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The Reason for Structural Causation

> Denn das Selbstgedachte versteht man viel gründlicher, als das Erlernte, und erhält, wenn man es nachmals bei jenen Frühern findet, unverhofft eine stark für die Wahrheit desselben zeugende Bestätigung, durch fremde, anerkannte Auktorität, wodurch man sodann Zuversicht und Standhaftigkeit gewinnt, es gegen jeden Widerspruch zu verfechten.
>
> Arthur Schopenhauer
> "Ueber Philosophie und ihre Methode"

1 The Problem

Suppose you have an urn containing fifty white balls and fifty black balls. You perform a series of a hundred drawings with replacements, and take down the results. As it happens, there are 52 white balls in the series and 48 black ones. This is a normal result under the circumstances described.[1] In fact, we are inclined to say that the colour ratio of balls in the urn in some way *explains* or *causes* the colour ratio among the balls actually drawn in our experiment. Were we to perform a great number of such drawing experiments of length 100, we would find that in the vast majority of cases, the number of white balls drawn is approximately the same as the number of black balls. In other words, the ratio of white to black balls remains approximately *constant* in our repeated sequences of drawings. Laplace ([19], p. xlviii) called the colour ratio of balls in the urn the *constant cause* of the colour ratio among the outcomes. According to Laplace, it is due to Jakob Bernoulli's theorem (a special case of the weak law of large numbers) that the constant cause will in the long run get the better of the so-called *variable causes*: causes which are effective in an uncontrolled way in every single drawing, but the effects of which finally "average out".

1 We may also say that this is a *lucky* result which allows for the kind of explanation to be discussed in this paper. We might as well end up with a series of 97 white and only 3 black balls. It is this realization of the improbable that made Stegmüller ([50], pp. 281–285, 313–314; [51]) argue against the very feasibility of statistical explanations. See Humphreys ([13], pp. 117–118) for a rejoinder.

On the other hand, there is the *mechanistic* picture of explanation. According to this view, an explanation consists in a detailed account of how an initial state of affairs develops and lawfully produces the explanandum. In the case at hand, the description of the initial conditions consists in a perfect specification of the positions and momenta of all balls, of the form of their surface as well as the surface of the urn, and a precise description of the drawing procedure. If we had all this information, then it could — in principle — be calculated exactly which of the balls were bound to be drawn. After adding information about the colours of the balls, one gets the colour ratio as a byproduct of the more informative explanation. The explanation of the colour ratio is as it were only parasitic on the genuine explanation of the actual order of drawings of *exactly these balls* (with "internal numbers" 56, 02, 88, 39, ..., say).[2] An advocate of this account of explanation will say that he does not understand what justifies us to endow the statement of so-called constant causes with the honourable term 'explanation'. In fact, Laplace himself should have fallen victim to this argument. His own creature, the famous demon, will challenge his doctrince of constant causes. Being in an ideal epistemic situation,[3] the omniscient intelligence simply would not understand the recourse to constant causes. He does not need them, he has no place for them.[4]

Is, then, Laplace in the situation of the proverbial sorcerer's apprentice who does not come to terms with the demon he has conjured up? It is the aim of this paper to help Laplace convince the demon that his — Laplace's — doctrine makes sense after all and can in fact be reconciled with the demon's mechanistic account of the world.

This latter account is thoroughly *deterministic*. It is of crucial importance, however, not to mix up two different senses of 'determinism'. We shall use the term to denote the thesis that whatever happens in the world is determined by antecedent conditions; that there are no irreducible chance phenomena which cannot be explained away by a consideration of additional (sometimes hidden) causal factors.[5] We do *not* use the term 'determinism' to signify the thesis that

2 Against a suggestion of Max Urchs, I am inclined to group the following condition as a paradigmatic non-theorem rather than a paradigmatic theorem of the logic of causation or explanation: If A causes/explains B and B logically entails C, then A causes/explains C. Parasitic explanations are no genuine explanations.

3 Naïvely, this is just the situation we attempt to reach in the course of the scienctific enterprise; but cf. footnote 2.

4 W.V.O. Quine ([41], §§ 46–47) appears to be a disciple of the Laplacian intelligence when he insinuates that dispositions and causal statements are to be reduced to the existence of certain "connecting" or "definite mechanisms" ([41], pp. 223, 225). In [42], quite the same explanatory role is played by the notion of natural kind, or — what "varies together" (p. 121) with the notion of kind — the notion of similarity. He holds, however, that "we can take it as a very special mark of the maturity of a branch of science that it no longer needs an irreducible notion of similarity and kind." (p. 138)

everything is predictable by human scientists.[6]

We know that there are fundamental limits to our capacities of determining exactly the antecedent conditions, as well as to our capacities of solving complex equation systems. Hence there are fundamental limits to our capacities of predicting future events. It is misleading to identify determinism with predictability-in-principle, as long as it is not pointed out that what is meant is predictability *for the Laplacian demon* rather than predictability *for humans*. In the following we shall assume, for the sake of argument, that the world is deterministic, even if that does not match the current state of scientific theorizing. The reason for this assumption is, first, that we would be happy to get clear about the (supposedly) simpler case of a deterministic world, and, secondly, that it is the Laplacian demon whom we are going to argue with anyway.

Why is this an interesting problem? I have two answers. First and foremost, the problems with the urn model are symptomatic of a problem concerning explanation in the social sciences. There statistical tables are supposed to be of explanatory value. They exhibit the same kind of constancy as repeated experiments with sequences of drawings. Numbers which are constant over a substantial time interval are taken to be *caused* by something — by some "constant causes." More specifically, the parallel may be drawn along the following lines:

- Due to the colour ratio of the balls in the urn, we may say that each individual drawing has a propensity of 0.5 of showing a black ball. Changing the ratio in the urn will result in a lowering or increasing of this propensity of each individual drawing. Though this does not give an account of the individual "kinetic histories" of actual drawings of black balls, we may still assume that in our deterministic but "chaotic" urn model the initial conditions are distributed in such a way that in a corresponding proportion of cases, black drawings will be prevented or promoted.
- Due to the social and legislative conditions in a society, each individual has a propensity of 0.0027 of committing a certain crime in Germany in 1993, say. Changing the social boundary conditions will result in a lowering or increasing of this propensity of each individual. Though this does not give an account of the individual "moral histories" of actual crimes, we may still

5 This is roughly what Anscombe ([1], p. 63) calls the *assumption of relevant differences*: "If an effect occurs in one case and a similar effect does not occur in an apparently similar case, there must be a relevant further difference."

6 Even if they are equipped with the best measuring devices and computers one can think of. This kind of explication of determinism is aimed at by Popper ([38], pp. 1–2): "what I call *'scientific'* determinism ..., the doctrine that the structure of the world is such that *any event can be rationally predicted, with any desired degree of precision, if we are given a sufficiently precise description of past events, together with all the laws of nature.*" (Popper's italics)

assume that in our deterministic but chaotic world the initial conditions are distributed in such a way that in a corresponding proportion of cases, crimes will be prevented or promoted.

A common cause philosophy was advocated in the social sciences by the Belgian statistician Adolphe Quetelet in the 1830s and '40s.[7] As Weyma Lübbe ([23]) points out, both in the urn example and in social statistics, we assume that the individual events (drawings and crimes) are independent of each other, and yet the collective results show a surprising constancy. This has led to the postulation of a constant cause for the constant macroscopic effects. While it is pretty clear what is the natural candidate for being the constant cause in the urn model — viz., the colour ratio amongst the balls in the urn — we are at a loss what to say in the case of the social sciences. The main problem in contrast to the urn model is that it is at least doubtful whether we can legitimately stipulate the existence of such a thing as a "constant cause" of the crime rate in Germany in 1993. This is not the only problem with the parallel between urn experiments and the tables of social statistics. It must be admitted that considerable work needs to be done in order to reveal that the analogy is indeed instructive.

My second reason for addressing urn experiments in this paper is simpler. There is enough discussion in the literature that warrants a new attempt at clarifying the concept of causality in urn models. In his paper on "The Slow Rise of Probabilism", Lorenz Krüger identifies a lack of discernment of the differences between the two above-mentioned accounts of causation or explanation during the greater part of the 19th century.[8]

The purpose of the present paper is an attempt to reconcile two very different approaches to the concept of causation. In the original form, it is the opposition found in Laplace between his doctrine of *constant* and *variable* causes on the one hand [9] and his *mechanistic determinism* on the other.[10] The tension between the two was formulated in all clarity only by Maxwell who repeatedly stressed the contrast between the *statistical* and the *dynamical* method (which latter he also called the *historical* or *strictly kinetic* method). This problem has again been brought to the fore and identified as a reason for the slow rise of determinism by Krüger who used the terms *structural* and *dynamical* causes

7 See e.g. Quetelet ([40], Vol. I, pp. 3–16).

8 Like Krüger and *pace* Stegmüller ([52], pp. 9, 633–634), I shall not distinguish between the problem of causation and the problem of explanation. Causation and explanation are closely tied in the work of Wesley Salmon.

9 This doctrine of Laplace's is no well-defined theory. It is given in the section "Des lois de la Probabilité qui résultent de la multiplication indéfinie des événements." ([19], pp. xlvii–lv) He does not even address the question of what "causes régulières et constantes" (p. xlviii) are. Presumably they are causes which are supposed to be constantly effective over a substantial period of time.

for the distinction in question.[11] In the recent American discussion, , a similar dichotomy surfaces in the work of Wesley Salmon ([46], p. 99) who distinguishes *statistical* from *aleatory*[12] causation. At any rate, we have *prima facie* conflicting intuitions about causality and explanation.[13] I shall argue that as far as games of chance are concerned, to assign probabilistic laws a fundamental role in scientific explanation does not conflict with the assumption of Laplacian determinism. My vague hope is that in the long run a proper understanding of games of chance will also shed some light on the role of probabilistic laws as employed in the social sciences.

I started to think about structural causation and the relation between determinism and probabilism in response to the stimulating paper of Lorenz Krüger and the equally stimulating discussions with my colleague Weyma Lübbe. I thought that some basic insights of modern chaos theory should be invoked in order to overcome a purely subjective view of probability and convince the Laplacian intelligence that it does make sense to use probabilistic patterns in causal explanations. Only then did I discover that basically the same idea had been put to good use by Henri Poincaré as early as 1896.[14] He pushed the idea much further by offering a mathematical theorem on which philosophical interpretations can turn. For some reason, Poincaré's contribution tends to be neglected in the current literature on probabilistic causality. One notable exception is Jan von Plato ([31] – [34]) who in a series of excellent papers traces the fate of Poincaré's *method of arbitrary functions* up to the modern theory of ergodic systems. I have benefitted a great deal from his work.

2 Four Concepts of Causation

We now present a very rough scheme of four different concepts of causation to which we will refer in our subsequent discussion.

1. *Causation of the Laplacian intelligence: Covering law, deductive-nomolo-*

10 Which found its immortal expression in the metaphor of Laplace's ([19], pp. vi–vii) demon: "Nous devons donc envisager l'état présent de l'univers comme l'effet de son état antérieur et comme la cause de celui qui va suivre. Une intelligence qui, pour un instant donné, connaîtrait toutes les forces dont la nature est animée et la situation respective des êtres qui la composent, si d'ailleurs elle était assez vaste pour soumettre ces données à l'Analyse, embrasserait dans la même formule les mouvements des plus grands corps de l'univers et ceux du plus léger atome: rien ne serait incertain pour elle, et l'avenir, comme le passé, serait présent à ses yeux."

11 "The ratio of the two effects 'white ball drawn' and 'black ball drawn', however, cannot be attributed to any dynamical cause operative in the process. The constant "cause" that brings about this ratio is, of course, the ratio of white to black balls in the urn. This is clearly a real condition of the dynamical process ..., but we may call it a "structural" condition, since it is not a dynamical part of the history of the system." ([18], p. 65)

gical, deterministic causation. Consider a complete description of the state of the world at a given time, take all laws of nature and derive from these two components the state of the world at any other time, again described in full completeness. It makes sense to stipulate that a cause must temporally precede its effects; moreover, an element of the description of the "initial state" qualifies as a *("determining") cause* for an effect under consideration only if it actually enters into the derivation of the effect. We have no occasion to consider counterfactual situations.

2. Laplacian causation: The doctrine of constant and variable causes. In order to ascertain whether something (a "factor") is a cause of the effect under discussion, one has to consider all/a large number of/a select set of situations in which this factor is constantly present, but in which all other factors are changed arbitrarily/with sufficient variability/according to certain criteria of choice. If the effect is obtained uniformly or with sufficient frequency and approximation, then the factor under consideration is in fact to be regarded as a (*"constant") cause*. Here we compare a real situation with — actual or counterfactual — situations where *everything except the putative cause is varied.*

(In numerous special cases the constant causes of Laplace may be identified with the *statistical* or *structural* causes already mentioned.)

3. Counterfactual causation, forward direction: sine qua non *causation.* In order to ascertain whether something (a "factor") is a cause of the effect under discussion, one has to consider all/a large number of/a select set of situations in which all other factors are constantly present, but in which this factor is changed arbitrarily/with sufficient variability/according to certain criteria of choice. If the effect is lost uniformly or with sufficient frequency as a result of the change in the putative causes, then the factor under consideration is in fact to be regarded as a (*"counterfactual") cause*. Here we compare a real situation with — actual or counterfactual — situations where *everything except the putative cause is kept fixed.*

4. Counterfactual causation, backward direction: Why ask, "Why?"? (Salmon [45], Humphreys [13]) Why-questions arise when we are surprised by an effect,

12 This term is due to Humphreys.

13 I shall not pursue here the interesting question whether the two types of causation are in some essential way related to the distinction between a *discrete* and a *continuous* conceptualization of the world. — For more conflicting intuitions, see Stegmüller ([50], pp. 311–317), Suppes ([53]) and Spohn ([48]).

14 It must be pointed out that Poincaré was no isolated figure. The gist of his argument can be found in remarkable detail already in von Kries ([17], Chapter III, Section 2), and a philosophically more penetrating assessment of the problem is provided in the beautiful paper of von Smoluchowski ([47]).

when we have been expecting, at least with considerable probability, a different course of events. In order to ascertain whether something (a "factor") is a cause of the effect under discussion, one has to consider all/a large number of/a select set of situations in which the effect is changed arbitrarily/with sufficient variability/according to certain criteria of choice. If the factor under consideration must have been absent uniformly or with sufficient frequency in order to acccomodate the change in the effects, then it is in fact to be regarded as a (*"counterfactual"*) *cause*. Here we compare a real situation with — actual or counterfactual — situations where *everything except the putative effect is kept fixed*.[15]

(*End of list.*)

Some readers will miss a reference to the positive relevance theories as discussed in contemporary approaches to "probabilistic causality." In fact there will be none. I have found that the recent discussions in this area are more or less irrelevant to the topics treated in the present paper. The account defended below is more like a "causal theory of probability" ([29], p. 59) than a "probabilistic theory of causation".

There is a very strong intuition that the "demonic" account of causation is the best one. The Laplacian demon is in an ideal epistemic situation which we can only envy.[16] What more could one ask from an explanation than the kind of information given by the omniscient demon?

Prima facie, constant causes are very suspect entities for the analytic philosopher. He or she would be afraid of illegitimately hypostatizing something which does not "really exist", or does not have any systematic or scientific significance. But still it is intuitively very compelling to think that there must be a cause (or perhaps: a reason) for the regularities in observed ratios. After all, this inference from regularities in observed frequencies to underlying causal processes is a central motive for embracing the statistical method.

However, I shall not advocate one particular concept of causation against another one as *the true* concept. Actually I think that this would at least in part be a quarrel about terminological questions devoid of any genuine philosophical interest. Rather, it will emerge in the course of our argument that counterfactual causation can help us on our way from Laplacian causation to the concept of causation which fits the Laplacian demon. In this way we hope to be able to

15 In the analysis of conditional and causal connectives, the the contrast between the forward and the backward directions may be traced back to papers of Nelson Goodman ([10]) and Gilbert Ryle ([44]). It is discussed in Rott ([43]) where it is argued that the backward direction is preferable. A similar conclusion is reached by McCall ([28], p. 315). Also compare Gärdenfors ([8], Chapters 8–9) where the anlysis of causality employs a contraction with respect to causes, while explanations involve a contraction with respect to effects.

help Laplace out of the predicament of the sorcerer's apprentice who cannot take a firm hold of the ghost he has conjured up.

3 How to Convince the Laplacian Intelligence of the Existence of Structural Causes

Counterfactual Reasoning

We ask about a cause or about an explanation of an event when we did not expect it to take place. This is not the same as to say that we expected that the explanandum event would not happen. We merely think that things *might* have turned out otherwise, that is, that the explanandum event was not necessary. If something has been necessary all the time, we do not ask for its cause.[17]

This line of reasoning should be plausible even for the Laplacian intelligence. True, in the first instance the demon does not particularly concern itself about alternative worlds or alternative developments of our real world. But granted the above-mentioned pragmatic presupposition of our search for causes, the intelligence can of course satisfy our curiosity. If you ask, "Why did this mixture of chemical substances explode?" you indicate that you rather expected the whole thing to remain in a stable condition. The Laplacian intelligence can tell you what sort of initial conditions would have to be given, if the mixture were to remain stable.

For the Laplacian intelligence, counterfactual reasoning in the backward direction is not essentially different from counterfactual reasoning in the forward direction. Remember that by hypothesis, it is capable of perfect deterministic prediction as well as perfect deterministic retrodiction. This applies to initial conditions given in the maximal specific description of a microstate as well as to initial conditions given in the form of macrostates. In the first case, the demon has to compute only one trajectory, in the second case, it has to perform the computation of a whole — and in general, a very large — set of trajectories.

16 This is still Maxwell's ([27], p. 439) opinion. But there are differing views. Popper ([38], §§ 10–11) gives the demon a rather human face and thus misrepresents, I think, Laplace's intentions. Similarly, Prigogine and Stengers ([39], p. 271) maintain that "[w]hen faced with ... unstable systems, Laplace's demon is just as powerless as we". Salmon ([45], p. 701) holds that the Laplacian intelligence may lack "knowledge of the mechanisms of production and propagation of structure in the world", and Spohn ([49], p. 186) contends that it lacks an "inductive scheme" and hence "would not know what to believe, if it were to discover that it is wrong". While Popper and Prigogine and Stengers doubt the demon's predictive competence, Salmon and Spohn deny that the intelligence's predictive competence suffices for its explanatory competence.

17 So far we are in perfect agreement with Kant [15].

As the backward and forward directions of counterfactual reasoning are perfectly symmetric for the Laplacian intelligence, we may not only ask "What would have been different, if the actual effect had not shown up?" We may as well ask the converse question "What would have been different, if the actual initial conditions had been slightly different?" For the Laplacian intelligence, these questions are not fundamentally different. For finite creatures, however, who like us assume that the causal direction follows the arrow of time, it seems more natural to pose the second question. We could have changed the effect by manipulating the cause, but not vice versa. This action-oriented intuition of control may be regarded as a good reason for preferring counterfactual reasoning in the forward direction than in the backward direction.[18]

The essential idea to be applied in our urn model, however, is neutral with respect to the temporal direction of counterfactual reasoning. Fix any set of initial conditions. For the Laplacian intelligence, the whole sequence of drawings (or, for that matter, a sequence of sequences of drawings) is just a tiny part of the course of the whole deterministic world. So everything, including the acts of drawing, is settled from the start.[19] Then, of course, the Laplacian intelligence can predict the results of our sequence (of sequences) of drawings. Clearly, we can say "If these-or-those initial conditions had obtained, the result would have been such-and-such." The problem is which initial conditions to consider in this deterministic thought experiment.

Weyma Lübbe ([23]) considers variations of the colour ratio of the balls in the urn in order to find out about its causal relevance. I do not think, however, that this is a very natural line of reasoning, or in the spirit of the Laplacian intelligence. As I said above, the colour ratio in our series of drawings is explained only in a parasitic manner, viz., by first explaining the fact that these very balls were drawn and then looking at the colour of the balls. The colours do not figure in the calculation of the intelligence, so they are no genuine causes for it. In our intuitive deliberations, too, we are not going to vary some special candidate cause (like the colour ratio), but we vary *something*. We produce as slight deviations from the actual initial conditions as possible. We counterfactually shift ball No 38 half a millimeter to the right, give No 82 a little push, remove a tiny unevenness in the urn's wall, or perhaps we exchange the positions of balls No 97 and No 98. The full description of the world, as far

18 *Contra* Ryle, McCall and Rott. — Why do people on trains prefer seats facing the engine?

19 The idea of interpreting repeated experiments as a part of one large experiment has also been used by von Plato ([31], p. 65, [33], p. 45) in his attempt to reconcile the initial-probability account (focusing on abstract "ensembles") with the time-average account suggested by ergodic theory. Von Plato claims that "objective" distributions of initial conditions can be *derived* with the help of the new physico-mathematical theories of ergodicity. If this were true, the explication of chance would stay completely in the realm of the objective.

as it enters into the demon's calculations, should be counted as the cause of the effect. Every conceivable deviation from the initial state should be taken into consideration. Slight deviations should receive more serious consideration than drastic and phantastic ones. Variaton of the colour ratio will not be the obvious idea.[20]

My intuitions, though not particularly firm, suggest that counterfactual reasoning in our case does not accord well with the simple schemes prepared in the previous section. It seems to me that we reflect on hypothetical *changes of any arbitrary kind* relating to the "initial time" when the explaining event is suppposed to have occurred. We do not consider changes with a special view to some preconceived candidate cause, but widespread probability distributions over a great variety of possible deviations from the actual course of events.

Subjectivity

No Equality

We want to know the probabilities for the outcomes of our drawings. To compute them, we need to supply the Laplacian intelligence with a prior distribution for initial states. On the basis of this information, and only on the basis of this information, can all subsequent deterministic calculations be based.

The first and most obvious idea is to say that all possible initial states should be assigned an equal probability. This is the answer Laplace would have given. But unfortunately, the answer is not well-defined. As already pointed out by von Kries ([17], Chapter I, Section 4), it is not clear, even when we are dealing with finite spaces of possibility, *which* elements to count as equiprobable. The problem raises its head with much more severity when infinite possibility spaces are to be considered. This is borne out by the famous paradox of Bertrand. In his influential *Calcul des Probabilités* of 1899, he showed that there are three equally plausible ways of estimating the probablity that the length of a chord in a circle is greater than the length of the sides of an inscribed equilateral triangle — but that the three methods yield three different probability values.

Now, pure geometry has certainly never been the indended domain of application of probability theory, and in practical problems of a similar structure we may expect hints from the empirically given *Versuchsanordnung* what the right formal representation of equipossibility will be. But the problem nevertheless

20 What would have been the result of changing the colour ratio in the urn of our introductory example by substituting a few white balls for black ones (or vice versa)? It may well be that we finally happen to come up with exactly the same colour ratio, 52:48, in our resulting sequence of drawings — although the a priori probability of doing so has increased or decreased. Is, then, the colour ratio in the urn a cause for the colour ratio in that particular sequence?

remains acute. What is the justification for counting certain initial conditions as more or less probable? I take it that Bertrand made clear once and for all that there is no absolute sense of equiprobability (equipossibility), no objective foothold for deciding which frame of reference to use for the fixation of equiprobability. It is not at all obvious how to choose the distribution over the initial states. It seems, therefore, that the problem necessarily involves *subjective* or *conventionalist* (Poincaré) elements.

Similarity

This is not as bad as it may appear. In considerations of causality, we do not entertain the assumption that the world might have been *totally* different from what it is like actually. We do not reckon with dramatic deviations from the actual state of affairs, or with markedly "exotic" initial conditions. In our urn example we do not consider it a serious possibility that the urn's opening is closed, that someone reshapes the balls into cubes, that there are only three instead of 100 balls in the urn, that the balls are green and red rather than black and white, that some hidden magnetic contrivance has been installed in order to introduce a bias, etc. We just think that things might have been *somewhat* different. That is, in most cases (though perhaps less characteristically in games of chance) we are inclined to weigh initial conditions according to how close they come to the actual initial conditions, or to some standard of "normality". When looking for causes, we try to let intact a large number of *ceteris paribus* conditions which only make the causes effective at all. In the set of all conceivable initial conditions, singularities in physical state spaces will be attributed a probability measure zero.

In causal reasoning, then, we invoke a notion of similarity in the style of David Lewis ([21], [22]). There are three points on which our suggestion differs from Lewis, however. First, Lewis's similarity takes the form of a ternary relation of comparative similarity between "worlds" or states. It is only the worlds which are closest to the actual one that matter in Lewis's semantics, all the other worlds do not have any relevance. This all-or-nothing principle for counting worlds *alias* states seems too restrictive. We would rather like to attribute different *weights* to different states, so as to make more remote possibilities count less than close possibilities, but still have some non-zero influence. We propose to measure the relevance of states with the help of a smooth probability distribution over the set of all possible initial states. Second, it seems that distance from the actual world is not the only thing that matters. If the real world happens to exhibit exotic features in some special situation, then we are inclined to give more weight to the possibility that things might have been more ordinary than to the possibility that things might have been still more exotic. What counts, then, is some compound of closeness to the actual world and closeness to a standard of normality.[21] Third, I do not see any objective basis for similarities between possible worlds, or even possible courses of the actual world. Similarity is always similarity-in-a-certain-respect, and there is a huge number of possible respects to pay attention to, but there is no universal rule how to combine similarities and dissimilarities in different respects into a plausible overall similarity. It seems obvious, then, that similarity is a hopelessly subjective notion. But to say this is to aggravate the problem that has already been lingering since our discussion of Bertrand's paradox. We cannot hope to convince the Laplacian intelligence of our own subjective standards of similarity and normality. This would definitely overstrain its readiness to engage in anthropomorphic patterns of reasoning. We somehow have to transcend the realm of the purely subjective.

Assigning weights to initial conditions in the form of probabilities (density functions) might tempt one to think that we are dealing here with a form of uncertainty. Indeed the problem of uncertainty is stressed in much of the relevant literature. But it is essential to understand that it is not our concern here that we are never able to measure the actual initial conditions with absolute precision. True as this is, it is not at all what we are interested in. We do not even aim at a precise determination of the empirical data. In the counterfactual deliberations involved in causal reasoning, we are going to vary the initial conditions anyway — and we do so in accordance with some irremediably subjective standards of similarity and normality.

Objectivity Regained

Consider a single draw of a ball from the urn. Assume that the initial conditions are fixed but unknown. The Laplacian intelligence can specify the region in the state space of initial conditions which leads to the result that ball No 01 is drawn; the region that leads to the drawing of ball No 02; the region that leads to the drawing of ball No 03; etc. etc. Call the set of all points in the state space of a "chance-producing" deterministic system which are bound to produce one and the same effect (e.g., "No 01 is drawn") an *equivalence region*. My claim now is that the equivalence regions form *fractal structures* in the space of all possible initial conditions. By approaching the actual initial state (according to some standard of closeness) we do *not* in the same way approach the actual result of the drawing. We cannot resolve the extremely involved structure of the equivalence regions by using a huge magnifying glass. The complexity of the regions repeats itself at all scales of magnification. This is a speculation so far, but it should present only technical problems to establish the point rather more rigorously.

21 For a discussion of these two kinds of closeness, viewed as two out of five "faces of minimality", see [25].

The essential part of the argument consists of the following idea:

For any (subjective) standard of closeness and for every initial state close to the actual one, there is a huge number of initial states still closer to the actual one which lead to entirely different results (i.e., different from the actual result, different from the result effected by the initial state mentioned first, and different from each other).

The extreme sensitivity of the results to minute variations of the initial conditions has a double effect: The equivalence regions are, first, intrinsically complex and, second, of quite the same size.[22] Thus we arrive at the following transformation of the above idea:

Any probability distribution over initial states mirroring our subjective standards of closeness and normality passes smoothly over the equivalence regions which are of bizarre shape but of equal size. Because these regions form an extremely — even infinitely — complex, criss-crossing filigree, every such probability distribution will in the end lead to the same probability distribution over the space of possible effects.

This is an *invariance argument*. On the basis of what seems to be an innocent mathematical assumption — the assumption of a kind of smoothness of the initial distribution — it becomes evident that the final distribution will correspond exactly to the respective numbers of balls in the urn. This can be attributed to objective, non-probabilistic features of the chance set-up. We have fifty white balls and fifty black balls, and we assume that there is no built-in mechanism producing any bias. For every initial probability distribution mirroring our subjective standards of closeness, then, each ball's probability of being drawn in a single drawing will be 1:100. The probability that a white ball is drawn in a single drawing will be 50:100, and so on.

The objectivization of the final probabilities of our drawings is due to the fact that all kinds of well-behaved subjective distributions over initial conditions lead — after sufficient mixing — to the same resultant distribution. This again is due to the chaotic behaviour of the system. What we get in the end are single-case probabilities[23] which are objective in the sense of being intersubjective but not in the sense that they are due to irreducibly indeterministic processes.

4 Maxwell and Poincaré: Some Historical Remarks

As pointed out by Krüger ([18]), James Clerk Maxwell was perhaps the first to perceive very clearly that the natural sciences in the middle of the 19th century

22 Cf. von Smoluchowski's ([47], p. 87) somewhat technical definition of what it means that a "causal relation" $y = f(x)$ has "'oscillating' character".

23 Von Smoluchowski ([47], p. 85) uses the term "Wahrscheinlichkeit schlechthin" ("probability as such").

were about to apply two methods which were, on the face of it, fundamentally different. We quote this at full length:

> The modern atomists have therefore adopted a method which is, I believe, new in the department of mathematical physics, though it has long been in use in the section of Statistics. When the working members of Section F get hold of a report of the Census, or any other document containing the numerical data of Economic and Social Science, they begin by distributing the whole population into groups, according to age, income-tax, education, religious belief, or criminal convictions. The number of individuals is far too great to allow of their tracing the history of each separately, so that, in order to reduce their labour within human limits, they concentrate their attention on a small number of artificial groups. The varying number of individuals in each group, and not the varying state of each individual, is the primary datum from which they work. This, of course, is not the only method of studying human nature. We may observe the conduct of individual men and compare it with that conduct which their previous character and their present circumstances, according to the best existing theory, would lead us to expect. Those who practise this method endeavour to improve their knowledge of the elements of human nature in much the same way as an astronomer corrects the elements of a planet by comparing its actual position with that deduced from the received elements. The study of human nature by parents and schoolmasters, by historians and statesmen, is therefore to be distinguished from that carried on by registrars and tabulators, and by those statesmen who put their faith in figures. The one may be called the historical, and the other the statistical method. ([26], pp. 373–374)

In an unpublished paper of the same year, Maxwell ([27], p. 438) elaborates on the distinction of the two methods, now called "two kinds of knowledge, ...the Dynamical and the Statistical".[24]

What is interesting is that Maxwell establishes a connection with the distinction between "historical" and "prophetical" inquiry, and with the contrast between stable and unstable systems. He calls a system *unstable* "when an infinitely small variation in the present state may bring about a finite difference in the state of the system in a finite time" ([27], p. 440); otherwise it is called *stable*. In all practical applications of physics, the *metaphysical axiom* "that from the same antecedents follow the same consequents" has to be replaced by the *physical axiom* "[t]hat from like antecedents follow like consequents". Similar distinctions were to be drawn in the 20th century under the names "'weak' determinacy" vs. "'strong' determinacy" by Born ([3], p. 80), "metaphysical determinism" vs. "'scientific' determinism" by Popper ([38], § 1), and "weak

24 The distinction has been a subject of lively discussion ever since. See for instance Planck ([30]) and Lévi-Strauss ([20], 528–531).

principle of causality" vs. "strong principle of causality" by Deker and Thomas ([5]).[25] The central role of the strong axioms in everyday reasoning as well as science has often been stressed, for example by Quine:

> Every reasonable expectation depends on resemblance of circumstances, together with our tendency to expect similar causes to have similar effects. ([42], p. 117)

and by Gleick:

> Scientists marching under Newton's banner actually waved another flag that said something like this: Given an *approximate* knowledge of a system's initial conditions and an understanding of natural law, one can calculate the *approximate* behavior of the system. ([9], p. 117)

In unstable systems and in systems near some singularity, Maxwell's "physical" axiom is simply false. Maxwell wants to undermine the common "prejudice in favour of determinism" ([27], p. 444), but he does not outright reject determinism. It can be conceded that metaphysical determinism is a doctrine which is not scientifically testable (because we never meet *exactly* the same initial conditions twice) and perhaps is altogether useless for the advancement of science. Nevertheless, it is a substantial philosophical doctrine for the merits and demerits of which it is interesting to argue — on the basis of current scientific knowledge.

Maxwell does not make a sustained effort at explicating the characteristics of the statistical method and its precise relation to the historical/dynamical method, nor does he argue in support of a linkage between this fundamental distinction and his emphasis on instabilities.

But as already mentioned, I think that Poincaré's method of arbitrary functions answers the questions surrounding Maxwell's diagnosis very generally and with surprising effectiveness. His paradigmatic example is an idealized roulette, *i.e.*, a game of chance, but he suggests that the method has a much wider field of application. In connecting dynamic and statistical reasoning it is first important what to count as chance, or a "fortuitous phenomenon":

> A very small cause which escapes our notice determines a considerable effect that we cannot fail to see, and then we say that that effect is due to chance. ... it may happen that small differences in the initial conditions produce very great ones in the final phenomena. A small error in the former will produce an enormous error in the latter. Prediction becomes impossible, and we have the fortuitous phenomenon. ([37], pp. 67–68)

This picture clearly reconciles Laplacian determinism with the seemingly random result of the drawings of our urn experiment. It must be stressed that

25 The predicates "weak" and "strong" are slightly misleading, since the strong principle does not logically entail the weak one.

Poincaré does not at all renounce determinism.[26] He rather suggests a kind of reducibility of chance to mechanistic patterns, or more exactly, the resolution of *the appearance of chance* in a deterministic world picture.

Why is this a solution of the problem posed by Maxwell and recently reinforced by Krüger? What has this achievement got to do with the question of whether it is reasonable to talk of "constant causes"? The latter term is from the realm of the statistical method. Poincaré in effect showed how a "structural" (physical, abstract, non-probabilistic) description of the system under consideration (in our case, of the urn containing white and black balls) can "explain" or "cause" the expected probability distributions of the results of single, independent drawings. As the explanation consists in the calculation of trajectories in the state space, causation is to be understood essentially in terms of *dynamical processes*. The final step leading from probabilities of single drawings to the constancy of statistical behaviour can then be taken with the help of the mathematical probability calculus. It will tell us that the colour ratio in the sequence of drawings will *very probably* be *approximately* equal to the single case probabilities. Notice that in accepting this as a causal explanation, we tacitly make use of a fifth concept of explanation based on the ***high probability*** of an *imprecisely specified effect*. This concludes our rational reconstruction of Laplace's doctrine of constant causes.

5 Essentials of Chaos Theory

The attention paid by Maxwell to instabilities and singularities is a symptom of the increased interest of physicists of the time in complex dynamical systems.[27] By the end of the 19th century, it was not only Poincaré who contributed to important progress in the field. Sofya Kovalevskaya ([16]) and Aleksandr Lyapunov ([24]) wrote classic papers on the concept of stability in dynamical systems, and Jacques Hadamard ([12]) proved a result which is made chief witness for the impact of unpredictability in classical physics by Duhem ([6], Chapter 7) and Popper ([38], § 14).

However, I do not think that the work of Maxwell, Poincaré, and others actually contradicts the Laplacian conception of the world. There is only a change of emphasis. While Laplace may be understood as implying that stable

26 This is very explicit in Poincaré ([37], pp. 64–65, 70). A similar view is expressed by von Smoluchowski ([47], p. 80).

27 Maxwell may have been influenced by Barré de Saint-Venant (cf. [11], p. 464). Maxwell ([27]) could be read as a proclamation of the programme of chaos theory, were it not for the fact that his principal concerns were the mass phenomena of the statistical theory of gases. However, he does refer to one very simple system exhibiting chaotic behaviour in Maxwell ([27], p. 442): the singularity in the refraction of light in a biaxial crystal.

and predictable systems are the rule and instabilities the exception, Maxwell and in particular Poincaré become aware that the truth is rather the other way round. It is the merit of chaos theory that this insight has gained widespread recognition today.

Modern chaos theory has a definite starting point. It began in 1963 with the publication of Edward Lorenz's article on "Deterministic nonperiodic flow" in the *Journal of the Atmospheric Sciences.*[28] Since the '80s at least, chaos theory has been very fashionable and attracted the interest of almost every scientific discipline. For our purposes a knowledge of chaos theory provided by good popular presentations such as Deker and Thomas ([5]), Crutchfield et al. ([4]), Jensen ([14]), or Gleick ([9]) is sufficient. Some of the most essential characteristics of chaos theory are the following:

- Even very simple deterministic systems can exhibit an incurably unpredictable behaviour, and this not only at a few isolated points of singularity but over a wide range of possible states. It does not help to gather more information about the initial conditions of such "chaotic" systems. A perfectly exact statement of the initial conditions cannot be obtained (uncertainty limitation), and an analytical solution of the equations involved is impossible (complexity limitation). Even the smallest errors introduced by incomplete knowledge or approximation techniques tend to have disastrous consequences for the calculation of the further development of the system.
- Very many real-life systems function as chance devices, by amplifying minute differences into extensive macroscopic effects. Iterative processes, like collisions in the mixing of the balls in our urn, lead to an exponential amplification of initial differences.
- The development of chaotic systems is extremely sensitive to the smallest changes of parameters or initial conditions; like causes do not have like effects. When the development of individual cases (as opposed to mass phenomena) is considered, one must not assume that small perturbations ("variable causes") cancel out in the long run. On the contrary, they will quite often build up to clearly perceptible effects. Maxwell's physical axiom is violated, and *a fortiori* Popper's ([38], § 3) "principle of accountability" becomes a demand that cannot be met.
- Many strictly deterministic systems exhibit what appears to be perfectly random behaviour.[29] Thus determinism and predictability, which had so often been taken to mean the same thing, get separated. It is only the latter

28 In so far as he insists on the unpredictable behaviour of very simple deterministic systems, one could think of calling Max Born a forerunner of modern chaos theory. However, one should pay attention to the fact that while Born dissociates causality from determinism (cf. [1], p. 78) but not predictability from determinism, the upshot of chaos theory goes just the other way round: its concept of causality is reflected in deterministic difference or differential equations, but predictability is emphatically denied.

which should be associated with the concepts randomness and chance. As a consequence, there is no incompatibility between determinism and chance.

- Chaos theory tells us that the geometric structure of equivalence regions in the sense specified above is extremely complex and interlaced, that it exhibits self-similarities on every scale of magnification, in short: that it forms a fractal structure.
- Recent high-powered computers make it possible to run fascinating simulations — although these simulations are just ridiculous sand-table games as compared to the complexity encountered in real life.

The insights are not *so* new after all, just the emphasis is. Again it seems to me that there is no contradiction between the views of Laplace and the new chaos-theoretic ideology. The difference is one of interests: Whereas Laplace still struggled with the implications of Newtonian mechanics and confined himself to aspects of the orderliness of nature, Poincaré and his followers had mathematical means and vigour to attack all the complexities of chaos. Poincaré actually *worked* on concrete chaotic systems in celestial mechanics (notably the three-body problem) and the theory of turbulent fluids, without at first caring to provide a general "philosophy" of chaos. But as we have seen, even before he made his renowned contributions to concrete problems of dynamical systems, it was Maxwell who had indicated the way to go.

What does chaos theory contribute to the solution of our problem? I should like to argue that this strong theoretical movement, and the powerful computational resources now available, help us understand the importance and ubiquity of "chaotic" phenomena. It transforms the casual discussions of roulette examples at the turn of the century into a respectable approach which is applicable to an enormous variety of real-life processes at every scale of magnification.

Even in apparently simple systems governed by simple deterministic laws and a small number of parameters, prediction is not possible over any reasonably long period. Equivalence regions leading to identical results do not form "natural" classes of points in the state space. The objective similarity relation — in fact: equivalence relation — engendered by identical outcomes of apparently random processes, such as the drawings from an urn, does not correspond to any subjective similarity relation among the initial conditions.[30] This does, however, by no means preclude the possibility of *ex post facto* explanations once the event in question has happened.[31]

29 It is important that this kind of randomness, although measurable in terms of probabilities, is *not* a measure of ignorance. "The randomness is fundamental; gathering more information does not make it go away." ([4], p. 38). The same point is made by Prigogine and Stengers ([39], p. 263). *Absolute* precision — the fixation of absolutely correct values of the relevant parameters — would help, but it is unattainable.

30 This moral may in many cases be transferred into the social sciences and the humanities. Cf. Poincaré's ([37], pp. 86–87) insightful remark on historiography.

6 Conclusion: What About Probabilism in a Deterministic Framework?

Is there a fundamental role played by probabilities in scientific explanation within the framework of classical physics? Do we really overthrow the traditional tenet that probability in classical physics just mirrors the degree of our ignorance?

Before trying to answer these questions, let us consider the following quotation:[32]

> probability considerations and causal determination are complementary; the former are only admissible where the latter has not (yet) succeeded. Lawlike connections must be absent or destroyed, e.g., by mixing the cards or shaking the die, in order to make room for valid applications of probability theory. Fries appears to have seen that statistical regularities, far from being explainable in terms of causes, are not even compatible with the possibility of a complete regulation of the relevant events by determining laws. ([18], p. 68)

Contrast this with Poincaré's ([37], pp. 66–67) remarks on life insurance companies and indiscreet doctors, and von Smoluchowksi's ([47], p. 82) remarks on the kinetic theory of gases. Against Laplace, these authors argue that judgements of probability may remain valid even after our ignorance is removed. Let us see how Poincaré construes the link between deterministic causality and the concept of chance:

> ...then we say that this event is due to chance, and so the word has the same sense as in the physical sciences; it means that small causes have produced great effects. ([37], p. 87)

or Marian von Smoluchowski[33] :

> Man nennt *Zufall* eine spezielle Art von Kausalrelationen. Man sagt nämlich gewöhnlich, daß ein Ereignis y vom Zufall abhängt, wenn es eine solche Funktion einer veränderlichen (eventl. auch ihrem Werte nach unbekannten oder absichtlich ignorierten) Ursache oder Teilbedingung x ist, daß *sein Eintreten oder Nichteintreten von einer sehr kleinen Änderung des x abhängt* ... ([47], p. 86)

31 This opens, I believe, an interesting perspective on much-debated issues in the theory of scientific explanation, e.g., Scriven's syphilitic (see [52], pp. 215–216, 978–982) and Rosen's golfer (see [53], pp. 159–160).

32 Krüger refers to the following passage of Jakob Friedrich Fries ([7], p. 3): "Diese ganze Berechnungsweise der subjectiven mittleren Wahrscheinlichkeit *a posteriori* [by the law of large numbers, HR] hat immer die Voraussetzung im Hintergrunde, dass in ihrem Bereiche keine nothwendigen Naturgesetze gelten, sondern immer noch möglicherweise ein Spielraum für den Wechsel unbekannter gleich möglicher Fälle bleibe."

Poincaré and von Smoluchowski, and our above discussion, seem to indicate that Krüger's position in the quoted passage is untenable. Still our objectivization program for final probabilities seems insufficient. Some philosophers might insist that the initial probabilities should have an objective meaning. One solution is provided by results of ergodic theory. ([2]; cf. [34])

Under some quite moderate conditions characterizing a chaotic dynamical system, the time-average of the system's being in a particular state is in the long run the same for almost every conceivable initial condition.

This is a mathematical result, but is it really a solution of our problem? First question: Is it *legitimate*, in the context of our problem, to identify these time-averages (which follow from the physical specification of the chaotic dynamical system) with the probabilities over the set of conceivable initial states? Von Plato seems to suggest that the answer is 'yes.' For this reason, he claims, ergodic theory is successful in replacing subjective probabilites (as underlying the ensemble approach of Gibbs) by objective probabilities (specified by Boltzmann's time-average approach). Unfortunately, I do not see the rationale of that replacement — apart from the fact that it avoids subjectivity.

But then there is also a second question: Should we *try* to get rid of the subjective probabilites over the initial conditions? I think the answer is 'no.' Our ideas of similarity and normality are irreducibly subjective, and we can without any reluctance base our expectations about the outcomes on subjective probabilities. The reason is quite directly given by Poincaré and spelled out in our reasoning above: In chance-producing deterministic systems, it simply does not matter on which initial distribution we base our calculations, as long as this distribution is reasonably smooth ("*practically* continuous", [37], pp. 82–83[34]). Thanks to chaos, virtually all differences of initial state distributions equal out in the long run.

What I take to be the main lesson from Poincaré's proof and all its later refinements is this:

In games of chance, but also in a great variety of less artificial applications, *an extreme sensitivity of effects to changes of initial conditions (i.e., of the causes) entails an extreme insensitivity of the probability distribution of*

33 Compare Laplace ([19], p. xlvii): "Au milieu des causes variables et inconnues que nous comprenons sous le nom de *hasard*, ..." Still thirty years earlier, it was Kant who very strictly tied his notion of "Zufall" to causality: "Daß gleichwohl der Satz: alles Zufällige müsse eine Ursache haben, doch jedermann aus bloßen Begriffen klar einleuchte, ist nicht zu leugnen" ([15], B 289–290). Whilst we are at pains to argue that *even* what appears to happen by chance *may* have purely deterministic causes, for Kant the link between "Zufall" and causal determination is analytic! Kant's "Zufall", however, must be translated by "contingency" or "conditionality" rather than by "chance" or "'randomness".

34 That it is a non-mathematical sense of continuity which is required was already pointed out by von Kries ([17], p. 51, footnote 1).

effects to the probability distributions of initial conditions (of the causes).

Those readers who would like to see the relevance of this paper to "causal logic" — which is the unifying topic of the present volume — may call this principle leading from individual chaos to collective order a principle of the *probabilistic logic of deterministic causality*.

Last but by no means least, how can this lesson be brought to bear in the social sciences? I do not have anything like a conclusive answer. On the face of it, it does not seem too implausible to suggest that many of our decisions are influenced by so many contingencies that they have the appearance of random behaviour. Minute differences in some inconspicuous boundary conditions might well have effected enormous differences for the rest of your life.[35] However, statisticians once and again observe amazing constancies across large populations. If this is due to some well-behaved distribution of initial conditions, aren't we back then to the question of how the distribution of initial conditions should be explained? Can the patterns of such an explanation be the same as those encountered in games of chance?

The purpose of this paper has been to point out the compatibility of metaphysical determinism with a fundamental role of probabilities in scientific accounts of causation and explanation. I have advanced no arguments in favour of determinism, but I have tried to block a possible argument against determinism from a probabilistic world view. There is a bridge between the *dynamical* or *historical* account of causation (which seems to be about single cases or "tokens") and the concept of causation as understood in statistics (which seems to be about collections or "types"). Like many writers, I have emphasized the crucial role played by the fact that in most systems of interest, we confront a complex interaction of multiple causal factors and an extreme sensitivity to small variations of initial conditions.

We have to be aware, though, that there is a basic philosophical problem which remains unsolved. What do the smooth probability distributions over initial conditions really mean, and where do they come from? Poincaré, the inventor of the method of arbitrary functions and the precursor of modern chaos theory,[36] did not find a satisfactory answer. In *Science and Hypothesis* ([36], pp. 192, 195, 200, 210) he invokes the principle of sufficient reason, an aprioristic idea which is not even well-defined (see section 3). In *Science and Method* ([37], pp. 83, 85) he appeals to the historical tendency of the world towards smoother and smoother probability distributions, and thus invites the question at which point in history the development of the probabilities should be taken to start.

In the present paper I have advocated a subjective interpretation of initial

35 Remember how you came to know your partner in life.

36 It is amazing that the literature on Poincaré does not establish a connection between these two achievements of his. — Also recall footnote 1.

single case probabilities which is to reflect not uncertainty, but a combined measure of similarity-to-the-actual-state-of-affairs and normality. This makes the objectivity that was gained in the course of our argument just an intersubjective agreement, and falls short of something objective in the sense presupposed by natural scientists. But I do not see how a workable objective interpretation could be obtained. We cannot simply count relative frequencies in order to find the initial probabilities, because empirical frequencies are just the things we set out to explain, and we would find ourselves trapped in an infinite regress. We cannot employ limits of time averages of sojourn, even when they exist and are unique, because there is no justification for taking these single-system probabilities to be probabilities of initial states. Finally, it seems to me that a propensity interpretation of probabilities does not help us either, since it is hard to understand and seems incompatible with the dynamic point of view we wanted to rescue. [37] Most likely quantum theory will have the last word in the interpretation of probability. But even in the more humdrum domain of classical Newtonian—or Laplacian—physics, not to speak of the domain of psychological and social phenomena, there are deep riddles left for the philosopher's acumen.

Acknowledgements

I should like to thank Ole Olsen, Uwe Scheffler, Max Urchs and in particular Weyma Lübbe for literature hints and stimulating discussions on structural causation. Thanks are also due to Igor Urbas for checking my English.

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37 As Popper ([38], pp. 95, 105) is ready to admit, his propensity account explains "the known by the unknown".

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