know some rough and ready facts about Peirce's career, often presented as an endless sequence of hackwork. It is noted that he applied for but failed to gain or retain regular university work, that late in life he eked out a living writing 182 longish entries in Baldwin's philosophical *Dictionary*, doing translations for the Smithsonian, or producing 348 more or less weekly reviews for *The Nation*. Hence it has been less emphasized that for the 30 most vigorous years of his life he was employed by the US Government in the Coast Survey. This is not a mere biographical detail. His job was measurement and the improvement of measuring devices and it was there that he formed his philosophy of chance.[5] Peirce was a transitional figure, a public employee who for most of his years in office was able to do pretty much what he wanted. When the Coast Survey fell under the fiscal scrutiny of Congress, he was on his way out.[*6]

Peirce grew up in Cambridge, of solid New England stock. His father Benjamin Peirce ('universally acknowledged to be by far the strongest mathematician in the country'[7]) worked the boy mercilessly but could provide patronage because in addition to being a professor at Harvard he was a dominant figure in the Observatory and a power in the Coast Survey. C.S. Peirce was taken on by the Survey in mid–1861, when he was 21, and promoted to the rank of assistant in 1867, when his father became Superintendent. His father died in 1880. After the survey was reorganized on the lines of a more modern bureaucracy, Peirce was obliged to resign.

He had not been very constrained by his job but he performed his duties with passion.[8] He was a measurer, an observer and a designer of instruments. He was much occupied with measuring gravity, using

* It was found 'that for several years beginning in 1873 C.S. Peirce, assistant, has been making experimental researches with pendulums without restriction as to times or places; that since 1879 expenditures on account of those experiments, aside from salaries of chiefs and assistants, amount to about $31,000; that the meager value of those experiments to the bureau has been substantially destroyed'. That is the *Washington Post* of 7 August 1885 reporting on the findings of a Joint Commission of Congress looking into the Coast Survey. Peirce got off lightly, as may be seen from the headline: 'Intoxicated and Demoralized/A Terrible Arraignment of the Coast Survey Officials/Prof. Hilgard and Others Charged with Being Drunk in Office Hours/Full Text of the Investigating Committee's Report'. Peirce was accused only of overdedication to worthless science. He retorted that the costs were one third of those alleged, that his instructions were all on file, that no records had been destroyed, and 'I maintain the value of determinations of gravity in general, and the excellence of mine in particular.'

pendulums of his own design. His researches in photometry were intense. He managed to match wavelengths of light to the length of a rod, an achievement that would make the standard metre obsolete. His father thought that his greatest achievement.

## 2 NECESSITY EXAMINED

'The Doctrine of Necessity Examined' might seem a fine conclusion to a study of the erosion of determinism. But now that we have arrived it is perfunctory. Peirce could not take seriously any determinist antagonist. Could such a one not open his eyes or any other sense and see? In brief, Peirce noted that necessity is a not a universal doctrine, not even in the European tradition: we've had Epicurus (and Lucretius) on 'spontaneous chance'.[9] Observation can't establish 'mechanical causation'. We observe only 'that there is an element of regularity in nature'. That has 'no bearing whatever upon the question of whether such regularity is exact and uniform'.[10] Arguments *a priori* or based on inconceivability can (thanks to J.S. Mill) be given no credence. But most important, the diversity and specificity of the universe is evolving, together with laws of the universe. There is spontaneity in the world, of which our sense of free choice is a minor element.

Such was Peirce's sequence of commonplaces; he himself concluded by not explaining 'the chief of my reasons'. He asserted that the 'hypothesis of chance-spontaneity is one whose inevitable consequences are capable of being traced out with mathematical precision into considerable detail'. He doubted that other mathematicians would follow him, 'so that the strongest reason for my belief must for the present remain a private reason of my own', although one that will for future mathematicians prove to be a 'veritable gold mine'.[11]

## 3 ERRORS OF OBSERVATION

Peirce spoke of 'that law of the distribution of errors which Quetelet, Galton, and others, have applied with so much success to the study of biological and social matters'.[12] He respected their work, but because he was an observer, the error law was first of all about error and about judgements, not biometrics.

His 1870 study 'On the Theory of Errors of Observations' began with casual remarks on the logic of relations and the nature of induction, which must have puzzled virtually all of its few readers with the exception of Peirce's own father – but then it appeared as an appendix to the *Report* of the newly-appointed Superintendent of the Coast Survey, namely his father.[13] The paper had a plain derivation of the theory of observations. Care must be taken in application. The right sorts of approximations must be used. Peirce was scrupulous in commending procedures set out by Encke in Berlin 30 years earlier, but the point of the paper came at the end.[14] He wanted to see how training could improve the 'personal equation' of an observer.

Observatories routinely determined the instant at which a planet or a star crossed the meridian. Observers differ systematically in their measurements. Bessel represented this by the 'personal equation', a correction factor to be added to the measurement taken by an individual.[15] Peirce asked: could one improve an observer's personal equation? For someone versed in the error curve, this does not mean: could one be trained to make less error? It means: could the variation in one's errors be diminished by practice?

Peirce reported on an untrained boy, who for a month made 500 judgements of time every weekday. He had to press a key each time he heard a sharp rap. His errors (delays) made on each day were plotted and the curve smoothed. On the first day 'the observations were scattered to such an extent' that no serious curve-smoothing was possible, but soon the familiar bell-shaped curve emerged. The 'personal equation' changed, first reducing to the point at which the lad was only a seventh of a second late, then increasing a little. But the 'probable error or range of errors was constantly decreasing after the twelfth day'. By the end of the month this measure of variation was only about $\frac{1}{80}$th of a second. This meant that one or two of his observations were as good as a great many by someone less on his toes. Draconian Peirce 'would therefore recommend that transit-observers be kept in constant training by means of some observations of an artificial event which can be repeated with great rapidity, so that several hundred can be taken daily without great labor'. One could train a person to make judgements that fit the Normal curve. We have seen the curve become a biological and social reality. For Peirce it became a psychological reality.

## 4 PSYCHOPHYSICS AND RANDOMIZATION

The personal equation arose in astronomy, but is a matter for psychology. Psychophysics was founded in the 1850s by the brilliant but strange Gustav Fechner. He asked, how well can people distinguish objects of slightly different weights? He used 'a method of right and wrong cases'. A subject, typically the experimenter Fechner himself, was given two boxes, one heavier than the other, and asked on a series of trials to pick the heavier. The difference in weight, and the proportion of right judgements, indicated sensitivity to that difference in weight. But was there a general law for a person's ability to discriminate?

Yes: the Gaussian curve once again. The variance measured the sensitivity of an individual.[16] Here was more autonomy for statistical laws: they presented a psychological reality of which we are not even conscious, but which is nonetheless part of our system of sensation and judgement. Fechner, like Galton, found the probability curve wonderful. He compared it to Proteus who 'seems to avoid every answer through the charming forms he assumes, but one thing suffices: remain undeterred, hold him constantly to the same point – and a reliable answer will be forced out of him'.[17] Michael Heidelberger argues that Fechner was quite literally the first thoroughgoing indeterminist of modern times.[18] It is unclear how closely he connected indeterminism and stochastic variation. If one judges that he assimilated the two, then he must have conceived of the Gaussian distribution as autonomous well before Galton came on the scene.

Fechner had held that there is a threshold below which one cannot discern small differences. The Normal distribution of sensitivity becomes invalid for a small enough difference in weight. Peirce made the next step by insisting on the 'reality' of the curve even below the threshold of conscious perception: if forced to judge which of two weights is the heaviest, the observer will make subliminal distinctions, whose accuracy will continue to fall off according to the curve of error. How to investigate this hypothesis? The experiment conducted in 1884 by Peirce and a student, Joseph Jastrow, later a distinguished professor of psychology, embodied a number of innovations that we now take for granted in work of this sort.[19] For example the subject was 'blind' – elaborate devices ensured that he did not know whether he was presented first with a heavier or a lighter weight. More important, this was the first experiment in which the sequence of trials was chosen by an artificial randomizer, and in which the use of the randomizer was built into the analysis of the data.[20]

Here we witness two small steps in the taming of chance. First, one's psychological curve of error became an inferred, theoretical curve, which one cannot judge by introspection. It became a reality underneath the phenomena of consciousness. Secondly, Peirce deliberately used the properties of chance devices to introduce a new level of control into his experimentation. Control not by getting rid of chance fluctuations, but by adding some more![21] Peirce thought that his discovery, that there is no minimum threshold,

> has highly important practical bearings, since it gives new reason for believing that we gather what is passing in one another's minds in large measure from sensations so faint that we are not fully aware of them, and can give no account of how we

reach our conclusions from such matters. The insight of females as well as certain 'telepathic' phenomena may be explained in this way. Such faint sensations ought to be fully studied by the psychologist and assiduously cultivated by every man.[22]

Some will read the 'insight of females' and 'every man' in the light of the fact that Peirce had just endured a painful divorce and successful remarriage. The remark about telepathy is relevant. The word 'telepathy' was two years old. The Society for Psychical Research had been founded in London in 1882. Its members wanted to replace vulgar and popular enthusiasm for mediums by a scientific study; instead of communication from the dead at séances, they supposed that there was a phenomenon of thought transference between living people. The first project of the society was to conduct a census of examples of telepathy, and then to engage in experiments. An American Society for Psychical Research was founded in Boston in 1884, with the same ends. (The aura of those psychic times can be gleaned from Henry James's novel *The Bostonians*). The 1884 American society was short-lived, folding in 1889 out of scepticism. The English society continues to this day. Experiments on telepathy not surprisingly led to a long tradition of randomized experimental design, although the full rationale was poorly understood until the work of R.A. Fisher in the 1920s. But that is another story.[23]

## 5 INDUCTION AND HYPOTHESIS

Randomization in the design of experiments is a technique for drawing statistical inferences. It has become part of the logic of induction, reminding us that induction is not just a matter of thinking but of doing. Peirce's own theory of probable inference is closest to that of Jerzy Neyman and E.S. Pearson. That is, it is a theory of inductive behaviour, of doing. But Peirce did not dismiss the philosophers' problem of induction. He took it with high seriousness.

How is it that a man can observe one fact and straightway pronounce judgment concerning another different fact not involved in the first? Such reasoning, as we have seen, has, at least in the usual sense of the phrase, no definite probability; how then can it add to our knowledge? This is a strange paradox; the Abbé Gratry says it is a miracle, and that every true induction is an immediate inspiration from on high. I respect this explanation far more than many a pedantic attempt to solve the question by some juggle with probabilities, with the forms of syllogism, or whatnot. I respect it because it shows an adequate cause, and because it is intimately connected – as the true account should be – with a general philosophy of the universe.[*24]

* When the Vatican Council of 1870 sanctioned the doctrine of papal infallibility, Gratry became its best known critic. Peirce's 'Four methods of Settling Opinion' was penned in 1872, directly after the Council. It was an early version of Peirce's most widely read essay,

Peirce connected induction and probability in a novel way, connected with his own general philosophy of the universe. But before we get to that, some preparatory explanation is in order. From the time of his Harvard lectures of 1865 Peirce consistently distinguished 'three kinds of inference': deduction, induction and hypothesis.[25] What's hypothesis?

> I once landed at a seaport in a Turkish province [while scouting for the 1870 solar eclipse expedition]; and, as I was walking up to the house which I was to visit, I met a man upon horseback, surrounded by four horsemen holding a canopy over his head. As the governor of the province was the only personage I could think of who would be so greatly honored, I inferred that this was he. This was an hypothesis.[26]

The method of hypothesis proposes a conjecture that explains a puzzling or interesting phenomenon. For a while he renamed this method 'abduction'. (He also used 'retroduction' in a related sense.[27]) He said he wanted this 'peculiar name' to make clear that the conjecturing of a preferred hypothesis was not induction at all.[28] A few philosophers have adopted Peirce's peculiar word, and others follow Gilbert Harman's attractive phrase 'inference to the best explanation'. I shall continue with the standard nineteenth-century word of Peirce and his predecessors such as Whewell: hypothesis.[29]

Peirce only briefly toyed with the thought that some kind of probability attaches to an inference made by the method of hypothesis. He gave that up. One difference between the foundations of induction and of hypothesis is this: probability has *nothing* to do with hypothesis. Probability has *something* to do with induction. Peirce's innovation lay in saying what that something is.

## 6 DISPOSITION AND RELATIVE FREQUENCY

Told that probability has something to do with induction, most people suppose that if the proposition $A$ is the conclusion of an inductive inference, then we infer something of the form, 'The probability of $A$ is $p$.' No!

> It may be conceived, and often is conceived, that induction lends a probability to its conclusion. Now that is not the way in which induction leads to the truth. It lends no definite probability to its conclusion.[30]

To see why, we need to examine both Peirce's conception of probability and his conception of inference.

'The Fixation of Belief'. What the final version calls 'the method of authority' Peirce called, in 1872, 'the method of despotism'. The references are undoubtedly to the Vatican Council and Gratry's onslaught. Peirce often spoke of Gratry with respect: 'the modern theories of Boole, Apelt, Herschel, Gratry, Whewell, Mill'. Much later he said that Gratry was like the more famous Babbage and Boole, 'off the main lines of intellectual traffic' but 'still read'.

Peirce's central ideas about probability were commonplace. He regularly and rightly honoured Boole's 1854 *Laws of Thought.*[31] From Boole he learned the idea of a logical algebra. More important, he realized that his youthful unreflective degree-of-belief approach to probabilities and combining evidence was badly wrong.[*32]

He was soon convinced that probability applies not to an individual event, but to a series. He first thought that a probability is a relative frequency in an actual series. That was Venn's idea. When he reviewed Venn's book in 1867, the year after it appeared, he wrote: 'Here is a book which should be read by every thinking man'.[33] (Years later he was less enthusiastic: a 'blundering little book'.[34]) Originality is not at issue. As I said in chapter 15, most writers younger than De Morgan had a frequentist theory, which was almost inevitable in an era of enthusiasm for statistical laws.

Peirce came to call this approach nominalist. He said his own thought evolved towards the realism of Duns Scotus. He remarked that every young man should be a nominalist, but every mature one a realist. His own ideas about probability followed this pattern. By the 1890s he was proposing a dispositional or propensity theory of probability: the probability of throwing a six with a die is the relative frequency with which the die would fall six in tosses of a certain sort, were they possible. He spoke of the 'would be' of a die. Arthur Burks has documented this evolution from frequency to propensity and suggested reasons for Peirce's development.[35]

The dispositional 'would-be' idea is new only in explicitness. What did Laplace mean by the *facilité* of obtaining heads with a coin – the ease of throwing heads – if he did not mean Peirce's 'would be'?[36] At most we may say of Peirce what he said of Venn in 1878:

> The conception of probability here set forth is substantially that first developed by Mr Venn, in his *Logic of Chance*. Of course, a vague apprehension of the idea had always existed, but the problem was to make it perfectly clear, and to him belongs the credit of first doing this.[37]

## 7 THE TRUTH-PRESERVING VIRTUE

What is remarkable is not Peirce's conception of probability but the way that he connected it with the soundness of arguments. The idea was already present in a Boston lecture delivered on 31 October 1866: 'A piece

* Before reading Boole he wrote rubbish about probability. In 1861: 'If a premiss rests on a thousand data each of which has one chance in ten of being worthless, the chance of the premiss itself being false is one out of twenty octillion nonillion vigintillion vigintillion vigintillion vigintillion vigintillion vigintillion vigintillion vigintillion vigintillion vigintillion vigintillion vigintillion vigintillion vigintillion vigintillion.' Compare an older Peirce lashing out at the probabilities derived by the hapless leaders of the Society for Psychical

of evidence which yields a likelihood always yields that likelihood by a process which would more often yield truth than the reverse; and every process which is known to yield truth more often than the reverse gives likelihood'.[38]

'More often yielding truth than the reverse': that is the core of Peirce's understanding of deductive and inductive logic. '*Logic* is the science needed in order to test argument.' It does so not by examining individual arguments but by considering the 'genus' of an argument. If the genus is such that the conclusion of the argument is true whenever the premises are true, the argument is *demonstrative*. If it is such that the conclusion is usually true when the premises are true, it is merely *probable*.[39] In either case, a valid argument has 'the truth-producing virtue'.[40]

When the premises are quantitative, we may be able to replace the 'usually' by a numerical probability. That does not mean that conclusion has a probability of such and such. Rather: the conclusion is reached by an argument that, with such and such a probability, gives true conclusions from true premises.

## 8 PROBABLE ERROR

Peirce had a model for this kind of argument, based on the standard practice of astronomers, the 'probable error'. The probable error divides measurements into two equal classes. If the errors are Normally distributed, then in the long run half the measurements will err in excess of the probable error, and half will be more exact. But what does this amount to?

Then, as now, most consumers of statistics used 'cookbooks' to make calculations without caring much what they meant. Most seem to have thought: 'I am measuring a position $x$. I average my measurements to obtain the mean $m$. I compute the probable error $e$. The probability, that $m$ is within $e$ of $x$, is a half.' That is a mistake – but not far from the truth.

Think of estimation on the basis of measurements as a kind of inference. Inductive inference pertains to a genus of arguments. Arguments have premises. In this case, arguments of the genus will have two premises (a) the actual set of measurements of which the mean is $m$ and the probable error is $e$, and (b) the proposition that errors are Normally distributed. The inference to be drawn is '$x$ is within $e$ of $m$'. The inference is *not* 'the probability is $\frac{1}{2}$ that $x$ is within $e$ of $m$'. If we wish to use a probability-related concept, we ought to say, 'this conclusion is reached by a

Research: 'these numbers, which captivate the ignorant, but which repel thinking men, who know that no human certitude reaches such figures of trillions, or even billions, to one'.

genus of arguments which lead from true premises to true conclusions as often as not'.

Peirce is original in understanding the logic of the situation. Readers familiar with the logic of statistical inference will have noticed that Peirce was providing the core of the rationale of the theory of confidence intervals and of hypothesis-testing advanced by Jerzy Neyman and E.S. Pearson in the 1930s, which is still, for many, the preferred route in statistics.[41] As usual, I am unconcerned with Peirce the precursor. Neyman did not learn anything from Peirce. Still, there is a certain line of filiation. The first modern statement of the rationale of confidence intervals was given not by Neyman but by the Harvard statistician E.B. Wilson. Wilson had been a pupil of Peirce's cousin B.O. Peirce, and was a lifelong admirer of the family. He was one of the few readers of C.S. Peirce on errors of observation, and wrote a paper about it.[42] He had the right perspective as regards predecessors. What he had done, he wrote very late in life, was merely to correct the 'logic' of reasoning that employs standard deviations.[43] E.L. Lehmann has pointed out that as far as computation (as opposed to logic) is concerned there is a long tradition of constructing confidence intervals involving Laplace and Poisson, followed by Lexis and one may add Cournot.[44] But it appears that only Peirce, Wilson and then Neyman got clear about the logical principles of this type of reasoning.

## 9 INDUCTION AND THE WEIGHT OF EVIDENCE

Have we lost the problem of induction in the niceties of statistical inference? Peirce thought that the matters just examined are at the heart of induction:

> the general nature of induction is everywhere the same, and is completely typified by the following example. From a bag of mixed black and white beans I take out a handful, and count the number of black and the number of white beans, and I assume that the black and white are nearly in the same ratio throughout the bag.[45]

Sampling, then, was Peirce's model for induction. The rationale can always be cast into the same logical form as the beanbag. 'Now the scientific conduct of this kind of reasoning is highly complex', he wrote, but the logical principle is always the same.

Peirce became clear about the relation between induction and hypothesis. We frame hypotheses, and test them by induction. Thus we reject hypotheses by a method that errs only a small proportion of the time. My emphasis on rejection is faithful to Peirce: a scientific person 'ardently desires to have his present provisional beliefs (and all his beliefs are merely provisional) swept away, and will work hard to accomplish that object'.[46]

Peirce's theory of probable inference applies only when the premises are quantitative enough to validate probability calculation. He did distinguish – rather too late in his life to satisfy some critics – what he called crude, qualitative and quantitative induction.[47] His account of qualitative induction was poor. He thought that in science one should strive for hypotheses that can be tested quantitatively. He was a man of his time, assenting to Kelvin's dictum that one does not understand a thing until one is able to measure it. That was to be expected of a professional measurer, a student of geodesy.

Peirce was well aware that there are personal judgements of probability and that a psychologist might measure them. Stigler has conjectured that in the psychophysics experiments described above, Peirce was the first experimenter 'to elicit subjective or personal probabilities, determining that these probabilities varied approximately linearly with the log odds'.[48] If the probability of an event is $p$, the odds are the ratio $p/(1\text{-}p)$. The log odds are the logarithm of that ratio. Peirce also had the idea that a logarithm of odds helps explain an intuitive idea of the weight of evidence, a theme which has been extensively developed by I.J. Good.[49]

## 10 COMMUNITY

'But there remains', wrote Peirce after urging his ideas about induction, 'an important point to be cleared up.'[50] I want to know how reliable my *next* inference is, not that my method of inferring leads to the truth more often than not.

> An individual inference must be either true or false, and can show no effect of probability; and, therefore, in reference to a single case considered in itself, probability can have no meaning. Yet if a man had to choose between drawing a card from a pack containing twenty-five red cards and a black one, or from a pack containing twenty-five black cards and a red one, and if the drawing of a red card were destined to transport him to eternal felicity, and that of a black one to consign him to everlasting woe, it would be folly to deny that he ought to prefer the pack containing the larger proportion of red cards, although from the nature of the risk it could not be repeated. It is not easy to reconcile this with our analysis of the conception of chance.[51]

Peirce's response was remarkable.

> It seems to me that we are driven to this, that logicality inexorably requires that our interests shall *not* be limited. They must not stop at our own fate, but must embrace the whole community. This community, again, must not be limited, but must extend to all races of beings with whom we can come into immediate or mediate intellectual relation . . . There is nothing in the facts to forbid our having a *hope*, or calm and cheerful wish, that the community may last beyond any assignable date.

That leads to 'that famous trio of Charity, Faith and Hope'.[52] Cantankerous solitary Peirce held that 'social sentiment [is] presupposed in reasoning'. In Peirce's first major series of papers we read that 'the very origin of the conception of reality shows that this conception essentially involves the notion of COMMUNITY, without definite limits, and capable of an indefinite increase in knowledge'.[53] This is the exact opposite of the Cartesian foundation of reality on the introspective individual ego. 'Most modern philosophers', Peirce wrote in the same essay, 'have been in effect Cartesians. Now without wishing to return to scholasticism, it seems to me that modern science and modern logic require us to stand upon a very different platform from this.'[54] The frequent references to community were written at the Coast Survey, where it had real emotional content. His community of inquirers was the community of geodesists, the people in Boston, Berlin, London, Paris, Brussels and even some in Washington.

## 11 TRUTH AND SELF-CORRECTION

Peirce seldom discussed truth. He did teach that truth is the opinion that people would settle down on if they settle down on anything. Early and nominalistically he wrote that truth is what we are fated to believe. Later, 'if truth consists in satisfaction, it cannot be any *actual* satisfaction but must be the satisfaction that *would* ultimately be found if the inquiry were pushed to its ultimate and indefeasible issue'.[55] This is the general form of the transition from nominalism to realism, already noted in connection with chance: it corresponds to the switch from probability relative frequency in an actual series to a 'would-be'. Note the 'ifs' in his minimalist account of truth. Peirce was well aware that

> we cannot be quite sure that the community ever will settle down to an unalterable conclusion upon any given question. Even if they do so for the most part, we have no reason to think the unanimity will be quite complete, nor can we rationally presume any overwhelming *consensus* of opinion will be reached upon every question. All that we are entitled to assume is in the form of a *hope* that such conclusion may be substantially reached concerning the particular questions with which our inquiries are busied'.[56]

This hope is identical to the hope already voiced when he wrote that probability logic is founded on faith, hope and charity.

Peirce is thought to have had a justification of induction, namely, that it is a self-correcting method that leads to the truth. He has even been praised for inventing the idea. Larry Laudan has observed that the praise is misplaced, for this was no innovation in the nineteenth century. It was commonplace and if anything Peirce 'trivialized' it.[57] But an even stronger statement is to be made. It is a simple tautology to say that induction is a self-correcting method that necessarily leads to the truth. Peirce did not think that first of all there is the truth, and then there is a method for reaching it. The truth is what induction gives. His theory of probable inference is a way of producing stable estimates of relative frequencies. But on the other hand the real world just *is* a set of stabilized relative frequencies whose formal properties are precisely those of Peirce's estimators. Method and reality do not fit by good fortune or preestablished harmony. Each defines the other.

This is not an 'interpretation' of Peirce. He said as much himself, even as early as 1869. An inductive form of argument should lead to conclusions that 'would be more apt to be true in the long run ... than a random assertion would be'. In a footnote:

> This sufficiently sets forth the essential elements of an argument; but does not define it, since in introducing the conception of truth it commits a diallele.[58]

'Diallele'? The right word (if such there be) was 'diallelon', invented by Sir William Hamilton in 1860. In the *Century* dictionary, Peirce defined it thus:

> *diallelon*: In *logic*, a tautological definition, a definition which contains the word defined, the definition of a term by means of another which is itself defined by means of the first; definition in a circle.

## 12 EVOLUTIONARY LOVE

It seems empty hocus-pocus to think of truth and scientific method as linked by circular definition. Truth is a matter of how the world is, we protest, and method is what we do. Hence there is a fundamental question about a method: is it any good? That means, does it effectively lead us to find out the way the world is?

Peirce's answer is extraordinary to us, but not to his contemporaries. Many took for granted a striking and necessary parallel between the evolution of mind and of matter. Idealism, of a sort that we have long forgotten, was rampant. 'Matter is effete mind' is a far more striking saying in 1989 than in 1898. Peirce's father Benjamin, in a textbook of mechanics: 'Every portion of the material universe is pervaded by the same laws of mechanical action, which are incorporated into the very constitution of the human mind.'[59] Pragmaticism is a hyperbolic version of this: *the universe reaches its successive states by processes formally and materially analogous to those by which sound method reaches its conclusions.* The connection between 'the way the world is' and 'how we find out about it' is one of identity of organic structure.

At the end of chapter 18 I mentioned Emile Boutroux's doctrine of contingently evolving natural law. William James and to some extent Peirce were close to the Boutroux circle and to Renouvier. Laws of nature, they held, were not given from the beginning of the universe, as most modern cosmology has it. Laws of complex forms were not determined by laws of simpler forms, but came into being as those complex forms emerged in the history of the universe. That's Boutroux in 1875.

Peirce wrote that a philosophy of induction should be embedded in metaphysics. For him, that meant evolutionary metaphysics. It was a metaphysics rich in corollaries for a professional measurer like Peirce. Laws of nature are commonly presented as equations with some fixed parameters, none other than Babbage's constants of nature. But if laws are evolving from chance, we need not imagine that the constants are anything more than limiting values that will be reached in some indefinite future. The ultimate 'reality' of our measurements and what they measure has the form of the Gaussian law of error. It is bank balances and credit ledgers that are exact, said Peirce, not the constants of nature. Stop trying to model the world, as we have done since the time of Descartes, on the transactions of shopkeepers. The 'constants' are only chance variables that have settled down in the course of the evolution of laws.

Peirce combined evolving laws with an evolutionary epistemology. Why are our instinctive ways of classifying things so well suited to simple induction? It is often suggested that natural selection adapts species so that they make discriminations that match the functionally relevant aspects of their environment. If we distinguish colours early, it is because telling things apart by their colour helps us survive. Even if this were true, it would not explain why people are able to explore the cosmos and the microcosmos. There is no discernible evolutionary advantage in our ability to formulate the concept of gravitational force, to go through the steps from Kepler to Newton, and finally to be a 'pendulum-swinger' like Peirce determining the gravitational constant. Peirce morosely remarked that the talent for such thoughts and activities makes one poor company and impedes survival.

Our ability for inquiry of an abstract sort is a product of evolution, but it is at best of indifferent value for our survival. We should think instead of mental abilities as evolving parallel to the evolution of the laws of the universe. We can discover the latter because they and our minds have evolved in the same way. Peirce called this 'evolutionary love'.[60]

## 13 CHANCE IS FIRST

I have not been presenting an interpretation of Peirce, an attempt to explain or to highlight what he really meant. I have aimed only at describing a man whose professional life as a measurer was immersed in the technologies of chance and probability, and who, in consequence of that daily experience, finally surrendered to the idea that there is absolute chance in the universe. He poured this experience of chance into most aspects of his philosophy, including those that we now find esoteric. Peirce was the first philosopher completely to internalize the way in which chance had been tamed in the course of the nineteenth century. It is fitting that the further reaches of his metaphysics could also be summed up in my title, 'the taming of chance'. But where my title was metaphorical, in a Peircian summation it would be literal. For Peirce's history of the universe, in which blind Chance stabilizes into approximate Law, is nothing other than the taming of chance.

Is Reason comforted then, does that giantess, metaphysical chance, no longer threaten or offer untold delights? Do we live in a world made safe by statistical laws, the laws of averages writ small upon the tiniest particles of matter? Of course not. Peirce was fond of trios, which he called Firsts, Seconds and Thirds. 'Chance is First, Law is Second, the tendency to take habits is Third.'[61] That did not mean that chance is annulled by statistical law, or that the successive throws of the dice engender a world in which we can resume or reassume Hume's comfortable habits. What was First is always so. Even when the dice are cast in circumstances of eternity, as when we contemplate the constellations of the cosmos, or cast in circumstances of complete and personal particularity, as when we seal our own fate, chance pours in at every avenue of sense. We cannot suppose that Peirce saw the 1897 copy of *Cosmopolis* containing the poem by Mallarmé, three years his junior.[62] But he was at one with the thought, 'A throw of the dice never will annul chance.'