

# EQUILIBRIUM IN BOLTZMANNIAN STATISTICAL MECHANICS

*Roman Frigg and Charlotte Werndl*

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## 27.1 Introduction

A gas that is confined to the left half of a container starts expanding as soon as the confining wall is removed and eventually spreads evenly over the entire available space. The state of being spread evenly is the *equilibrium state* and the process of expansion culminating in that state is the *approach to equilibrium*. It is one of the aims of statistical mechanics (SM) to give an exact characterization of equilibrium, and to explain why and how systems approach the state of equilibrium in terms of the dynamical laws that govern the individual molecules of which the gas is made up of. What is it about molecules and their motions that lead them to spread when the wall is removed? An important answer to these questions was suggested by Boltzmann (1909 [1877]), and variants of this answer are currently regarded by many as a promising option. As is customary in the current literature on SM, we refer to the approach that originates in Boltzmann’s 1877 paper as Boltzmannian SM (BSM). In this chapter, we introduce different versions of BSM and discuss how they conceptualize equilibrium. Another approach that could be labeled “Boltzmannian” departs from the so-called Boltzmann equation. We set this approach aside and focus on what we call BSM.<sup>1</sup>

We begin by introducing the basic notions of BSM. A system in SM has the mathematical structure of a *measure-preserving deterministic dynamical system*  $(X, \mu, T_t)$ . In line with most of the literature on BSM we focus on deterministic systems; for a discussion of BSM from a stochastic point of view see Werndl and Frigg (2017a).  $X$  is the state space and it contains all possible *micro-states* of the system. In the case of an isolated gas in a box made up of  $n$  particles a micro-state is given by the positions and the momenta of every molecule in the gas. The measure  $\mu$  specifies how large certain parts of  $X$  are. We assume that the measure is normalized:  $\mu(X) = 1$ . This is for reasons of mathematical convenience and no connection between this measure and probability is assumed at this point. The time evolution of the system is given by the *evolution function*  $T_t : X \rightarrow X$  where  $t$  is time and the function satisfies the requirement  $T_{t_1+t_2}(x) = T_{t_2}(T_{t_1}(x))$  for all micro-states  $x \in X$  and all instants of time  $t_1$  and  $t_2$ . Time can either be continuous ( $t \in \mathbb{R}$ ) or discrete ( $t \in \mathbb{Z}$ ).<sup>2</sup> Intuitively this means that it does not matter whether the time-evolution is carried out in one go or takes place in stages: if the process begins in micro-state  $x$ , then we end up in the same final state  $y \in X$  irrespective of whether we evolve  $x$  forward in time by  $t_1 + t_2$  at once or whether we first evolve it  $x$  forward by  $t_1$  and then evolve the resulting state forward by  $t_2$ . The measure  $\mu$  is assumed to be invariant under the dynamics, meaning that  $\mu(T_t(A)) = \mu(A)$  for all measurable subsets  $A$  of  $X$  and all  $t$ .

The macro-condition of a system is specified by its *macro-state*. In the case of the gas this can be done, for instance, by specifying the volume, pressure and temperature of the gas. It is usually assumed that there are a finite number  $m$  of macro-states, and we denote these by  $M_i$ ,  $i = 1, \dots, m$ . The core posit of Boltzmannian SM is that macro-states supervene on micro-states, meaning that a change in the macro-state must be accompanied by a change in the micro-state (it is not possible, for instance, to change the pressure of a gas without also changing the state of motion of at least some of its molecules). Hence, to every given micro-state  $x$  there corresponds *exactly one* macro-state. This determination relation is not one-to-one; usually many different micro-states correspond to the same macro-state. One can now group together all micro-states that belong to the same macro-state  $M_i$ . The result of this grouping is the *macro-region*  $X_{M_i}$ . All  $X_{M_i}$  taken together form a partition of  $X$ , meaning that they do not overlap and jointly cover  $X$ .

The *Boltzmann entropy* of a macro-state  $M_i$  is defined as  $S_B(M_i) := k \log[\mu(X_{M_i})]$ , where  $k$  is the Boltzmann constant; the Boltzmann entropy of a *system* at time  $t$ ,  $S_B(t)$ , is the entropy of the macro-state of the system at  $t$ :  $S_B(t) := S_B(M_{x(t)})$ , where  $x(t)$  is the micro-state at  $t$  and  $M_{x(t)}$  is the macro-state supervening on  $x(t)$ .

Among the macro-states of the system two are of particular importance, namely the equilibrium state and the macro-state at the beginning of the process, also referred to as the “past state.” We introduce the special labels  $M_{eq}$  and  $M_p$  for these states (and choose a labeling of macro-states so that  $M_p = M_1$  and  $M_{eq} = M_m$ ). A crucial aspect of the standard presentation of BSM is that the equilibrium macro-region  $X_{M_{eq}}$  is vastly larger than any other macro-region. In fact  $X_{M_{eq}}$  is so large that it takes up most of  $X$ .<sup>3</sup> However, Lavis (2005, pp. 255–258, 2008, pp. 685–687) points out that in some situations the equilibrium macro-region is *larger than any other macro-state* without taking up most of  $X$ . We set this problem aside, but it is discussed in Werndl and Frigg (2015b). The size of the macro-region is generally seen as the crucial feature of equilibrium.

This raises two questions. First, why is equilibrium associated with the state that has the largest macro-region? The connection between equilibrium and large macro-regions is certainly not analytical: there is nothing in the *concept* of equilibrium tying it to the macro-state with the largest macro-region. Second, isolated systems, when left to themselves, end up in the equilibrium state. In our initial example the gas spreads until it fills the container evenly. Why do systems approach equilibrium? We discuss these problems in the next two sections.

## 27.2 Definitions of Equilibrium

Why is the equilibrium macro-state the state with the largest macro-region? A prominent answer, now known as the *combinatorial argument*, originates in Boltzmann (1909 [1877]).<sup>4</sup> The basic principle at work in Boltzmann’s argument is that equilibrium is the macro-state that is compatible with the largest number of micro-states. Based on this notion Boltzmann constructs  $X_{M_{eq}}$  as follows. Consider the same system of  $n$  identical particles as above, but now focus on the phase space  $\chi$  of *one* of these particles (the space has six dimensions – the three position and three momentum coordinates of the particle). Only a finite portion  $\chi_a$  of  $\chi$  is accessible to the particle because the motion of the particle is constrained by the container walls and the total energy  $E$  of the gas. Now put a regular grid on  $\chi_a$  such that the grid lines run parallel to the position and momentum axis, thereby dividing  $\chi_a$  into a finite number of cells of equal size  $\delta\omega$ . This grid is also known as a coarse-graining of  $\chi_a$ . Now label the grid cells  $\omega_i$  for  $i = 1, \dots, l$ . The so-called coarse-grained micro-state of a particle is given by specifying in which cell  $\omega_j$  its micro-state lies. The micro-state of the entire gas is a specification of the micro-state of every particle in the system, which is therefore determined by  $n$  labeled points in  $\chi_a$ . The coarse-grained micro-state of the gas, also known as an *arrangement*, is a specification of which state of the particle lies in which cell of the partition.

The crucial observation now is that a number of arrangements correspond to the same macro-state because the macro-properties of the system are determined solely by the number of particles in each cell, while it is irrelevant exactly which particle is in which cell. For instance, whether particle number 5 and particle number 7 are in cells  $\omega_1$  and  $\omega_2$  respectively, or vice versa, makes no difference to the macro-properties of the system as a whole because these do not depend on which particle is in which cell. Hence, all one needs in order to determine the macro-properties of the system is a specification of how many particles there are in each cell of the coarse-graining of  $\chi_a$ . Such a specification is called a *distribution* and it can be written as a tuple  $D = (n_1, \dots, n_l)$ , meaning that there are  $n_1$  particles in cell  $\omega_1$ , etc. The  $n_j$  are referred to as *occupation numbers*. This allows us to ask a crucial question: how many arrangements are compatible with a given distribution  $D$ ? Some elementary combinatorial considerations show that

$$G(D) := \frac{n!}{n_1! \dots n_l!} \quad (27.1)$$

arrangements are compatible with a given distribution  $D$  (where ‘!’ denotes factorials, i.e.  $k! := k(k-1) \dots 1$ , for any natural number  $k$  and  $0! := 1$ ). By construction  $G(D)$  is a measure for how many micro-states are compatible with a macro-state: the larger the value of  $G$ , the more the arrangements are compatible with a given distribution  $D$ .

The number  $G(D)$  matters to our original question about the size of  $X_{M_{eq}}$ . Since  $X$  is a Cartesian product of  $6n$  copies of  $\chi$ , each point in  $X$  corresponds to exactly one distribution  $D$ . Let us denote the region in  $X$  that corresponds to distribution  $D$  as  $X_D$ . One can then show that the size of that region is given by

$$\mu(X_D) = G(D) (\delta\omega)^n. \quad (27.2)$$

Hence the size of the part of  $X$  that corresponds to  $D$  is directly proportional to  $G(D)$ : the larger  $G(D)$ , the larger  $X_D$ . So, by maximizing  $G(D)$  — that is, by finding the distribution that is compatible with the largest number of (coarse-grained) micro-states — one at the same also finds the largest macro-region. Let  $D_{max}$  be that distribution. If one then adopts the above principle that equilibrium is the macro-state that is compatible with the largest number of micro-states, then, by definition,  $D_{max}$  is the equilibrium distribution and  $X_{D_{max}}$  is the equilibrium macro-region:  $X_{D_{max}} = X_{M_{eq}}$ . Since  $D_{max}$  has the largest  $G$  of all distributions, it also corresponds to the largest macro-region, which provides the sought-after justification of the notion that equilibrium is the state with the largest macro-region.

It remains to find  $D_{max}$ . To solve this problem, Boltzmann makes two crucial sets of assumptions. The first concerns the energy of the particles. Boltzmann assumes that the energy of a particle only depends on which cell  $\omega_j$  it is in, but *not* on the states of the other particles; that is, he neglects the contribution to the energy of the system that stems from interactions between the particles. He then also assumes that the energy of a particle only depends on which cell it is in but not on where it lies within the cell. Under these assumptions, the total energy of the system is given by  $\sum_{j=1}^l n_j E_j$ , where  $E_j$  is the energy of a particle in cell  $j$ . The second assumption is that there are many particles in each individual cell:  $n_j \gg 1$  for all  $j$ . Under these assumptions one can prove that  $G(D)$  reaches its maximum for

$$n_j = \alpha \exp(-\beta E_j), \quad (27.3)$$

which is the (discrete) *Maxwell-Boltzmann distribution*, where  $\alpha$  and  $\beta$  are constants depending on the nature of the system. So the largest macro-region in  $X$  corresponds to the Maxwell-Boltzmann and that is the equilibrium region.

This justificatory strategy faces both conceptual and technical challenges. The main conceptual problem is the absence of a connection with the thermodynamic notion of equilibrium. The following is a typical thermodynamics (TD) textbook definition of equilibrium: “A thermodynamic system

is in equilibrium when none of its thermodynamic properties are changing with time [...]” (Reiss, 1996, p. 3). The problem is that there is no *conceptual* connection between this notion of equilibrium and the idea that the equilibrium macro-state is the macro-state that is compatible with the largest number of micro-states. This is a problem for anyone wishing to maintain at least some congruence between SM and TD.

Even if one was willing to set aside conceptual issues, there remains the technical problem that the assumptions made for the optimization are so strong that the domain of application of the theory is in effect limited to dilute gases. The combinatorial argument assumes that the energy of a particle depends only on the cell in which it is located. This assumption applies, strictly speaking, *only to systems with non-interacting particles, i.e., ideal gases* (Uffink, 2007; Frigg, 2008). Ideal gases are, perhaps, a good approximation for *dilute gases*, i.e., gases of low density, and so the argument may deliver the approximately correct results for such systems. However, the argument remains silent about systems with stronger inter-particle forces such as liquids and solids. This is a serious limitation, and no suggestions have been made so far as to how it could be overcome.

One might try to avoid at least the technical difficulties by reading the argument backward, as it were: postulate that the Maxwell-Boltzmann distribution is the equilibrium distribution (rather than deriving it from an optimization procedure), and then appeal to combinatorial considerations to establish that the corresponding macro-region is large. This move is motivated by the fact that Maxwell’s (1860) original derivation of the Maxwell-Boltzmann distribution does not appeal to optimization procedures. Unfortunately, this is a blind alley. The Maxwell-Boltzmann distribution is in fact the equilibrium distribution only for a limited class of systems, namely for systems consisting of particles with negligible inter-particle forces. In general, systems with non-negligible interactions will have equilibrium distributions that are different from the Maxwell-Boltzmann distribution (Gupta, 2003). A closer look at Maxwell’s derivation shows why: non-interaction enters via the postulate that the probability distributions in different spatial directions can be factorized, which is true only if there is no interaction between particles (see Uffink, 2007).

A different justification of the postulate that equilibrium corresponds to the largest macro-region appeals to the time-evolution of a system and argues that if a part of the state space is overwhelmingly large, then a state sooner or later has to move into that part and stay there for a long time. This, so the argument continues, is the defining feature of equilibrium, and therefore the largest macro-state is the equilibrium state. This reply is closely tied to the typicality approach that we discuss in the next section, where we will see that the basic assumptions of that approach are questionable.

As we have seen in the last section, the Boltzmann entropy is proportional to the logarithm of the measure of a macro-region and therefore the macro-state with the largest macro-region also has the highest Boltzmann entropy. We know from TD that, if left to itself, a system approaches equilibrium, and equilibrium is the maximum entropy state. Therefore the macro-state with the largest macro-region is the equilibrium state.<sup>5</sup>

This line of argument faces a number of difficulties. The first is that it attributes an entropy to systems out of equilibrium and compares entropy levels at different stages of a process. But thermodynamics does not attribute an entropy to systems out of equilibrium at all and therefore comparing the entropies of different macro-states (most of which will be non-equilibrium states) makes little sense from a thermodynamic point of view. But even if this problem could be resolved (through a suitable generalization of thermodynamics, for instance) there would remain a question why the fact that the thermodynamic entropy reaches a maximum in equilibrium would imply that this also holds for the Boltzmann entropy. To justify this inference, the assumption would need to be made that the thermodynamic entropy reduces to the Boltzmann entropy. However, it is far from clear whether that is the case. A connection between the TD entropy and the Boltzmann entropy has been established only for ideal gases where the so-called Sackur-Tatrod formula can be derived from BSM, which shows that both entropies have the same functional dependence on thermodynamic state variables. Yet for

systems with interactions no such results are known (cf. Frigg and Werndl, 2011b). Furthermore, there are well-known differences between the TD and the Boltzmann entropy. For example, the TD entropy is extensive but the Boltzmann entropy is not (Ainsworth, 2012), and an extensive concept cannot reduce to a non-extensive concept (at least not without further qualifications).

A different route is taken in the long-run-fraction-of-time-account recently proposed by Werndl and Frigg (2015a, 2015b). This approach *defines* equilibrium in terms of how long a system spends in a certain state (rather than in terms of the size of its macro-region) and then proves a theorem establishing that the state is large in the requisite sense. Let  $LF_M(x)$  be the long-run fraction of time that a system starting in micro-state  $x$  spends in macro-region  $X_M$ . If, for instance,  $LF_M(x) = 0.43$ , then the system starting in initial condition  $x$  spends 43% of the time in macro-state  $M$  in the long run. Equilibrium, then, is the macro-state in which the system spend most of its time for nearly all initial conditions. Formally, if a macro-state  $M$  satisfies the following condition, then it is the system's equilibrium macro-state:  $LF_M(x) \geq \alpha$  for a real number  $\alpha \in (0.5, 1]$  and for all  $x \in Y$  where  $Y$  is a subset of  $X$  such that  $\mu(Y) \geq 1 - \varepsilon$  for a very small real number  $\varepsilon \geq 0$ . An obvious question concerns the value of  $\alpha$ . Often the assumption seems to be that  $\alpha$  is close to one. This is a reasonable but not the only possible choice, and nothing hangs on the value of  $\alpha$ . The introduction of  $\varepsilon$  accounts for the possibility that we should not expect *every* initial condition to approach equilibrium (Callender, 2001).

This definition is couched entirely in terms of time and remains completely silent about the size of  $X_M$ . This raises the question: is  $X_M$  large in the way BSM would have it? The answer to this question is given by the Dominance Theorem: If  $M$  is the equilibrium macro-state, then  $\mu(X_M) \geq \alpha(1 - \varepsilon)$  (Werndl and Frigg, 2015b). The theorem is completely general in that no dynamical assumptions are made (in particular it is not assumed that the system is ergodic) and hence the theorem also applies to strongly interacting systems such as solids and liquids. It is important to note that the theorem makes the conditional claim that *if* there is an equilibrium, *then*  $X_M$  is large. As with all conditionals, the crucial and often vexing question is whether, and under what conditions, the antecedent holds.

### **27.3 The Approach to Equilibrium**

Why do isolated systems, when left to themselves, approach equilibrium? To appreciate the thrust of this question we have to recall an important property of the phenomenology of the approach to equilibrium: irreversibility. We see gases spread, yet we never observe the reverse process of a uniformly distributed gas suddenly concentrating in the left half of the container. So it seems that the question ought to be: why do systems approach equilibrium *irreversibly*? The standard line is that this irreversible approach to equilibrium is a consequence of the second law of thermodynamics, which, roughly, states that entropy cannot decrease in isolated systems. However, as Brown and Uffink (2001) rightly point out, that systems prepared in a non-equilibrium state do approach equilibrium is not a consequence of the second law (or any other law of standard thermodynamics) and has to be added as an independent principle, which they call the “minus first law.” This suggests that the task for non-equilibrium SM is to derive the minus first law of thermodynamics from BSM.

This is setting the bar too high. SM systems show Poincaré recurrence. That is, the system's micro-state will eventually return arbitrarily close to the original initial condition. But a system with Poincaré recurrence cannot possibly exhibit strict irreversible behavior (and that it may take a very long time until a system actually returns close to its initial condition does not make this point moot). Contributors to the discussion have acknowledged this fact (more or less explicitly) and there is an (at least tacit) agreement that what should be derived from SM is an *approximate* version of the minus first law. Different version of BSM differ in how they explicate the approximation.

Boltzmann (1909 [1877]) conceptualized the problem as one of showing that the system is overwhelmingly likely to be in equilibrium whenever one observes the system. To this end Boltzmann defined macro-states in terms of distributions (he assumed that to every distribution  $D$  there corresponds a macro-state) and introduced the postulate that the probability of a macro-state is proportional to  $G(D)$ . Since, as we have seen above,  $G(D)$  is largest for the equilibrium state, it follows immediately that equilibrium is the most likely state and he argued that “the system of bodies always evolves from a more unlikely to a more likely state” (Boltzmann, 1909 [1877]: 166; our translation). This view faces a number of problems (Frigg, 2010a). There is the question of how to justify the postulate that the probability of a macro-state is proportional to  $G(D)$ , and, more seriously, it is unclear where the tendency to move toward more probable states comes from. The probabilities of macro-states are unconditional probabilities, and as such they do not imply anything about the succession of macro-states, let alone that ones of low probability are followed by ones of higher probability.

A related approach (also originating in the work of Boltzmann) appeals to the notion of ergodicity. Intuitively, a system is ergodic if the fraction of time an arbitrary solution stays in a subset  $A$  of  $X$  equals the measure of  $A$  in the long run. More formally, let  $LF_A(x)$  be the long-run fraction of time a system starting in initial condition  $x$  will spend in a set  $A \subset X$  (that is, for time  $t \rightarrow \infty$ ). A system is ergodic iff  $LF_A(x) = \mu(A)$  for all subsets  $A \subset X$  and for almost all initial conditions  $x$  (that is, except perhaps for some initial conditions that taken together form a set of measure zero). If, for instance, a certain set has measure  $1/3$  and if the system is ergodic, then we know that in the long run it will spend  $1/3$  of the time in that set.

Since ergodicity concerns any set  $A \subset X$ , it a fortiori also concerns macro-regions, and hence an ergodic system will spend a fraction of time in every macro-state that is proportional to the size of the corresponding macro-region. Since equilibrium is the state with the largest macro-region, an ergodic system will spend most of the time in equilibrium for almost all initial conditions (which implies, of course, that if the system is set in motion in a non-equilibrium micro-condition, it will soon enough move into the equilibrium macro-region). This is the justification of the minus first law of thermodynamics. It is an approximate justification in two ways. First, systems are said to spend most of the time in equilibrium but they are not said to never move out of equilibrium. In fact they can (and they will), and so ergodicity does not imply strict irreversibility. Second, ergodicity does not rule out that there are “bad” initial conditions, i.e., initial conditions that lie on trajectories that do not satisfy  $LF_A(x) = \mu(A)$ ; ergodicity just requires that there are ‘few’ bad initial conditions in the sense that all bad conditions taken together form a set of measure zero.

The ergodic program faces two main challenges. The first, known as the *measure zero problem*, points out that the set of allowed “bad” states can actually rather large since measure zero sets can be rather big. As an example consider the set of the rational numbers. This set has measure zero in the real numbers, but there are great many rational numbers. This fact can be made visible by using alternative ways to assess the size of sets, such as Baire categories (Sklar, 1993, pp. 182–188). The second objection, the *irrelevancy challenge*, is that ergodicity is irrelevant because many real systems are not ergodic (Earman and Rédei, 1996). The force of this argument can be mitigated by introducing the notion of epsilon-ergodicity (Vranas, 1998). Intuitively, a dynamical system is epsilon-ergodic if it is ergodic on the vast majority of  $X$ , namely, on a set of measure  $\geq 1 - \varepsilon$ , where  $\varepsilon$  is a very small real number or zero. The class of systems that are epsilon-ergodic is larger than the class of systems that is ergodic and it is plausible that it comprises many realistic systems such as gases and some liquids (Frigg and Werndl, 2011a). However, there will be SM systems that are not epsilon-ergodic and it remains unclear how the ergodic program deals with them.

An alternative approach is developed by Albert (2000), who proposes to explain irreversible behavior in terms of transition probabilities: rather than assigning probabilities to macro-states, we should look at how likely a system is to transition into a right macro-state given that it is in a certain

macro-state now. To this end Albert introduces the *statistical postulate*: given that the system is in macro-state  $M$  at time  $t$ , the probability of finding the micro-state of the system in a set  $C \subset X_M$  at time  $t$  is  $\mu(C)/\mu(X_M)$ . One can separate the states in  $X_M$  into “good” and “bad” ones, with good ones being those lying on trajectories that move into macro-states of higher entropy (than  $M$ ) once they leave  $X_M$  and bad ones being those lying on trajectories that move into lower entropy (than  $M$ ) states. Thermodynamic behavior is then justified if one can show that for all macro-states (other than the equilibrium macro-state) it is the case that the system is overwhelmingly more likely to move toward a higher (Boltzmann) entropy macro-state than to a lower (Boltzmann) entropy macro-state. However, the statistical postulate also allows to calculate the probability that a system has moved into the current macro-state from a higher or a lower entropy macro-state. It turns out to be the case that whenever the system is overwhelmingly likely to evolve toward a macro-state of higher entropy in the future, it is also overwhelmingly likely to have evolved into the current macro-state from a past macro-state which also has higher entropy. So the formalism returns wrong transition probabilities.

Albert discusses this problem at length and suggests fixing it by first taking the system under investigation to be the entire universe and then adopting the so-called *Past Hypothesis*, the postulate that “the world came into being in whatever particular low-entropy highly condensed big-bang sort of macro-condition it is that the normal inferential procedures of cosmology will eventually present to us” (Albert, 2000, p. 96). The problem with the wrong transition probabilities is then solved by conditioning on the past state: given that the system is in macro-state  $M$  at time  $t$ , the probability of finding the system’s micro-state in set  $C \subset X_M$  at time  $t$  is  $\mu(C \cap T_t(X_{M_p}))/\mu(X_M \cap T_t(X_{M_p}))$ . Albert argues at length that if this rule is used to calculate probabilities, then a high-entropy future as well as a low-entropy past are overwhelmingly likely.

There are a number of concerns about this explanation. A crucial aspect of Albert’s explanation is to apply SM to the universe as a whole. Earman argued that this project is doomed to failure because in the setting of standard cosmological models the past hypothesis is “not even false” (Earman, 2006, p. 400).<sup>6</sup> The Boltzmann entropy is a global quantity characterizing the macro-state of an entire system, in Albert’s case the entire universe.

As Winsberg (2004) points out, just because the overall entropy of the universe increases, it need not be the case that the entropy in a small subsystem also increases and hence Albert’s approach cannot explain the behavior of small systems such as gases in laboratories. Albert’s argument relies on assigning probabilities to sets of microstates based on a certain algorithm, and there are questions about the justification of that algorithm (see Davey, 2008; Frigg, 2010a). Furthermore, Albert’s justification that a high-entropy future and low-entropy past are overwhelmingly likely appeals to a dynamical assumption, which he calls the “scattering condition.” And there is a question whether this condition holds true in SM systems.<sup>7</sup>

An alternative account explains the approach to equilibrium in terms of *typicality*. Consider an element  $e$  of a set  $\Sigma$ . Typicality is a relational property of  $e$ , which  $e$  possesses with respect to  $\Sigma$ , a property  $P$  and a measure  $\nu$ , often referred to as the “typicality measure.” Intuitively,  $e$  is typical if most members of  $\Sigma$  have property  $P$  and  $e$  is one of them. More precisely, let  $\Pi \subset \Sigma$  be the set of all elements that have property  $P$ . Then the element  $e$  is typical if  $e \in \Pi$  and  $\nu(\Pi)/\nu(\Sigma) \geq 1 - \varepsilon$ , where  $\varepsilon$  is a finite but very small positive real number. The element of interest in SM is a micro-state  $x$  and the typicality measure is the Lebesgue measure  $\mu$ . The typicality account comes in different versions that disagree about the choice of the set  $\Sigma$  and the selection of property  $P$  (for a discussion of these different accounts see Frigg (2009) and (2010b)).

The first account explains the approach to equilibrium in terms of equilibrium micro-states being typical. As we have seen above, the equilibrium macro-region is by far the largest macro-region. This implies that equilibrium micro-states are typical with respect to  $X$  because “reaching the equilibrium distribution in the course of the temporal evolution of a system is inevitable due to the fact that the overwhelming majority of microstates in the phase space have this distribution”

(Zanghi, 2005, p. 196; our translation). However, as Uffink (2007, pp. 979–980) points out, in general there is no reason to assume that points in an atypical set have to evolve into a typical set; typical states are not attractors of atypical states. The second account combines a typicality claim about equilibrium micro-states with a further typicality claim about the dynamics of the system. Goldstein champions such an account when he submits that for “a non-equilibrium phase point  $[x]$  of energy  $E$ , the Hamiltonian dynamics governing the motion  $[x(t)]$  would have to be ridiculously special to avoid reasonably quickly carrying  $[x(t)]$  into  $[X_{Meq}]$  and keeping it there for an extremely long time — unless, of course,  $[x]$  itself were ridiculously special” (Goldstein, 2001, pp. 43–44). Unfortunately, Goldstein offers no account of what it means for a dynamical law not to be “ridiculously special” and so the account remains underspecified (see Frigg and Werndl (2012) for a proposed completion of the account in terms of epsilon-ergodicity.) The third account, due to Lebowitz (1993a, 1993b), considers the internal structure of macro-regions (in much the same way as the transition probability approach we have seen above) and argues that micro-states lying on entropy-increasing trajectories are typical. This line of argument faces the same challenges as Albert’s and it remains an unproven assertion that the internal structure of the macro-regions is as the account asserts (see Frigg, 2010b for a discussion of this issue).

The problem of the approach to equilibrium takes a different form in the long-run-fraction-of-time-account (Werndl and Frigg, 2015a, 2015b). In that account it is part and parcel of the notion of an equilibrium state that the system approaches that state and stays there most of the time; if a state does not have that feature, then it is not an equilibrium state. The crucial question then is: under what conditions does an equilibrium exist? Werndl and Frigg (forthcoming) point out that for an equilibrium to exist three factors need to co operate: the choice of macro-variables, the dynamics of the system, and the choice of the effective state space. They then prove a theorem providing general necessary and sufficient conditions for the existence of an equilibrium state. Intuitively, the theorem says that there is an equilibrium just in case the effective state space of the system is split up into invariant regions on which the motion is ergodic and the equilibrium macro-state takes up most of each region. This suggests a change in the way in which we think about equilibrium: rather than launching a search for one crucial factor (such as ergodicity or typicality), the focus should be on finding triplets of macro-variables, dynamical conditions, and effective state spaces that satisfy the conditions of the theorem. An example of such triplet is the dynamics of the Kac ring on the full state space with a homogeneity macro-variable (Werndl and Frigg, 2015b). With this theorem, the theoretical question when and under what conditions an equilibrium exists is solved. But the identification and classification of these triplets for concrete problems remains an open question.

## 27.4 Conclusion

We have reviewed a number of Boltzmannian definitions of equilibrium along with explanations of the approach to equilibrium. We want to conclude by drawing attention to two further issues. The first is the relation of BSM to Gibbsian SM. There are two competing theoretical approaches in SM, one associated with Boltzmann and the other with Gibbs, which offer different conceptualizations of equilibrium. This would not be a problem if the two formalisms were equivalent (for instance, in a similar way in which the Schrödinger and the Heisenberg picture in quantum mechanics are equivalent). Unfortunately they are not, and there is no obvious way to translate results from one framework into the other. But having two incompatible notions of equilibrium at work in SM is unsatisfactory and a sustained reflection on how the Boltzmannian and the Gibbsian approach relate to each other is necessary. Steps toward a better understanding of the relationship between these two approaches are made in Lavis (2005), Werndl and Frigg (2017b, 2020), and Frigg and Werndl (2019), but the problem is one that deserves more attention than it has received so far.

The second issue is the interpretation of probability. As we have seen, many explanations of the approach to equilibrium rely in one way or another on probabilities, and there is a question of how these should be interpreted. A number of suggestions have been made including Humean Best Systems (Loewer, 2001; Frigg and Hoefer, 2015), typicality measures (Werndl, 2013), frequencies (discussed but not endorsed in van Lith, 2001) and propensities (discussed but not endorsed in Clark, 2001). Another approach attempts to ground the probabilities of SM in quantum mechanics, and so they are interpreted in whatever way quantum probabilities are interpreted. See Frigg and Werndl (this volume b) for a discussion of these approaches. There is no consensus on this issue, and finding a coherent interpretation of SM probabilities remains a challenge.

## Notes

- 1 For a discussion of this approach see Ardourel (2017), Brown et al. (2009), Uffink and Valente (2015) and Valente (2014).
- 2 For a simple and intuitive introduction to dynamical systems see Berkovitz et al. (2016). Detailed discussions of the framework of BSM can be found in Frigg (2008) and Uffink (2007).
- 3 See, for instance, Albert (2000, pp. 56–57), Bricmont (1995, p. 146); Goldstein (2001, pp. 43, 45); Goldstein and Lebowitz (2004, p. 57), Penrose (1989, p. 403) and Zanghì (2005, pp. 191, 196).
- 4 Classical presentations of the argument can be found in Ehrenfest and Ehrenfest (2002 [1912]) and Tolman (1979 [1938]: Ch. 4). For discussions see Frigg (2008) and Uffink (2007).
- 5 This strategy has been mentioned to us in conversation but is hard to track down in print. Albert's (2000) considerations concerning entropy seem to gesture in the direction of this third strategy.
- 6 For a further discussion of the past hypothesis see Wallace (2017), and for a discussion of its explanatory relevance see Callender (2004) and Price (2004).
- 7 For further discussions see Shahvisi (this volume) and Frigg (2008, Sec. 3.2.5).

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### **Further Reading from the Editors**

Many philosophers of physics get their first introduction to philosophy of thermal physics through D. Albert, *Time and Chance* (Cambridge, MA: Harvard University Press, 2000). For a rounded introduction, read this alongside L. Sklar, *Physics and Chance: Philosophical Issues in the Foundations of Statistical Mechanics* (Cambridge: Cambridge University Press, 1993). A scholarly treatment of Boltzmannian statistical mechanics is given by J. Uffink, “Boltzmann’s Work in Statistical Physics”, *Stanford Encyclopedia of Philosophy* (2014), and a vigorous defence of the Boltzmannian approach by S. Goldstein, “Boltzmann’s Approach to Statistical Mechanics”, in J. Bricmont, G. Ghirardi, D. Dürr, F. Petruccione, M.C. Galavotti, N. Zanghì (eds.) *Chance in Physics* (Heidelberg: Springer, 2001). A more conciliatory approach is taken by D. A. Lavis, “Boltzmann and Gibbs: An attempted reconciliation”, *Studies in History and Philosophy of Science Part B: Studies in History and Philosophy of Modern Physics* 36(2), (2005): 245–273. R. Frigg, “A Field Guide to recent Work on the Foundations of Statistical Mechanics,” in D. Rickles (ed.), *The Ashgate Companion to Contemporary Philosophy of Physics* (London: Ashgate, 2008): 99–196 provides a broad overview of recent work.