

## Chapter 4

# Interpretations of probability

### 4.1 Introduction

In studying the foundations of statistical mechanics a major problem that springs up time and again is how to connect the formal machinery of phase spaces and probability measures defined on them on the one hand, and the empirical world on the other. Investigating these connections, and thus interpreting the theory, is a difficult and controversial topic especially because of problems relating to the interpretation of probability.

For any application of the formal theory of probability an interpretation should be given of the probabilistic terms that occur in the theory. The formalism of probability theory itself of course does not provide this; it just gives theorems valid for all probability assignments that are allowed by its axioms. The formalism is necessarily silent both on the empirical meaning of statements containing probabilities, and on how probabilities ought to be assigned. This is the realm of the interpretation of probability. Thus, the task which we set ourselves when investigating the interpretation of probability in SM is, first, to give the (empirical) meaning of the probability functions that occur in the formalism of the theory. A second task is to justify the use of particular probability distributions (the Gibbsian ensembles). This chapter is devoted to the first of these tasks: determining the meaning of the probability distributions  $\rho(x)$  on phase space. Part III will be devoted to the second task. Specific interpretational problems for statistical mechanics arise due to the fact that the micro-evolution obeys deterministic laws. This means that the time evolution of a single system cannot be considered to yield independent trials of a chance experiment, at least not without very special conditions on the dynamics.

Methods of application of probability theory differ not only in the meaning they ascribe to the probability measure  $P$ , but also in the choice of the two other ingredients of a probability space: the sample space  $\Omega$  and the algebra  $F$ . In statistical mechanics, probabilities can be defined either on the different possible states of a total system (that is, on  $\Gamma$ -space) or on states of the particles within a system (that is, on  $\mu$ -space). In the following the sample space will be  $\Gamma$ -space and not  $\mu$ -space, since the latter plays a role only in the Boltzmannian approach to statistical mechanics. The algebra will be the standard Borel algebra, but other algebras are sometimes used, for instance as the expression of a coarse graining procedure.

I shall divide the interpretations of probability into three main groups: objective, intersubjective and personalistic interpretations. Objective probabilities are properties of physical systems. Perhaps these systems are large collections of, say, coins; or perhaps they include an environment of experimental conditions. In any case the probabilities are “in the world”, and measuring them involves doing physical measurements. Personalistic probabilities do not represent a property of a physical system, but a personal belief. Intersubjective probabilities represent a degree of credibility which depends on the information that is available about the system. Unlike personalistic probabilities, these are not supposed to vary from person to person (unless they have different information at their disposal).

The best way to illustrate the differences between the various interpretations of probability is perhaps through the ways in which probability statements can be tested. Take the statement that a certain coin has a probability of 0.55 to land on heads. In objective interpretations, a test would consist of measurements on the coin (coin tosses are the obvious choice, but other kinds of measurement can also be considered), or on the collection from which the particular coin is taken. In intersubjective interpretations, the probability statement can be tested only by checking whether the probability value is a correct representation of the information that is available about the coin. The behaviour of the coin itself does not play a role; it may well happen that it does not behave as could be expected from the available information. Finally, in the personalistic interpretation probabilistic statements can be tested by betting on the outcome of the coin toss, or simply by introspection if it is one's own probability assignment that has to be tested.

In the next sections I will discuss several interpretations of probability as far as they are relevant in the context of Gibbsian statistical mechanics. I will give an overview of the most important facets of the three main groups of interpretations of probability, thereby giving special attention to the treatment of equilibrium. The question is then: How should the fact that a system is in phenomenological

equilibrium be reflected as a property of the probability distribution? Because the interpretations differ with respect to the relation between probability and empirical data, also the answer to this question may differ. This means that the details of the definition scheme for equilibrium, which were left open in the previous chapter, will depend on the interpretation of probability.

I will not plead for a particular interpretation of probability. The question what the right interpretation of probability is, is in my opinion wrong-headed. Also, an interpretation need not be universally applicable. Rather, the task in interpreting probability is to provide a coherent interpretation for each field in which probabilities are used. Different interpretations may be used in different fields of application. It may even be that in a single field several interpretations of probability are tenable. This can only be fruitful for the field of application in question, since in this case its theorems are true with several meanings of the probabilistic terms, making the theory richer.

## 4.2 Objective probabilities

There are two main objective interpretations of probability: the propensity interpretation, and the frequency interpretation. Propensities (defended by (Popper 1959)) are a sort of dispositions; they can be attributed to a single trial or experiment. For instance, that a coin has probability one half of showing up heads means, on this account, that it has an internal tendency to show up heads in one half of the trials (in the long run). This tendency is a dispositional property; it applies also if the coin isn't actually tossed (or if it isn't tossed infinitely often). Our discussion of the propensity interpretation can be short, since in a deterministic context it is hard to uphold the standpoint that probability can be viewed as a purely intrinsic property. Indeed, the general opinion is that the propensity interpretation can only be used in genuinely stochastic theories. Therefore its main field of application is quantum mechanics, since this theory is thought by many to involve irreducible chances.

In the frequency interpretation (with Von Mises as its famous founding father (Von Mises 1931)) probabilities are identified with relative frequencies in long series of independent repetitions. Probabilities are construed as properties of such repeated experiments, or generally, of mass phenomena, and not of single events as in the propensity interpretation. The sequence is usually taken to be infinitely long, since otherwise irrational probability numbers would be impossible. (See (Hajek 1996) for fourteen other arguments against finite frequentism). The price to pay is that this involves some idealisation, because we never observe infinite sequences.

The condition of independence deserves some further consideration. It is introduced here as a primitive notion. That is, one should know whether repetitions are independent before one can speak of probabilities. In other interpretations of probability it is exactly the other way around. There the notion of independence can be defined in probabilistic terms.

Within statistical mechanics there are two ways of looking at repetitions. First, repetitions can have reference to a single system at different moments in its evolution in the course of time. Secondly, repetitions can relate to identically prepared, but different systems. In other contexts, for example in coin tossing, there isn't much difference between these two kinds of repetitions. It doesn't matter whether we repeatedly toss the same coin, or whether we toss different coins. At least, this doesn't matter if all coins are fair (or bent in the same way), but this is guaranteed by the condition of identical preparation. However, for dynamical systems there is a big difference between the two kinds of repetitions, because the time evolution of a single system is governed by deterministic laws. This means that repetitions in the course of time are not independent, whereas repetitions on different identically prepared systems are.

The standard frequency interpretation in statistical mechanics starts from the second kind of repetitions, i.e. those regarding different, identically prepared systems. In statistical physics such a collection of systems is called an ensemble. A well-known example is a collection of systems that are all in contact with a heat reservoir (or thermostat) of a certain fixed temperature, and that all have equal values for external parameters such as volume and electromagnetic fields. This is called a canonical ensemble. The members of this ensemble all agree with respect to the mentioned (macroscopic) properties, but they differ with respect to their exact microscopic state. Then  $\rho(x)dx$  gives the probability to find a system in microstate  $x$  when drawn aselectively from the ensemble, where  $\rho(x)$  in this example is the canonical distribution.

### Frequency interpretation and equilibrium

Now it is time to ask how equilibrium should be represented in the frequentist interpretation of probabilities in statistical mechanics. The question is as follows: How should the fact that the system is in phenomenological equilibrium be reflected as a property of the probability distribution? We can already say that the standard answer, namely that the distribution should be stationary under the Hamiltonian evolution, does not follow as a necessary consequence of the condition of phenomenological equilibrium. This condition only says that there are no changes taking place

on a macroscopic level. This leaves open the possibility that there are variations on a microscopic level. Also, and more importantly, it leaves open the possibility that there are variations in the composition of the ensemble, and thus in the probability distribution.

So, within the frequency interpretation of probabilities in statistical mechanics which we are considering here, the condition of phenomenological equilibrium does not lead necessarily to a stationary probability distribution. But can we give a positive characterisation of the properties a probability distribution should have, given that the system is in phenomenological equilibrium? Here is a naive argument. Since we are dealing with objective probabilities, the probability distribution is an objective property of the system. The fact that the system is in phenomenological equilibrium means that all of its observable properties do not change in time. Therefore the probability distribution must be constant.

This argument is too simple, because not all objective properties are also observable. But we can say something more by looking at the systems that constitute the ensemble. By definition, all members of an ensemble are identically prepared. Thus, they are all subject to a common macroscopic constraint, such as being in contact with a thermostat in the case of the canonical ensemble. In our case it is natural to consider the condition of phenomenological equilibrium as a macroscopic constraint. This means that every member of the ensemble is subject to the condition that the observables  $F \in \Omega$  are nearly constant, as expressed in definition 3.2. Now the natural way to proceed is to regard the observables  $F$  in the definition of  $\varepsilon$ -equilibrium as phase functions, and then to investigate the consequences for the ensemble as a whole. But if for all members of the ensemble the values of the phase functions  $F$  differ at most  $\varepsilon_F$  from a common constant value, then certainly the ensemble averages of those functions are nearly constant in time as well. Thus, we have

$$\forall F \in \Omega \quad \exists c_F \quad \forall t \in \tau \quad |\langle F \rangle_{P_t} - c_F| \leq \varepsilon_F, \quad (4.1)$$

as a necessary characteristic of phenomenological equilibrium in the frequency interpretation. Note that from the argument above even stronger conditions may be derived; (4.1) allows for exceptional members of the ensemble with values of  $F$  differing more than  $\varepsilon_F$  from  $c_F$ , whereas the argument above excludes this possibility.

### Time average interpretation

The time average interpretation is in many respects similar to the frequency interpretation, but with repetitions understood in the sense of the time development of a

single system. The distinctive feature is thus that the repetitions are determined by a deterministic process. A proponent of the time average interpretation is Von Plato (see his ‘Probability in dynamical systems’ (Von Plato 1989), in which he compares this interpretation with the usual frequentist interpretation).

In the time average interpretation the probability to find a system in a certain set in phase space is by definition equal to the infinite time average of the indicator function of that set:

$$P_{x_0}(A) = \lim_{T \rightarrow \infty} \frac{1}{T} \int_0^T \mathbb{1}_A(T_t x_0) dt. \quad (4.2)$$

Thus, the probability of the set  $A$  is equal to the fraction of time that the systems spends in that region, also called the sojourn time. Note that the probability function is labelled by the initial state of the system,  $x_0$ . In general different initial states lead to different paths in phase space, and therefore also the sojourn times may depend on  $x_0$ .

There are several problems with the time average interpretation, which in my view render it untenable. First, the fact that repetitions are determined by a deterministic process puts pressure on the condition that the repetitions should be independent. In fact, Von Mises is very clear that the time evolution of a single system does not build a *Kollektiv*, because one of the axioms of his theory of probability, the condition of random place selection (*Regellosigkeit*) is not fulfilled (Von Mises 1931, p. 519). Secondly, infinite time averages need not even exist! This follows from Birkhoff’s ergodic theorem (see appendix A.3). Third, as noted the probability of a set  $A$  depends on the initial state  $x_0$ , which is an awkward feature. Fourth, and in my view most importantly, there is no obvious way to extend the application of this notion of probability to time-dependent phenomena, and thus to the more general theory of non-equilibrium statistical mechanics.

According to Von Plato ergodic theory points to cases where (some of) the mentioned problems can be overcome and thus this particular interpretation can be applied:

‘the notion of ergodicity gives us a *characterisation* of cases where probability as time average, and frequentist probability more generally, can be applied.’ (Von Plato 1988) (Original italics).

Von Plato concludes that frequentist probability can only be applied to ergodic systems. Let’s look at the four mentioned problems.

Infinite time averages, if they exist, obey the axioms of Kolmogorov. But do they also satisfy the demands of frequentist probability? Especially the condition

of independent repetitions is very difficult to satisfy; this is the first of the above-mentioned problems. Whether the sampling is “unbiased”, or whether the trajectory can be seen as a sequence of independent repetitions of a random event, depends on the dynamics of the system. If the dynamics is metrically transitive (for the definition see appendix A.3), we have “asymptotically representative sampling” (in Von Plato’s words). Only at the top of the ergodic hierarchy, for Bernoulli systems, we have independent repetitions.

The second problem is that time averages need not exist. The first part of the ergodic theorem demonstrates the  $\mu$ -almost everywhere existence of infinite time averages. Thus, the existence of the probabilities as defined above is ensured for almost all starting points  $x_0$ , where “almost all” is measured in the Liouville measure.

The third problem is that time averages generally depend on the initial state. Ergodic theory shows that for metrically transitive systems, time average probabilities are equal to the microcanonical measure (again with the proviso of exceptions of  $\mu$ -measure zero). This means that in this case infinite time averages are independent of the initial state  $x_0$ . Metrical transitivity is however a dynamical condition that isn’t always met.

The fourth, and in my opinion most serious problem is that the time average interpretation cannot be generalised to time-dependent phenomena. Now Von Plato is very clear that one needn’t pursue a single interpretation of probability in all applications of probability theory, and I agree with him. But still it would be a strange state of affairs to be compelled to use different interpretations in the single context of statistical mechanics. Indeed, how could one make sense of the statistical mechanical description of non-equilibrium phenomena, say the approach to equilibrium?

Finally, related to this last point, what is the connection between equilibrium and properties of the probability distribution such as stationarity? It appears immediately from the definition (4.2) that time average probabilities are necessarily stationary under the transformations  $T_t$ . It is also clear that they are meant to be applied to systems that are in equilibrium, or to predict the behaviour in the long run of systems that are not yet in equilibrium. Thus, the connection between equilibrium and stationarity is imposed by definition from the start.

### **A digression: The laws of large numbers**

It is sometimes said that probability should be equated with relative frequencies as a result of the laws of large numbers (see Appendix A.4 for an exact formulation of those laws). Those laws tell us that in the long run the relative frequency of independent and identically distributed (i.i.d.) repetitions of a certain chance event

converges to the probability of that event. The weak law says that this convergence takes place “in probability”, which means that the probability goes to zero that the relative frequency and the probability of the event  $A$  differ by any amount greater than zero. The strong law is strictly stronger, and says that convergence takes place “almost surely”. This means that the event that the relative frequency of  $A$  converges pointwise to the probability of  $A$  has itself probability one. Are not these two laws telling us that the limit of relative frequency of a certain event is nothing but its probability, and aren't they therefore committing us to the frequency interpretation?

This would of course be a strange state of affairs. The laws of large numbers are nothing but theorems in the formal theory of probability. They therefore hold irrespective of the interpretation that is attached to the formalism. They cannot enforce a certain interpretation. The error that is made in the above argument is that it failed to recognise that the term “probability” occurs twice in the statement of the laws of large numbers. The interpretation of probability is not derived from the formalism alone, since an interpretation is given to one of these occurrences of the term probability through the backdoor. Consider for the moment only the weak law, which says that the *probability* goes to zero that the relative frequency and the *probability* of the event  $A$  differ by any amount greater than zero. In the above analysis the phrase ‘the probability goes to zero that (...)’ has silently been replaced by ‘it won't happen that (...)’. By doing this an interpretation has been added, rather than derived.

So the claim that the frequency interpretation follows from the laws of large numbers turns out to be wrong. But of course if this claim is dropped, the frequency interpretation can still be applied consistently, and the laws of large numbers can be formulated on this account of probability. Thus, we interpret  $P(A)$  as the limiting relative frequency of occurrence of event  $A$  in a long series of repetitions. The weak law now speaks of a series of i.i.d. repetitions of the experiment. This means that a “collective of collectives” should be considered, that is, a series of repetitions of certain experiments which themselves are series of repetitions. The phrase in the weak law saying that ‘the probability is zero that (...)’ now applies to this collective of collectives. The weak law thus states that in this collective of collectives the relative frequency of collectives in which the relative frequency with which  $A$  occurs differs from  $P(A)$  goes to zero. In this way both  $P$ 's in the statement of the weak law of large numbers are interpreted consistently in a frequentist manner. A similar analysis applies to the strong law.

### 4.3 Intersubjective probabilities

In intersubjective interpretations of probability, a probability distribution reflects the information that is available. For example, in coin tossing experiments the statement that the probability of heads equals one half may reflect information about the shape of the coin, or about the outcomes of previous tosses. In sharp contrast with the objective interpretations we encountered in the previous section, it is not an objective property, neither of the coin itself nor of a collection of coins.

The source of a probability distribution in this interpretation is ignorance. There would be no reason to invoke probability distributions over the possible states of a system if we had complete information about the state the system is in (as long as we deal with a deterministic context). If we knew the exact position of the coin in the hand that tosses it, and all the dynamical details of the flight and landing, and if we could calculate from those data whether it would land heads, the probability would be either zero or one.

There are several schools which all fall within the category of intersubjective interpretations of probability. The most important differences lie, not surprisingly, in the answer to the question how the information should be translated into probabilistic language. Some schools only prescribe how probabilities should be updated when new information comes in (Bayesianism); others also provide rules how to assign probabilities (logicism, Maximum Entropy Principle). In the following I will only discuss Jaynes's Maximum Entropy formalism (Jaynes 1978), since this is the most influential school within statistical mechanics.

Note that also the frequency interpretation can be twisted to become an intersubjective interpretation, if the ensembles are thought of as "mental copies" of a single system of interest, and not as a collection of systems that all exist in the real world.

#### The Maximum Entropy Principle

The Maximum Entropy Principle (MEP) gives a procedure to handle partial information and to pick out a probability distribution that represents this information. Of all possible probability distributions on a certain probability space

$$E = \left\{ \rho(x) : \int_{\Gamma} \rho(x) dx = 1 \text{ and } \rho(x) \geq 0 \right\} \quad (4.3)$$

first a subclass  $J \subset E$  is selected by making use of the data, and secondly one distribution from this restricted class is selected by maximising the Shannon entropy

$$H(\rho) = - \int \rho(x) \ln \rho(x) dx. \quad (4.4)$$

The idea behind maximising the entropy is that in this way the information is used in a “fair” way: Only the available information is used, and apart from that the probability distribution is chosen as much spread out as possible. Thus, the Maximum Entropy formalism is closely related to Laplace’s Principle of Insufficient Reason.

An important part of this MEP-procedure is of course the exact way in which data are used as constraints on probability distributions. Here Jaynes prescribes a rule which says that the value of an observable should be taken to fix the expectation value of that observable. So, if the values of  $m$  independent functions  $f_i(x) = c_i, i = 1, \dots, m$  are given, this restricts the class of probability distributions in the following way:

$$J_1 = \left\{ \rho(x) \in E : \int_{\Gamma} f_i(x) \rho(x) dx = c_i \quad (i = 1, \dots, m) \right\}. \quad (4.5)$$

A well-known result, obtained by the method of Lagrange multipliers, is the fact that the MEP-distribution that follows from this constraint is the exponential distribution

$$\rho_{MEP}(x) = \frac{1}{\mu(\lambda_1, \lambda_2, \dots, \lambda_m)} e^{\lambda_1 f_1(x) + \lambda_2 f_2(x) + \dots + \lambda_m f_m(x)} \quad (4.6)$$

with as important special case the canonical distribution in case  $f(x)$  is the Hamiltonian. The Lagrange multipliers are related to the constraint values  $c_i$  in the following way:

$$c_i = \frac{\partial}{\partial \lambda_i} \mu(\lambda_1, \dots, \lambda_m). \quad (4.7)$$

Jaynes applies his formalism not only to equilibrium but also to non-equilibrium statistical mechanics. In the latter case the time evolution of probability distributions is influenced by two mechanisms: the dynamical evolution, according to the Hamilton (or Schrödinger) equations, and updating when new information becomes available. The class  $E$  of allowed time-dependent probability distributions now only contains those distributions that satisfy the equations of motion. The constraint that picks out a subclass  $J$  may refer to different points in time. For example, it may consist of the expectation value of an observable at two different times. But when a constraint of this form is incompatible with the dynamical evolution, the MEP-scheme simply breaks down, as it should.

## The Maximum Entropy Principle and equilibrium

The maximum entropy principle (MEP) gives a clear procedure of how to assign values to probability distributions on the basis of empirical data. The data are treated as restrictions on the set of possible probability distributions. The connection between empirical input and probabilities is in fact presupposed, and not argued for (or not argued for convincingly; see (Uffink 1996)). However, it can be viewed as part and parcel of the MEP that data like the value of an observable should be translated into a constraint fixing the value of the expectation value of that observable. Thus, setting aside the justification of this procedure, there are no ambiguities as to how empirical data translate into conditions on probability distributions. In fact, after maximising the entropy (and when  $J$  is a convex set), a unique distribution will follow.

So what if the empirical data tell us that the system is in phenomenological equilibrium? The most straightforward thing to do is to use the equilibrium values of one or more thermodynamic observables as constraints, which results in an exponential MEP distribution as was discussed above. Indeed it is distributions of this type that Jaynes himself calls equilibrium distributions. However, by just feeding into the MEP algorithm the equilibrium values of observables the available information is not fully taken into account, since the fact that the observable has had this value for a longer period of time has not been used.

I take it that the best way to search for the MEP distribution that represents phenomenological equilibrium is to use the definition of  $\varepsilon$ -equilibrium (as given in section 3.5) with expectation values for the functions  $F$  to delimit the class of allowed probability distributions, and to take the maximum entropy distribution from this class. Thus, the constraint set is given by

$$J_{eq} = \left\{ \rho(x) \in E : \left| \int_{\Gamma} F(T_{-t}(x)) \rho(x) dx - c_F \right| \leq \varepsilon_F \quad \forall F \in \Omega \quad \forall t \in \tau \right\}, \quad (4.8)$$

and just as in the case of the frequency interpretation, the condition of phenomenological equilibrium is translated into a condition of nearly constant expectation values. However, the difference is that the MEP-procedure picks out a single distribution from the class  $J_{eq}$  (if the set is convex), namely the one with largest Shannon entropy.

The class  $J_{eq}$  differs from  $J_1$  in two respects. First, instead of a fixed value  $c_F$  for the expectation value of  $F$ , an interval of values around  $c_F$  is specified. Secondly, a time interval is specified in which  $\langle F \rangle$  has kept this value, necessitating the application of the general time-dependent MEP-scheme. In general this will amount

to a complicated calculation, but let us consider specific cases. With regard to the second point, Jaynes shows (but, to be sure, for the quantum mechanical case) that a constant value of an expectation value is redundant information in the sense that it drops out of the equations, and leaves the MEP-outcome unchanged (Jaynes 1978, p. 295). With respect to the first issue, let's consider a specified interval of values for the energy:  $a \leq \langle H \rangle \leq b$ . It can be calculated that the MEP-distribution will be the same as with constraint  $\langle H \rangle = b$ . This can be understood since the entropy is a concave function of  $\langle H \rangle$ . Thus, in the simplest example of (4.8) where an interval of energy values is specified in an interval of time, the MEP-solution again simply is a canonical distribution.

Can we conclude from this that in the MEP-formalism phenomenological equilibrium will always be represented by a stationary distribution? Unfortunately it isn't as simple as that. This is because not all exponential distributions are stationary! The canonical distribution is special in this respect, since the Hamiltonian is a constant of the motion. Remember the Liouville equation

$$\frac{\partial \rho(x, t)}{\partial t} = -\{\rho, H\}, \quad (4.9)$$

which implies that any distribution of the form  $\rho(H(x))$  is stationary. In more general cases, i.e. when the observables in the exponent of the MEP-distribution are not constants of the motion, the MEP-distribution may be non-stationary. We can therefore conclude that the condition of phenomenological equilibrium will not in general lead to a stationary MEP-distribution. There is also no general rule which tells us what an equilibrium distribution looks like. But for each individual case (depending on the format in which the constraint is given) there is an unambiguous procedure leading to a unique MEP-distribution.

Note that the first of our original problems with stationary distributions, namely the inconsistency with the dynamics, is not a problem for followers of Jaynes. In the maximum entropy formalism there are two different kinds of evolution of the probability distribution, one dynamical, given by the Hamiltonian or Schrödinger evolution, and one inferential, given by the procedure of maximising the entropy. These two kinds of evolution both play their role. It is no problem at all if some piece of information leads, via the MEP-procedure, to a distribution which could not be the result of a Hamiltonian evolution. There is no conflict here, since the individual phase trajectories do obey the Hamiltonian evolution.

## 4.4 Personalistic probabilities

In the personalistic interpretation, probabilities reflect personal beliefs about statements. When a person determines his belief he may be led by objective properties of the things those statements refer to, but these needn't play a role. As long as certain coherence conditions are satisfied, everyone is free to believe what he wants. In order to quantify degrees of belief usually the model of bets is used. The personal probability that a person gives to a certain statement is then equal to the amount of money he is prepared to pay, divided by the amount of money he will receive if the statement turns out to be true.

An important theorem for the proponents of the personalistic interpretation of probability is the so-called Dutch Book theorem. A Dutch Book is a set of bets which will inevitably lead to a loss, that is, independently of the outcome of the chance experiment. As an example, consider a person who is willing to bet 6 Euro on "heads", receiving 10 Euro if he wins the bet, and who is prepared to do the same for "tails". If he makes both bets at the same time, he is certain to lose 2 Euro, so the set of bets is a Dutch Book. The theorem says that no Dutch Book is possible iff the set of personal beliefs obeys Kolmogorov's axioms of probability theory. Indeed, in the example the sum of probabilities equals 1.2 while according to probability theory it should be exactly one.

We see that in any application of probability theory we can interpret the probabilities as personal degrees of belief of a rational agent; this is the term reserved for a person who will not accept a Dutch Book. Also within the context of statistical mechanics probability ascriptions can be interpreted as degrees of belief. Probabilities are attached to statements like 'The pressure inside this balloon equals 2 Bar' or 'The temperature of this cup of coffee will not decrease by more than 2 degrees within the next hour'. It is typical of the personalistic interpretation that those probabilities may change from person to person, although the balloon and the cup of coffee are the same for everyone.

### Personalistic probabilities and equilibrium

Let us now turn to the question how equilibrium should be accounted for in the personalistic interpretation. Von Plato writes:

'(...) the notion of stationarity is for an objectivist a natural correspondence, in probabilistic terms, of the idea of an unaltered random source. For a subjectivist, stationarity is even more easily at hand. He

needs only assume that his probabilities are unaltered in time' (Von Plato 1989, p. 421)

Indeed, a subjectivist may assume that his probabilities are stationary, but he is not compelled to do so.

Guttmann discusses the question how to justify the assumption of stationarity from a subjectivist (i.e. personalistic) point of view (Guttmann 1999, Ch. 2). The context of his discussion is an investigation into the use and applicability of a special part of ergodic theory, the ergodic decomposition theorem, in the personalistic interpretation of probability. Since this theorem holds only for stationary measures, Guttmann seeks a justification for stationarity. He argues however that there is no compelling reason for an agent to have stationary beliefs, and concludes that this specific ergodic approach cannot be combined successfully with a subjective interpretation of probabilities.

Guttmann discusses four different arguments for the stationarity assumption, which according to him all fail. They are i) the stationarity of the mechanism behind chance events, ii) Liouville's theorem, iii) a property of agents called tenacity and iv) another, more familiar property of agents, exchangeability.

The first argument says that if there are no changes in the source, such as magnets which are attached to a favoured number on the roulette wheel, then it may be expected that the probabilities also stay the same. This argument fails however for subjectivists because personal beliefs need not be such that they are stationary if the mechanism is. Perhaps I believe that someone cheats on me and makes changes to the roulette wheel, even if this is in fact not the case. Or perhaps I just believe today is my lucky day, so that my chances are better than yesterday even though the roulette wheel is exactly the same. In both cases the mechanism is stationary but my personal belief changes in time. The point is of course that I am free to believe what I want to believe.

The second argument says that it follows from Liouville's theorem that an equilibrium measure should be stationary. This argument is blatantly wrong. The theorem only shows that the microcanonical measure is stationary. It does not show that equilibrium should be represented by the microcanonical measure, or any other stationary measure.

The third and fourth arguments really are subjectivist arguments, since they refer to properties of agents. A *tenacious* agent is someone who is reluctant to change his (probabilistic) opinions. Tenacity is however not exactly equal to stationarity. This is because tenacity is an attitude of agents with respect to their own beliefs ("epistemic inertia"), whereas stationarity is a property of the beliefs themselves.

The argument is that agents should be tenacious, and that it follows that their degrees of belief are stationary. The first part is false because tenacity is not a coherence requirement. The second part is false because tenacity and stationarity are just different things.

If an agent has personal beliefs which are independent of the order in which random events occur, his degrees of belief are *exchangeable*. The fourth argument says that an agent should hold exchangeable beliefs about statistical mechanical systems in (phenomenological) equilibrium. Exchangeability implies stationarity, but not vice versa. In many contexts exchangeability is an acceptable property. For instance, in the case of coin tosses it is reasonable to believe that only the relative frequency of heads and tails, and not the order is important for their probabilities. Guttmann convincingly argues that the context of statistical mechanics is an exception, because of the deterministic rules that underlie the random phenomena. Therefore, also this last argument fails.

The general problem is that for subjectivists only coherence requirements are binding. There are no coherence requirements that enforce stationary (or nearly stationary) personal probabilities. Therefore, agents are free to hold non-stationary beliefs about systems in phenomenological equilibrium.

## 4.5 Ensembles and equilibrium

How should the fact that a thermal system is in phenomenological equilibrium be reflected as a property of the probability distribution? Let me briefly summarise the answers we have found to this question, for the different interpretations of probability.

First, for the frequency interpretation in which probability is regarded as a relative frequency in an ensemble of identically prepared systems, I have argued that equilibrium should be represented as

$$\forall F \in \Omega \quad \exists c_F \quad \forall t \in \tau \quad |\langle F \rangle_{P_t} - c_F| \leq \varepsilon_F. \quad (4.10)$$

Any distribution that obeys this relation represents phenomenological equilibrium.

Secondly, also in the Maximum Entropy formalism the probability distribution that represents phenomenological equilibrium obeys the above condition. Here however, the MEP formalism picks out one distribution from those obeying the condition, namely the one with maximal entropy.

The time average interpretation of probabilities does not possess the means to discriminate between equilibrium and non-equilibrium. All probabilities are sta-

tionary by definition on this interpretation. They are meant to be applied only to systems in equilibrium.

Finally, if probabilities are interpreted as personal degrees of belief, there is no generally valid characterisation of equilibrium, since beliefs can change from person to person, and there are no coherence requirements that fix a particular characterisation of equilibrium such as stationarity.