

A Field Guide to Recent Work on the Foundations of Statistical Mechanics

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1 Introduction

1.1 Statistical Mechanics—A Trailer

Statistical mechanics (SM) is the study of the connection between micro-physics and macro-physics.¹ Thermodynamics (TD) correctly describes a large class of phenomena we observe in macroscopic systems. The aim of statistical mechanics is to account for this behaviour in terms of the dynamical laws governing the microscopic constituents of macroscopic systems and probabilistic assumptions.²

This project can be divided into two sub-projects, equilibrium SM and non-equilibrium SM. This distinction is best illustrated with an example. Consider a gas initially confined to the left half of a box (see Figure 1.1):

[Insert Figure 1.1 (appended at the end of the document).]

Figure 1.1 Initial state of a gas, wholly confined to the left compartment of a box separated by a barrier.

This gas is in equilibrium as all natural processes of change have come to an end and the observable state of the system is constant in time, meaning that all macroscopic parameters such as local temperature and local pressure assume constant values. Now we remove the barrier separating the two halves of the box. As a result, the gas is no longer in equilibrium and it quickly disperses (see Figures 1.2 and 1.3):

[Insert Figures 1.2 and 1.3 (appended at the end of the document).]

¹Throughout this chapter I use ‘micro’ and ‘macro’ as shorthands for ‘microscopic’ and ‘macroscopic’ respectively.

²There is a widespread agreement on the broad aim of SM; see for instance Ehrenfest & Ehrenfest (1912, 1), Khinchin (1949, 7), Dougherty (1993, 843), Sklar (1993, 3), Lebowitz (1999, 346), Goldstein (2001, 40), Ridderbos (2002, 66) and Uffink (2007, 923).

Figures 1.2 and 1.3: When the barrier is removed the gas is no longer in equilibrium and disperses.

This process of dispersion continues until the gas homogeneously fills the entire box, at which point the system will have reached a new equilibrium state (see Figure 1.4):

[Insert Figure 1.4 (appended at the end of the document).]

Figure 1.4: Gas occupies new equilibrium state.

From an SM point of view, equilibrium needs to be characterised in micro-physical terms. What conditions does the motion of the molecules have to satisfy to ensure that the macroscopic parameters remain constant as long as the system is not subjected to perturbations from the outside (such as the removal of barriers)? And how can the values of macroscopic parameters like pressure and temperature be calculated on the basis of such a micro-physical description? Equilibrium SM provides answers to these and related questions.

Non-equilibrium SM deals with systems out of equilibrium. How does a system approach equilibrium when left to itself in a non-equilibrium state and why does it do so to begin with? What is it about molecules and their motions that leads them to spread out and assume a new equilibrium state when the shutter is removed? And crucially, what accounts for the fact that the reverse process won't happen? The gas diffuses and spreads evenly over the entire box; but it won't, at some later point, spontaneously move back to where it started. And in this the gas is no exception. We see ice cubes melting, coffee getting cold when left alone, and milk mix with tea; but we never observe the opposite happening. Ice cubes don't suddenly emerge from lukewarm water, cold coffee doesn't spontaneously heat up, and white tea doesn't un-mix, leaving a spoonful of milk at the top of a cup otherwise filled with black tea. Change in the world is *unidirectional*: systems, when left alone, move towards equilibrium but not away from it. Let us introduce a term of art and refer to processes of this kind as 'irreversible'. The fact

that many processes in the world are irreversible is enshrined in the so-called Second Law of thermodynamics, which, roughly, states that transitions from equilibrium to non-equilibrium states cannot occur in isolated systems. What explains this regularity? It is the aim of non-equilibrium SM to give a precise characterisation of irreversibility and to provide a microphysical explanation of why processes in the world are in fact irreversible.³

The issue of irreversibility is particularly perplexing because (as we will see) the laws of micro physics have no asymmetry of this kind built into them. If a system can evolve from state A into state B , the inverse evolution, from state B to state A , is not ruled out by any law governing the microscopic constituents of matter. For instance, there is nothing in the laws governing the motion of molecules that prevents them from gathering again in the left half of the box after having uniformly filled the box for some time. But how is it possible that irreversible behaviour emerges in systems whose components are governed by laws which are not irreversible? One of the central problems of non-equilibrium SM is to reconcile the asymmetric behavior of irreversible thermodynamic processes with the underlying symmetric dynamics.

1.2 Aspirations and Limitations

This chapter presents a survey of recent work on the foundations of SM from a systematic perspective. To borrow a metaphor of Gilbert Ryle's, it tries to map out the logical geography of the field, place the different positions and contributions on this map, and indicate where the lines are blurred and blank spots occur. Classical positions, approaches, and questions are discussed only if they have a bearing on current foundational debates; the presentation of the material does not follow the actual course of the history of SM, nor does it aim at historical accuracy when stating arguments and positions.⁴

Such a project faces an immediate difficulty. Foundational debates in many other fields can take as their point of departure a generally accepted formalism and a clear understanding of what the theory is. Not so in SM. Unlike quantum mechanics and relativity theory, say, SM has not yet found a

³Different meanings are attached to the term 'irreversible' in different contexts, and even within thermodynamics itself (see Denbigh 1989a and Uffink 2001, Section 3). I am not concerned with these in what follows and always use the term in the sense introduced here.

⁴Those interested in the long and intricate history of SM are referred to Brush (1976), Sklar (1993, Ch. 2), von Plato (1994) and Uffink (2007).

generally accepted theoretical framework, let alone a canonical formulation. What we find in SM is a plethora of different approaches and schools, each with its own programme and mathematical apparatus, none of which has a legitimate claim to be more fundamental than its competitors.

For this reason a review of foundational work in SM cannot simply begin with a concise statement of the theory's formalism and its basic principles, and then move on to the different interpretational problems that arise. What, then, is the appropriate way to proceed? An encyclopaedic list of the different schools and their programme would do little to enhance our understanding of the workings of SM. Now it might seem that an answer to this question can be found in the observation that across the different approaches equilibrium theory is better understood than non-equilibrium theory, which might suggest that a review should begin with a presentation and discussion of equilibrium, and then move on to examining non-equilibrium.

Although not uncongenial, this approach has two serious drawbacks. First, it has the disadvantage that the discussion of specific positions (for instance the ergodic approach) will be spread out over different sections, and as a result it becomes difficult to assess these positions as consistent bodies of theory. Second, it creates the wrong and potentially misleading impression that equilibrium theory can (or even should be) thought of as an autonomous discipline. By disconnecting the treatment of equilibrium from a discussion of non-equilibrium we lose sight of the question of how and in what way the equilibrium state constitutes the final point towards which the dynamical evolution of a system converges.

In what follows I take my lead from the fact that all these different schools (or at any rate those that I discuss) use (slight variants) of either of two theoretical frameworks, one of which can be associated with Boltzmann (1877) and the other with Gibbs (1902), and can thereby classify different approaches as either 'Boltzmannian' or 'Gibbsian'. The reliance on a shared formalism (even if the understanding of the formalism varies radically) provides the necessary point of reference to compare these accounts and assess their respective merits and drawbacks. This is so because the problems that I mentioned in Subsection 1.1 can be given a precise formulation only within a particular mathematical framework. Moreover it turns out that these frameworks give rise to markedly different characterisations both of equilibrium and of non-equilibrium, and accordingly the problems that beset accounts formulated within either framework are peculiar to one framework and often do not have a counterpart in the other. And last but not least, the scope of

an approach essentially depends on the framework in which it is formulated, and, as we shall see, there are significant differences between two (I return to this issue in the conclusion).

Needless to say, omissions are unavoidable in an chapter-size review. I hope that any adversity caused by these omissions is somewhat alleviated by the fact that I clearly indicate at what point they occur and how the omitted positions or issues fit into the overall picture; I also provide ample references for those who wish to pursue the avenues I bypass.

The most notable omission concerns the macro theory at stake, thermodynamics. The precise formulation of the theory, and in particular the Second Law, raises important questions. These are beyond the scope of this review; Appendix B provides a brief statement of the theory and flags the most important problems that attach to it.

What is the relevant microphysical theory? A natural response would be to turn to quantum theory, which is generally regarded as the currently best description of micro entities. The actual debate has followed a different path. With some rare exceptions, foundational debates in SM have been, and still are, couched in terms of classical mechanics (which I briefly review in Appendix A). I adopt this point of view and confine the discussion to classical statistical mechanics. This, however, is not meant to suggest that the decision to discuss foundational issues in a classical rather than a quantum setting is unproblematic. On the contrary, many problems that occupy centre stage in the debate over the foundations of SM are intimately linked to aspects of classical mechanics and it seems legitimate to ask whether, and, if so, how, these problems surface in quantum statistical mechanics. (For a review of foundational issues in quantum SM see Emch (2007)).

2 The Boltzmann Approach

Over the years Boltzmann developed a multitude of different approaches to SM. However, contemporary Boltzmannians (references will be given below), take the account introduced in Boltzmann (1877) and streamlined by Ehrenfest & Ehrenfest (1912) as their starting point. For this reason I concentrate on this approach, and refer the reader to Klein (1973), Brush (1976), Sklar (1993), von Plato (1994), Cercignani (1998) and Uffink (2004, 2007) for a discussion of Boltzmann's other approaches and their tangled history, and to de Regt (1996), Blackmore (1999) and Visser (1999) for discussions of

Boltzmann’s philosophical and methodological presuppositions at different times.

2.1 The Framework

Consider a system consisting of n particles with three degrees of freedom,⁵ which is confined to a container of finite volume V and has total energy E .⁶ The system’s fine-grained micro-state is given by a point in its $6n$ dimensional phase space Γ_γ .⁷ In what follows we assume that the system’s dynamics is governed by Hamilton’s equations of motion,⁸ and that the system is isolated from its environment.⁹ Hence, the system’s fine-grained micro-state x lies within a finite sub-region $\Gamma_{\gamma,a}$ of Γ_γ , the so-called ‘accessible region’ of Γ_γ . This region is determined by the constraints that the motion of the particles is confined to volume V and that the system has constant energy E —in fact, the latter implies that $\Gamma_{\gamma,a}$ entirely lies within a $6n - 1$ dimensional hypersurface Γ_E , the so-called ‘energy hypersurface’, which is defined by the condition $H(x) = E$, where H is the Hamiltonian of the system and E the system’s total energy. The phase space is endowed with a Lebesgue measure μ_L , which induces a measure $\mu_{L,E}$ on the energy hypersurface via Equation (46) in the Appendix. Intuitively, these measures associate a ‘volume’ with subsets of Γ_γ and Γ_E ; to indicate that this ‘volume’ is not the the familiar volume in three dimensional physical space it is often referred to as ‘hypervolume’.

Hamilton’s equations define a measure preserving flow ϕ_t on Γ_γ , meaning that $\phi_t : \Gamma_\gamma \rightarrow \Gamma_\gamma$ is a one-to-one mapping and $\mu_L(R) = \mu_L(\phi_t(R))$ for

⁵The generalisation of what follows to systems consisting of objects with any finite number of degrees of freedom is straightforward.

⁶The version of the Boltzmann framework introduced in this subsection is the one favoured by Lebowitz (1993a, 1993b, 1999), Goldstein (2001), and Goldstein & Lebowitz (2004). As we shall see in the next subsection, some authors give different definitions of some of the central concepts, most notably the Boltzmann entropy.

⁷The choice of the somewhat gawky notation ‘ Γ_γ ’ will be justified in the next subsection.

⁸For brief review of classical mechanics see Appendix A. From a technical point of view the requirement that the system be Hamiltonian is restrictive because the Boltzmannian machinery, in particular the combinatorial argument introduced in the next subsection, can be used also in some cases of non-Hamiltonian systems (for instance the Baker’s gas and the Kac ring). However, as long as one believes that classical mechanics is the true theory of particle motion (which is what we do in classical SM), these other systems are not relevant from a foundational point of view.

⁹This assumption is not uncontroversial; in particular, it is rejected by those who advocate an interventionist approach to SM; for a discussion see Subsection 3.5.2.

all times t and all regions $R \subseteq \Gamma_\gamma$, from which it follows that $\mu_{L,E}(R_E) = \mu_{L,E}(\phi_t(R_E))$ for all regions $R_E \subseteq \Gamma_E$.

Let M_i , $i = 1, \dots, m$, be the system's macro-states. These are characterised by the values of macroscopic variables such as local pressure, local temperature, and volume.¹⁰ It is one of the basic posits of the Boltzmann approach that a system's macro-state supervenes on its fine-grained micro-state, meaning that a change in the macro-state must be accompanied by a change in the fine-grained micro-state (i.e. it is not possible, say, that the pressure of a system changes while its fine-grained micro-state remains the same). Hence, to every given fine-grained micro-state $x \in \Gamma_E$ there corresponds *exactly one* macro-state. Let us refer to this macro-state as $M(x)$.¹¹ This determination relation is not one-to-one; in fact many different $x \in \Gamma_E$ can correspond to the same macro-state (this will be illustrated in detail in the next subsection). It is therefore natural to define

$$\Gamma_{M_i} := \{x \in \Gamma_E \mid M_i = M(x)\}, i = 1, \dots, m, \quad (1)$$

the subset of Γ_E consisting of all fine-grained micro-states that correspond to macro-state M_i . The proposition that a system with energy E is in macro-state M_i and the proposition that the system's fine-grained micro-state lies within Γ_{M_i} always have the same truth value; for this reason, M_i and Γ_{M_i} alike are sometimes referred to as 'macro-states'. However, at some points in what follows it is important to keep the two separate and so I do not follow this convention; I reserve the term 'macro-state' for the M_i 's and refer to the Γ_{M_i} 's as 'macro-regions'.

The Γ_{M_i} don't overlap because macro-states supervene on micro-states: $\Gamma_{M_i} \cap \Gamma_{M_j} = \emptyset$ for all $i \neq j$ and $i, j = 1, \dots, m$. For a complete set of macro-states the Γ_{M_i} also jointly cover the accessible region of the energy hypersurface: $\Gamma_{M_1} \cup \dots \cup \Gamma_{M_m} = \Gamma_{\gamma,a}$ (where ' \cup ', ' \cap ' and ' \emptyset ' denote set theoretic union, intersection and the empty set respectively). In this case the Γ_{M_i} form a partition of $\Gamma_{\gamma,a}$.¹²

¹⁰Whether index i ranges over a set of finite, countably infinite, or uncountably infinite cardinality depends both on the system and on how macro-states are defined. In what follows I assume, for the sake of simplicity, that there is a finite number m of macro-states.

¹¹This is not to claim that *all* macroscopic properties of a gas supervene on its mechanical configuration; some (e.g. colour and smell) do not. Rather, it is an exclusion principle: if a property does not supervene on the system's mechanical configuration then it does not fall within the scope of SM.

¹²Formally, $\{\alpha_1, \dots, \alpha_k\}$, where $\alpha_i \subseteq A$ for all i , is a partition of A iff $\alpha_1 \cup \dots \cup \alpha_k = A$

The Boltzmann entropy of a macro-state M_i is defined as¹³

$$S_B(M_i) = k_B \log[\mu_{L,E}(\Gamma_{M_i})], \quad (2)$$

where k_B is the so-called Boltzmann constant. For later discussions, in particular for what Boltzmannians have to say about non-equilibrium and reductionism, a small ‘cosmetic’ amendment is needed. The Boltzmann entropy as introduced in Equation (2) is a property of a macro-state. Since a system is in exactly one macro-state at a time, the Boltzmann entropy can equally be regarded as a property of a system itself. Let $M(x(t))$ be the system’s macro-state at time t (i.e. $M(x(t))$ is the M_i in which the system’s state x happens to be at time t), then the *system’s* Boltzmann entropy at time t is defined as

$$S_B(t) := S_B[M(x(t))]. \quad (3)$$

By definition, the equilibrium state is the macro-state for which the Boltzmann entropy is maximal. Let us denote that state by M_{eq} (and, without loss of generality, choose the labelling of macro-states such that $M_{eq} = M_m$). Justifying this definition is one of the main challenges for the Boltzmannian approach, and I return to this issue below in Subsection 2.7.

We now need to explain the approach to equilibrium. As phrased in the introduction, this would amount to providing a mechanical explanation of the Second Law of thermodynamics. It is generally accepted that this would be aiming too high; the best we can hope for within SM is to get a justification of a ‘probabilistic version’ of the Second Law, which I call ‘Boltzmann’s Law’ (BL) (Callender 1999; Earman 2006, 401-403):

Boltzmann’s Law: Consider an arbitrary instant of time $t = t_1$ and assume that the Boltzmann entropy of the system at that time, $S_B(t_1)$, is far below its maximum value. It is then highly probable that at any later time $t_2 > t_1$ we have $S_B(t_2) \geq S_B(t_1)$.¹⁴

and $\alpha_i \cap \alpha_j = \emptyset$ for all $i \neq j$ and $i, j = 1, \dots, k$.

¹³Goldstein (2001, 43), Goldstein and Lebowitz (2004, 57), Lebowitz (1993a, 34; 1993b, 5; 1999, 348).

¹⁴BL is sometimes referred to as the ‘statistical H-Theorem’ or the ‘statistical interpretation of the H -theorem’ because in earlier approaches to SM Boltzmann introduced a quantity H , which is basically equal to $-S_B$, and aimed to prove that under suitable assumptions it decreased monotonically. For discussions of this approach see the references cited at the beginning of this subsection.

Unlike the Second Law, which is a universal law (in that it does not allow for exceptions), BL only makes claims about what is very likely to happen. Whether it is legitimate to replace the Second Law by BL will be discussed in Subsection 2.8. Even if this question is answered in the affirmative, what we expect from SM is an argument for the conclusion that BL, which so far is just a conjecture, holds true in the relevant systems, and, if it does, an explanation of why this is so. In order to address *this* question, we need to introduce probabilities into the theory to elucidate the locution ‘highly probable’.

There are two different ways of introducing probabilities into the Boltzmannian framework. The first assigns probabilities directly to the system’s macro-states; the second assigns probabilities to the system’s micro-state being in particular subsets of the macro-region corresponding to the system’s current macro-state.¹⁵ For want of better terms I refer to these as ‘macro-probabilities’ and ‘micro-probabilities’ respectively. Although implicit in the literature, the distinction between macro-probabilities and micro-probabilities has never been articulated, and it rarely, if ever, receives any attention. This distinction plays a central rôle in the discussion both of BL and the interpretation of SM probabilities, and it is therefore important to give precise definitions.

Macro-Probabilities: A way of introducing probabilities into the theory, invented by Boltzmann (1877) and advocated since then by (among others) those working within the ergodic programme (see Subsection 2.4), is to assign probabilities to the macro-states M_i of the system. This is done by introducing the postulate that the probability of a macro-state M_i is proportional to the measure of its corresponding macro-region:

$$p(M_i) := c \mu_{L,E}(\Gamma_{M_i}), \quad (4)$$

where c is a normalisation constant. I refer to this as the ‘Proportionality Postulate’ (PP). From this postulate and Equation (2) it follows immediately that the most likely macro-state is the macro-state with the highest Boltzmann entropy and the one that occupies the largest part of the (accessible) phase space.

From this point of view it seems natural to understand the approach to equilibrium as the evolution from an unlikely macro-state to a more likely

¹⁵This is not to say that these two kinds of probabilities are incompatible; in fact they could be used in conjunction. However, this is not what happens in the literature.

macro-state and finally to the most likely macro-state. If the system evolves from less to more likely macro-states *most of the time* then we have justified BL. Whether we have any reasons to believe that this is indeed the case will be discussed in Unit 2.3.2.

Micro-Probabilities: A different approach assigns probabilities to sets of micro-states (rather than to macro-states) on the basis of the so-called statistical postulate (SP).¹⁶

Statistical Postulate: Let M be the macro-state of a system at time t . Then the probability at t that the fine-grained micro-state of the system lies in a subset A of Γ_M is

$$\mu_{L,E}(A)/\mu_{L,E}(\Gamma_M). \quad (5)$$

With this assumption the truth of BL depends on the dynamics of the system, because now BL states that the overwhelming majority of fine-grained micro-states in any Γ_{M_i} (except the equilibrium macro-region) are such that they evolve under the dynamics of the system towards some other region Γ_{M_j} of higher entropy. Hence, the truth of BL depends on the features of the dynamics. The question is whether the systems we are interested in have this property. I come back to this issue in Subsection 2.6.

2.2 The Combinatorial Argument

An important element in most presentations of the Boltzmann approach is what is now known as the ‘combinatorial argument’. However, depending on how one understands the approach, this argument is put to different uses—a fact that is unfortunately hardly ever made explicit in the literature on the topic. I will first present the argument and then explain what these different uses are.

Consider the same system of n identical particles as above, but now focus on the 6 dimensional phase space of *one* of these particles, the so-called μ -space Γ_μ , rather than the ($6n$ dimensional) phase space Γ_γ of the entire

¹⁶It is not clear where this postulate originates. It has recently—with some qualifications, as we shall see—been advocated by Albert (2000), and also Bricmont (1996) uses arguments based on probabilities introduced in this way; see also Earman (2006, 405), where this postulate is discussed, but not endorsed. Principles very similar to this one have also been suggested by various writers within the Gibbsian tradition; see Section 3.

system.¹⁷ A point in Γ_μ denotes the particle's fine-grained micro-state. It necessarily lies within a finite sub-region $\Gamma_{\mu,a}$ of Γ_μ , the accessible region of Γ_μ . This region is determined by the constraints that the motion of the particles is confined to volume V and that the system as a whole has constant energy E . Now we choose a partition ω of $\Gamma_{\mu,a}$; that is, we divide $\Gamma_{\mu,a}$ into a finite number l of disjoint cells ω_j , which jointly cover the accessible region of the phase space. The introduction of a partition on a phase space is also referred to as 'coarse-graining'. The cells are taken to be rectangular with respect to the position and momentum coordinates and of equal volume $\delta\omega$ (this is illustrated in Figure 2).

[Insert Figure 2 (appended at the end of the document).]

Figure 2: Partitioning (or coarse-graining) of the phase space.

The so-called coarse-grained micro-state of a particle is given by specifying in which cell ω_j its fine-grained micro-state lies.¹⁸

The micro-state of the entire system is a specification of the micro-state of every particle in the system, and hence the fine-grained micro-state of the system is determined by n labelled points in Γ_μ .¹⁹ The so-called coarse-grained micro-state is a specification of which particle's state lies in which cell of the partition ω of $\Gamma_{\mu,a}$; for this reason the coarse-grained micro-state

¹⁷The use of the symbol μ both in 'μ-space' and to refer to the measure on the phase space is somewhat unfortunate as they have nothing to do with each other. However, as this terminology is widely used I stick to it. The distinction between μ -space and γ -space goes back to Ehrenfest & Ehrenfest (1912); it is a pragmatic and not a mathematical distinction in that it indicates how we *use* these spaces (namely to describe a single particle's or an entire system's state). From a mathematical point of view both μ -space and γ -space are classical phase spaces (usually denoted by Γ). This explains choice of the the seemingly unwieldy symbols Γ_μ and Γ_γ .

¹⁸There is a question about what cell a fine-grained micro-state belongs to if it lies exactly on the boundary between two cells. One could resolve this problem by adopting suitable conventions. However, it turns out later on that sets of measure zero (such as boundaries) can be disregarded and so there is no need to settle this issue.

¹⁹The points are labelled in the sense that it is specified which point represents the state of which particle.

of a system is also referred to as an ‘arrangement’.

The crucial observation now is that a number of arrangements correspond to the same macro-state because a system’s macro-properties 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 ω_1 and ω_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 we need in order to determine a system’s macro-properties is a specification of how many particles there are in each cell of the coarse-grained μ -space. Such a specification is called a ‘distribution’. Symbolically we can write it as a tuple $D = (n_1, \dots, n_l)$, meaning the distribution comprising n_1 particles in cell ω_1 , etc. The n_j are referred to as ‘occupation numbers’ and they satisfy the condition $\sum_{j=1}^l n_j = n$.

For what follows it is convenient to label the different distributions with a discrete index i (which is not a problem since for any given partition ω and particle number n there are only a finite number of distributions) and denote the i^{th} tuple by D_i . The beginning of such labelling could be, for instance, $D_1 = (n, 0, \dots, 0)$, $D_2 = (n - 1, 1, 0, \dots, 0)$, $D_3 = (n - 2, 1, 1, 0, \dots, 0)$, etc.

How many arrangements are compatible with a given distribution D ? Some elementary combinatorial considerations show that

$$G(D) := \frac{n!}{n_1! \dots n_l!} \quad (6)$$

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 $0! := 1$). For this reason a distribution conveys much less information than an arrangement.

Each distribution corresponds to well-defined region of Γ_γ , which can be seen as follows. A partition of Γ_μ is introduced in exactly the same way as above. In fact, the choice of a partition of Γ_μ induces a partition of Γ_γ because Γ_γ is just the Cartesian product of n copies of Γ_μ . The coarse-grained state of the system is then given by specifying in which cell of the partition its fine-grained state lies. This is illustrated in Figure 3 for the fictitious case of a two particle system, where each particle’s μ -space is one dimensional and endowed with a partition consisting of four cells $\omega_1, \dots, \omega_4$. (This case is fictitious because in classical mechanics there is no Γ_μ with less than two

dimensions. I consider this example for ease of illustration; the main idea carries over to higher dimensional spaces without difficulties.)

[Insert Figure 3 (appended at the end of the document).]

Figure 3: Specification of coarse-grained state of a system.

This illustration shows that each distribution D corresponds to a particular part of $\Gamma_{\gamma,a}$; and it also shows the important fact that parts corresponding to different distributions do not overlap. In Figure 3, the hatched areas (which differ by which particle is in which cell) correspond to the distribution $(1,0,0,1)$ and the dotted area (where both particles are in the same cell) correspond to $(0,2,0,0)$. Furthermore, we see that the hatched area is twice as large as the dotted area, which illustrates an important fact about distributions to which we now turn.

From the above it becomes clear that each point x in $\Gamma_{\gamma,a}$ corresponds to exactly one distribution; call this distribution $D(x)$. The converse of this, of course, fails since in general many points in $\Gamma_{\gamma,a}$ correspond to the same distribution D_i . These states together form the set Γ_{D_i} :

$$\Gamma_{D_i} := \{x \in \Gamma_{\gamma} \mid D(x) = D_i\}. \quad (7)$$

From equations (6) and (7), together with the assumption that all cells have the same size $\delta\omega$ (in the 6 dimensional μ -space), it follows that

$$\mu_L(\Gamma_{D_i}) = G(D_i) (\delta\omega)^n, \quad (8)$$

Next we want to know the distribution D for which $G(D)$, and with it $\mu_L(\Gamma_{D_i})$, assume their maximum. To solve this problem we make two crucial sets of assumptions, one concerning the energy of the system and one concerning the system's size (their implications will be discussed in Subsection 2.7).

First, we assume that the energy of a particle only depends on which cell ω_j it is in, but *not* on the states of the other particles; that is, we neglect the contribution to the energy of the system that stems from interactions between the particles. We then also assume that the energy E_j of a particle

whose fine-grained state lies in cell ω_j only depends on the index j , i.e. on the cell in which the state is, and not on its exact location within the cell. This can be achieved, for instance, by taking E_j to be the average energy in ω_j . Under these assumptions, the total energy of the system is given by $\sum_{j=1}^l n_j E_j$.

Second, we assume that the system as a whole is large, and that there are many particles in each individual cell: ($n_j \gg 1$ for all j). These assumptions allows us to use Stirling's formula to approximate factorials:

$$n! \approx \sqrt{2\pi n} \left(\frac{n}{e}\right)^n \quad (9)$$

Now we have to maximise $G(D)$ under the 'boundary conditions' that the number n of particles is constant ($n = \sum_j n_j$) and that the total energy E of the system is constant ($E = \sum_j n_j E_j$). Under these assumptions one can then prove (using Stirling's approximation and the Lagrange multiplier method) that $G(D)$ reaches its maximum for

$$n_j = \alpha \exp(-\beta E_j), \quad (10)$$

which is the (discrete) *Maxwell-Boltzmann distribution*, where α and β are constants depending on the nature of the system (Ehrenfest & Ehrenfest 1912, 30; Tolman 1938, Ch. 4).

Before we turn to a discussion of the significance of these calculations, something needs to be said about observable quantities. It is obvious from what has been said so far that observable quantities are averages of the form

$$\langle f \rangle := \sum_{j=1}^n n_j f_{\omega_j}, \quad (11)$$

where f is a function of position and momentum of a particle, and f_{ω_j} is the value of the function in cell ω_j (where, as in the case of the energy, it is assumed that the values of f depend only on the cell ω_j but not of the particle's location within the cell; i.e. it is assumed that f does not fluctuate on a scale of $\delta\omega$). In particular one can calculate the pressure of a gas in equilibrium in this way.²⁰

²⁰In practice this is not straightforward. To derive the desired results, one first has to express the Maxwell-Boltzmann distribution in differential form, transform it to position and momentum coordinates and take a suitable continuum limit. For details see, for instance, Tolman (1938, Ch. 4).

What is the relevance of these considerations for the Boltzmann approach? The answer to this question is not immediately obvious. Intuitively one would like to associate the Γ_{D_i} with the system's macro-regions Γ_{M_i} . However, such an association is undercut by the fact that the Γ_{D_i} are $6n$ dimensional objects, while the Γ_{M_i} , as defined by Equation (1), are subsets of the $6n - 1$ dimensional energy hypersurface Γ_E .

Two responses to this problem are possible. The first is to replace the definition of a macro-region given in the previous subsection by one that associates macro-states with $6n$ dimensional rather than $6n - 1$ dimensional parts of Γ_γ , which amounts to replacing Equation (1) by $\Gamma'_{M_i} := \{x \in \Gamma_\gamma \mid M_i = M(x)\}$ for all $i = 1, \dots, m$. Macro-states thus defined can then be identified with the regions of Γ_γ corresponding to a given distribution: $\Gamma'_{M_i} = \Gamma_{D_i}$ for all $i = 1, \dots, m$, where m now is the number of different distributions.

This requires various adjustments in the apparatus developed in Subsection 2.1, most notably in the definition of the Boltzmann entropy. Taking the lead from the idea that the Boltzmann entropy is the logarithm of the hypervolume of the part of the phase space associated with a macro-state we have

$$S'_B(M_i) := k_B \log[\mu_L(\Gamma'_{M_i})], \quad (12)$$

and with Equation (8) we get

$$S'_B(M_i) = k_B \log[G(D_i)] + k_B n \log(\delta\omega). \quad (13)$$

Since the last term is just an additive constant it can be dropped (provided we keep the partition fixed), because ultimately we are interested in entropy differences rather than in absolute values. We then obtain $S'_B(M_i) = k_B \log[G(D_i)]$, which is the definition of the Boltzmann entropy we find in Albert (2000, 50).

In passing it is worth mentioning that S'_B can be expressed in alternative ways. If we plug Equation (6) into Equation (13) and take into account the above assumption that all n_j are large (which allows us to use Stirling's approximation) we obtain (Tolman 1938, Ch. 4):

$$S'_B(M_i) \approx -k_B \sum_j n_j \log n_j + c(n, \delta\omega), \quad (14)$$

where the n_j are the occupation numbers of distribution D_i and $c(n, \delta\omega)$ is a constant depending on n and $\delta\omega$. Introducing the quotients $p_j := n_j/n$ and plugging them into the above formula we find

$$S'_B(M_i) \approx -n k_B \sum_j p_j \log p_j + \tilde{c}(n, \delta\omega), \quad (15)$$

where, again, $\tilde{c}(n, \delta\omega)$ is a constant depending on n and $\delta\omega$.²¹ The quotients p_i are often said to be the probability of finding a randomly chosen particle in cell ω_i . This is correct, but is important not to confuse this probability, which is simply a finite relative frequency, with the probabilities that occur in BL. In fact, the two have nothing to do with each other.

What are the pros and the cons of this first response? The obvious advantage is that it provides an explicit construction of the macro-regions Γ'_{M_i} , and that this construction gives rise to a definition of the Boltzmann entropy which allows for easy calculation of its values.

The downside of this ‘ $6n$ dimensional approach’ is that the macro-regions Γ'_{M_i} almost entirely consist of micro-states which the system never visits (remember that the motion of the system’s micro-state is confined to the $6n - 1$ dimensional energy hypersurface). This is a problem because it is not clear what relevance considerations based on the hypervolume of certain parts of the phase space have if we know that the system’s actual micro-state only ever visits a subset of these parts which is of measure zero. Most notably, of what relevance is the observation that the equilibrium macro-region has the largest ($6n$ dimensional) hypervolume if the system can only ever access a subset of measure zero of this macro-region? Unless there is a relation between the $6n - 1$ dimensional hypervolume of relevant parts of the energy hypersurface and the $6n$ dimensional hypervolume of the parts of Γ_γ in which they lie, considerations based on the $6n$ dimensional hypervolume are inconsequential.²²

The second response to the above problem leaves the definition of macro-regions as subsets of the $6n - 1$ dimensional energy hypersurface unaltered

²¹This expression for the Boltzmann entropy is particularly useful because, as we shall see in Subsection 3.6.1, $\sum_j p_j \log p_j$ is a good measure for the ‘flatness’ of the distribution p_j .

²²Moreover, the ‘ $6n$ dimensional approach’ renders the ‘orthodox’ account of SM probability, the time average interpretation (see Subsection 2.4), impossible. This interpretation is based on the assumption that the system is ergodic on the union of the macro-regions, which is impossible if macro regions are $6n$ dimensional.

and endeavours to ‘translate’ the results of the combinatorial argument back into the original framework (as presented in Subsection 2.1). This, as we shall see, is possible, but only at the cost of introducing a further hypothesis postulating a relation between the values of the $6n$ and the $6n - 1$ dimensional hypervolumes of relevant parts of Γ_γ .

The most important achievement of the combinatorial argument is the construction of the Γ_{D_i} , the regions in phase space occupied by micro-states with the same macro-properties. Given that the original framework does not provide a recipe of how to construct the macro-regions, we want to make use of the Γ_{D_i} to define the Γ_{M_i} . A straightforward way to obtain the Γ_{M_i} from the Γ_{D_i} is intersect the Γ_{D_i} with Γ_E :²³

$$\Gamma_{M_i} := \Gamma_{D_i} \cap \Gamma_E. \quad (16)$$

How can we calculate the Boltzmann entropy of the macro-states corresponding to macro-regions thus defined? The problem is that in order to calculate the Boltzmann entropy of these states we need the $6n - 1$ dimensional hypervolume of the Γ_{M_i} , but what we are given (via Equation (8)) is the the $6n$ dimensional hypervolume of the Γ_{D_i} , and there is no way to compute the former on the basis of the latter.

The way out of this impasse is to introduce a new postulate, namely that the $6n - 1$ dimensional hypervolume of the Γ_{M_i} is proportional to $6n$ dimensional hypervolume of the Γ_{D_i} : $\mu_{L,E}(\Gamma_{M_i}) = k_v \mu_L(\Gamma_{D_i})$, where k_v is a proportionality constant. It is plausible to assume that this postulate is at least approximately correct because the energy hypersurface of characteristic SM systems is smooth and does not oscillate on the scale of $\delta\omega$. Given this, we have

$$S_B(M_i) = S'_B(M_i) + k_B \log(k_v); \quad (17)$$

²³This construction implicitly assumes that there is a one-to-one correspondence between distributions and macro-states. This assumption is too simplistic in at least two ways. First, $\Gamma_{D_i} \cap \Gamma_E$ may be empty for some i . Second, characteristically several distributions correspond to the same macro-state in that the macroscopic parameters defining the macro-state assume the same values for all of them. These problems can easily be overcome. The first can be solved by simply deleting empty M_i from the list of macro-regions; the second can be overcome by intersecting Γ_E not with each individual Γ_{D_i} , but instead with the union of all Γ_{D_i} that correspond to the same macro-state. Since this would not alter any of the considerations to follow, I disregard this issue henceforth.

that is, S_B and S'_B differ only by an additive constant, and so Equation (13) as well as Equations (14) and (15) can be used to determine the values of S_B .

In what follows I assume that this ‘proportionality assumption’ holds water and that the Boltzmann entropy of a macro-state can be calculated using Equation (17).

2.3 Problems and Tasks

In this subsection I point out what the issues are that the Boltzmannian needs to address in order to develop the approach introduced so-far into a full-fledged account of SM. Needless to say, these issues are not independent of each other and the response to one bears on the responses to others.

2.3.1 Issue 1: The Connection with Dynamics

The Boltzmannian account as developed so far does not make reference to dynamical properties of the system other than the conservation of energy, which is a consequence of Hamilton’s equations of motion. But not every dynamical system—not even if it consists of a large number of particles—behaves thermodynamically in that the Boltzmann entropy increases most of the time.²⁴ For such behaviour to take place it must be the case that a system, which is initially prepared in any low entropy state, eventually moves towards the region of Γ_γ associated with equilibrium. This is illustrated in Figure 4 (which is adapted from Penrose 1989, 401 and 407).

[Insert Figure 4 (appended at the end of the document).]

Figure 4: Trajectory from a low entropy state to a region associated with equilibrium.

But this need not be so. If, for instance, the initial low entropy macro-region

²⁴Lavis (2005, 254-61) criticises the standard preoccupation with ‘local’ entropy increase as misplaced and suggests that what SM should aim to explain is so-called thermodynamic-like behaviour, namely that the Boltzmann entropy be close to its maximum most of the time.

is separated from the equilibrium region by an invariant surface, then no approach to equilibrium takes place. Hence, the question is: of what kind the dynamics has to be for the system to behave thermodynamically.

A common response begins by pointing out that equilibrium is not only associated with the largest part of Γ_γ ; in fact, the equilibrium macro-region is *enormously* larger than *any* other macro-region (Ehrenfest & Ehrenfest 1912, 30). Numerical considerations show that the ratio $\Gamma_{M_{eq}}/\Gamma_M$, where M is a typical non-equilibrium distribution, is of the magnitude of 10^n (Goldstein 2001, 43; Penrose 1989, 403). If we now assume that the system's state drifts around more or less 'randomly' on $\Gamma_{\gamma,a}$ then, because $\Gamma_{M_{eq}}$ is vastly larger than any other macro region, sooner or later the system will reach equilibrium and stay there for at least a very long time. The qualification 'more or less randomly' is essential. If the motion is too regular, it is possible that the system successfully avoids equilibrium positions. But if the state wanders around on the energy hypersurface randomly, then, the idea is, it simply cannot avoid moving into the region associated with equilibrium sooner or later.

Plausible as it may seem, this argument has at best heuristic value. What does it mean for a system to drift around randomly? In particular in the context of Hamiltonian mechanics, a deterministic theory, the notion of drifting around randomly is in need of explanation: what conditions does a classical system have to satisfy in order to possess 'random properties' sufficient to warrant the approach to equilibrium?

2.3.2 Issue 2: Introducing and Interpreting Probabilities

There are several different (albeit interrelated) issues that must be addressed in order to understand the origin and meaning of probabilities in SM, and all of them are intimately connected to issue 1. The first of these is the problem of interpretation.

The interpretation of SM probabilities. How are SM probabilities to be understood? Approaches to probability can be divided into two broad groups.²⁵ First, *epistemic approaches* take probabilities to be measures for degrees of belief. Those who subscribe to an *objective epistemic theory* take proba-

²⁵What follows is only the briefest of sketches. Those options that have been seriously pursued within SM will be discussed in more detail below. For an in-depth discussion of all these approaches see, for instance, Howson (1995), Gillies (2000), Galavotti (2005) and Mellor (2005).

bilities to be degrees of rational belief, whereby ‘rational’ is understood to imply that given the same evidence all rational agents have the same degree of belief in any proposition. This is denied by those who hold a *subjective epistemic theory*, regarding probabilities as subjective degrees of belief that can differ between persons even if they are presented with the same body of evidence.²⁶ Second, *ontic approaches* take probabilities to be part of the ‘furniture of the world’. On the *frequency approach*, probabilities are long run frequencies of certain events. On the *propensity theory*, probabilities are tendencies or dispositions inherent in objects or situations. The *Humean best systems approach*—introduced in Lewis (1986)—views probability as defined by the probabilistic laws that are part of that set of laws which strike the best balance between simplicity, strength and fit.²⁷ To which of these groups do the probabilities introduced in the Boltzmannian scheme belong?

We have introduced two different kinds of probabilities (micro and macro), which, *prima facie*, need not be interpreted in the same way. But before delving into the issue of interpretation, we need to discuss whether these probabilities can, as they should, explain Boltzmann’s law. In fact, serious problems arise for both kinds of probabilities.

Macro-Probabilities. Boltzmann suggested that macro-probabilities explain the approach to equilibrium: if the system is initially prepared in an improbable macro-state (i.e. one far away from equilibrium), it will from then on evolve towards more likely states until it reaches, at equilibrium, the most likely state (1877, 165). This happens because Boltzmann takes it as a given that the system ‘always evolves from an improbable to a more probable state’ (*ibid.*, 166).

This assumption is unwarranted. Equation (4) assigns *unconditional* probabilities to macro-states, and as such they do not imply anything about the succession of states, let alone that ones of low probability are followed by ones of higher probability. As an example consider a brassed die; the probability to get a ‘six’ is 0.25 and all other numbers of spots have probability 0.15. Can we then infer that after, say, a ‘three’ we have to get a ‘six’ because the six is the most likely event? Of course not; in fact, we are much more likely not to get a ‘six’ (the probability for non-six is 0.75, while the

²⁶‘Subjective probability’ is often used as a synonym for ‘epistemic probability’. This is misleading because not all epistemic probabilities are also subjective. Jaynes’s approach to probabilities, to which I turn below, is a case in point.

²⁷Sometimes ontic probabilities are referred to as ‘objective probabilities’. This is misleading because epistemic probabilities can be objective as well.

one for six is 0.25). A further (yet related) problem is that BL makes a statement about a *conditional* probability, namely the probability of the system's macro-state at t_2 being such that $S_B(t_2) > S_B(t_1)$, *given* that the system's macro-state at the earlier time t_1 was such that its Boltzmann entropy was $S_B(t_1)$. The probabilities of PP (see Equation (4)) are not of this kind, and they cannot be turned into probabilities of this kind by using the elementary definition of conditional probabilities, $p(B|A) = p(B \& A)/p(A)$, for reasons pointed out by Frigg (2007a). For this reason non-equilibrium SM cannot be based on the PP, no matter how the probabilities in it are interpreted.

However, PP *does* play a rôle in equilibrium SM. It posits that the equilibrium state is the most likely of all states and hence that the system is most likely to be in equilibrium. This squares well with an intuitive understanding of equilibrium as the state that the system reaches after a (usually short) transient phase, after which it stays there (remember the spreading gas in the introduction). Granting that, what notion of probability is at work in PP? And why, if at all, is this postulate true? That is, what facts about the system make it the case that the equilibrium state is indeed the most likely state? These are the questions that Boltzmannian equilibrium SM has to answer, and I will turn to these in Subsection 2.4.

Micro-Probabilities. The conditional probabilities needed to explain BL can be calculated on the basis of SP (see Equation (5)).²⁸ Let M be the macro-state of a system at time t . For every point $x \in \Gamma_M$ there is a matter of fact (determined by the Hamiltonian of the system) about whether x evolves into a region of higher or lower entropy or stays at the same level of entropy. Call Γ_{M_+} , Γ_{M_-} , and Γ_{M_0} the sets of those points of Γ_M that evolve towards a region of higher, lower, or same entropy respectively (hence $\Gamma_M = \Gamma_{M_+} \cup \Gamma_{M_-} \cup \Gamma_{M_0}$). The probability for the system's entropy to either stay the same or increase as time evolves is $\mu(\Gamma_{M_+} \cup \Gamma_{M_0})/\mu(\Gamma_M)$. Hence, it is a necessary and sufficient condition for BL to be true that $\mu(\Gamma_{M_+} \cup \Gamma_{M_0}) \gg \mu(\Gamma_{M_-})$ for all micro-states M except the equilibrium state itself (for which, trivially, $\mu(\Gamma_{M_+}) = 0$). BL then translates into the statement that the overwhelming majority of micro-states in every macro-region Γ_M except $\Gamma_{M_{eq}}$ evolve under the dynamics of the system towards regions of higher entropy.

This proposal is seriously flawed. It turns out that *if* the system, in

²⁸To keep things simple I assume that there corresponds only one macro-state to a given entropy value. If this is not the case the exactly same calculations can be made using the union of the macro-regions of all macro-states with the same entropy.

macro-state M , is very likely to evolve towards a macro-state of higher entropy in the future (which we want to be the case), *then*, because of the time reversal invariance of the underlying dynamics, the system is also very likely to have evolved into the current macro-state M from another macro-state M' of *higher* entropy than M (see Appendix A for a discussion of time reversal invariance). So whenever the system is very likely to have a high entropy future it is also very likely to have a high entropy past; see Albert (2000, Ch. 4) for a discussion of this point. This stands in stark contradiction with both common sense experience and BL itself. If we have a lukewarm cup of coffee on the desk, SP makes the radically wrong retrodiction that is overwhelmingly likely that 5 minutes ago the coffee was cold (and the air in the room warmer), but then fluctuated away from equilibrium to become lukewarm and five minutes from now will be cold again. However, in fact the coffee was hot five minutes ago, cooled down a bit and will have further cooled down five minutes from now.

This point is usually attributed to the Ehrenfests. It is indeed true that the Ehrenfests (1912, 32-34) discuss transitions between different entropy levels and state that higher-lower-higher transitions of the kind that I just mentioned are overwhelmingly likely. However, they base their statement on calculations about a *probabilistic* model, their famous urn-model, and hence it is not clear what bearing, if any, their considerations have on deterministic dynamical systems; in fact, some of the claims they make are not in general true in conservative deterministic systems.

Nor is it true that the objection to the proposal follows directly from the time reversal invariance of the underlying dynamics on the simple grounds that everything that can happen in one direction of time can also happen in the other direction. However, one can indeed prove that the statement made in the last paragraph about entropic behaviour is true in conservative deterministic dynamical systems, if SP is assumed (Frigg 2007b). Hence there is a serious problem, because the micro-dynamics and (SP) lead us to expect the system to behave in a way that is entirely different from how the system actually behaves and from what the laws of thermodynamics lead us to expect. The upshot is that the dynamics at the micro-level and SP *by itself* do not underwrite the asymmetrical behaviour that we find at the macro level, and which is captured by BL. Hence the question is: where then does the irreversibility at the macro level come from, if not from the dynamical laws governing the micro constituents of a system? I turn to a discussion of this question in Subsection 2.5.

2.3.3 Issue 3: Loschmidt's Reversibility Objection

As we observed in the introduction, the world is rife with irreversible processes; that is, processes that happen in one temporal direction but not in the other. This asymmetry is built into the Second Law of thermodynamics. As Loschmidt pointed out in controversy with Boltzmann (in the 1870s), this does not sit well with the fact that classical mechanics is time-reversal invariant. The argument goes as follows:²⁹

Premise 1: It follows from the time reversal invariance of classical mechanics that if a transition from state x_i to state x_f (' i ' for 'initial' and ' f ' for 'final') in time span Δ is possible (in the sense that there is a Hamiltonian that generates it), then the transition from state Rx_f to state Rx_i in time span Δ is possible as well, where R reverses the momentum of the instantaneous state of the system (see Appendix A for details).

Premise 2: Consider a system in macro-state M with Boltzmann entropy $S_B(M)$. Let RM be the reversed macro-state, i.e. the one with macro-region $\Gamma_{RM} := \{x \in \Gamma \mid Rx \in \Gamma_M\}$ (basically we obtain RM by reversing the momenta of all particles at all points in Γ_M). Then we have $S_B(M) = S_B(RM)$; that is, the Boltzmann entropy is invariant under R .

Now consider a system that assumes macro-states M_i and M_f , at t_i and t_f respectively, where $S_i := S_B(M_i) < S_B(M_f) =: S_f$ and $t_i < t_f$. Furthermore assume that the system's fine grained state is $x_i \in \Gamma_{M_i}$ at t_i and is $x_f \in \Gamma_{M_f}$ at t_f , and that the transition from x_i to x_f during the interval $\Delta := t_f - t_i$ is allowed by the underlying dynamics. Now, by Premise 1 the system is time reversal invariant and hence the transition from Rx_f to Rx_i during Δ is possible as well. Because, by Premise 2, S_B is invariant under R , we have to conclude that the transition from S_f to S_i is possible as well. This contradicts the Second Law of thermodynamics which says that high to low entropy transitions cannot occur. So we are in the awkward position that a transition that is ruled out by the macro theory is allowed by the micro theory which is supposed to account for why the macro laws are the way they are.

²⁹The following is a more detailed version of the presentation of the argument in Ehrenfest & Ehrenfest (1907, 311).

2.3.4 Issue 4: Zermelo's Recurrence Objection

Poincaré's recurrence theorem says, roughly speaking, that for the systems at stake in SM, almost every initial condition will, after some *finite* time (the Poincaré recurrence time), return to a state that is arbitrarily close to its initial state (see Appendix A for details). As Zermelo pointed out in 1896, this has the unwelcome consequence that entropy cannot keep increasing all the time; sooner or later there will be a period of time during which the entropy of the system decreases. For instance, if we consider again the initial example of the gas (Figure 1), it follows from Poincaré's recurrence theorem that there is a future instant of time at which the gas returns *by itself* to the left half of the container. This stands in contradiction to the second law.

A first attempt to dismiss this objection points to the fact that the time needed for this to happen in a realistic system is several times the age of the universe. In fact Boltzmann himself estimated that the time needed for a recurrence to occur for a system consisting of a cubic centimeter of air was about $10^{10^{19}}$ seconds (Uffink 2007, 984). Hence, we never observe such a recurrence, which renders the objection irrelevant.

This response misses the point. The objection is not concerned with whether we ever *experience* a decrease of entropy; the objection points to the fact that there is an *in-principle* incompatibility between the Second Law and the behaviour of classical mechanical systems. This is, of course, compatible with saying that there need not be any conflict with actual experience.

Another response is to let the number of particles in the system tend towards infinity (which is the basic idea of the so-called thermodynamic limit; see Subsection 3.3.2). In this case the Poincaré recurrence time becomes infinite as well. However, actual systems are not infinite and whether such limiting behaviour explains the behaviour of actual systems is at least an open question.

2.3.5 Issue 5: The Justification of Coarse-Graining

The introduction of a finite partition on the system's μ -space is crucial to the combinatorial argument. Only with respect to such a partition can the notion of a distribution be introduced and thereby the Maxwell-Boltzmann equilibrium distribution be derived. Hence the use of a partition is essential. However, there is nothing in classical mechanics itself that either suggests or even justifies the introduction of a partition. How, then, can coarse-graining

be justified?

This question is further aggravated by the fact that the success of the combinatorial argument crucially depends on the choice of the *right* partition. The Maxwell-Boltzmann distribution is derived under the assumption that n is large and $n_i \gg 1$, $i = 1, \dots, l$. This assumption is false if we choose too fine a partition (for instance one for which $l \geq n$), in which case most n_i are small.

There are also restrictions on what kinds of partitions one can choose. It turns out that the combinatorial argument only works if one partitions phase space. Boltzmann first develops the argument by partitioning the particles' energy levels and shows that in this case the argument fails to reproduce the Maxwell-Boltzmann distribution (1877, 168-190). The argument yields the correct result only if the phase space is partitioned along the position and momentum axes into cells of equal size.³⁰ But why is the method sensitive to such choices and what distinguishes 'good' from 'bad' partitions (other than the ability to reproduce the correct distribution law)?

2.3.6 Issue 6: Limitations of the Formalism

When deriving the Maxwell-Boltzmann distribution in Subsection 2.2, we made the assumption that the energy of a particle depends only on its coarse-grained micro-state, i.e. on the cell in which its fine-grained micro-state comes to lie, which (trivially) implies that a particle's energy does not depend on the other particles' states. This assumption occupies centre stage in the combinatorial argument because the derivation of the Maxwell-Boltzmann distribution depends on it. However, this is true only if there is no interaction between the particles; wherever there is an interaction potential between the particles of a system the argument is inapplicable. Hence, the only system satisfying the assumptions of the argument is the ideal gas (which, by definition, consists of non-interacting particles).

This restriction is severe. Although some real gases approximately behave like ideal gases under certain circumstances (basically: if the density is low), most systems of interest in statistical mechanics cannot be regarded as ideal gases. The behaviour both of solids and liquids (and even of dense gases) essentially depends on the interaction between the micro-constituents of the

³⁰Strictly speaking this requirement is a bit too stringent. One can choose a different (but constant) cell size along each axis and still get the right results (Boltzmann 1877, 190).

system, and a theory that is forced to idealise these interactions away (should this be possible at all) is bound to miss out on what is essential to how real systems behave.³¹

A further limitation is that the argument assumes that all the particles of the system have the same phase space, which essentially amounts to assuming that we are dealing with a system of identical objects. A paradigm example for such a system is a monoatomic gas (e.g. helium). But many systems are not of that kind; most solids, for instance, contain constituents of different types.

Finally, the formalism remains silent about what happens when systems interact with their environments. In practice many systems are not completely isolated and one would like the formalism to cover at least selected kinds of interactions.

Hence, the question is whether the approach can be generalised so that it applies to the cases that, as it stands, are not within its scope.

2.3.7 Issue 7: Reductionism

There is a consensus that the principal aim of SM is to account for the thermodynamic behaviour of a macroscopic system in terms of the dynamical laws governing its micro constituents; and it is a measure of the success of SM how much of TD it is able to reproduce (see Subsection 1.1). In philosophical parlance, the aim of SM is to *reduce* TD to mechanics plus probabilistic assumptions.

What does such a reduction involve? How do the micro and the macro level have to relate to one another in order for it to be the case that the former reduces to the latter? The term ‘reduction’ has been used in many different senses and there is no consensus over what exactly it involves to reduce one domain to another. So we need to specify what exactly is asserted when SM is claimed to reduce TD and to discuss to what extent this assertion is true.

A particular problem for reductionism is that idealisations play a constitutive rôle in SM (Sklar 2000, 740). Depending on which approach we favour, we work with, for example: non-interacting particles or hard spheres in a box instead of ‘realistic’ interactions; or systems of infinite volume and an infinite number of particles; or vanishing densities; or processes that take infinitely long. These idealisations are more than the ‘usual’ inexactness that

³¹A discussion of this point can be found, for instance, in Schrödinger (1952, Ch. 1).

is unavoidable when applying a general theory to the messy world; they are essential to SM since the desired results usually cannot be derived without them. What is the status of results that only hold in highly idealised systems (and often are known to fail in more realistic systems) and what rôle can they play in a reduction of TD to SM?

2.3.8 Plan

Subsection 2.4 presents and discusses the ‘orthodox’ response to Issues 1 and 2, which is based on ergodic theory and the use of macro-probabilities. In Subsections 2.5 and I discuss the currently most influential alternative answer to these issues, which invokes the so-called Past Hypothesis and uses micro-probabilities. Issues 3 and 4 are addressed in Unit 2.6.3. In Subsection 2.7 I deal with Issues 5 and 6, and Issue 7 is discussed in Subsection 2.8.

2.4 The Ergodicity Programme

The best-known response to Issues 1 and 2, if macro probabilities are considered, is based on the notion of ergodicity. For this reason this subsection begins with an introduction to ergodic theory, then details how ergodicity is supposed to address the problems at stake, and finally explains what difficulties this approach faces.

2.4.1 Ergodic Theory

Modern ergodic theory is developed within the setting of dynamical systems theory.³² A dynamical system is a triplet (X, λ, ϕ_t) , where X is a state space endowed with a normalised measure λ (i.e. $\lambda(X) = 1$) and $\phi_t : X \rightarrow X$, where t is a real number, is a one-parameter family of measure preserving automorphisms (i.e. $\lambda(\phi_t(A)) = \lambda(A)$ for all measurable $A \subseteq X$ and for all t); the parameter t is interpreted as time. The Hamiltonian systems considered so far are dynamical systems in this sense if the following associations are made: X is the accessible part of the energy hypersurface; λ is the standard measure on the energy hypersurface, renormalised so that the measure of the accessible part is one; ϕ_t is the Hamiltonian flow.

³²The presentation of ergodic theory in this subsection follows by and large Arnold and Avez (1968) and Cornfeld *et al.* (1982). For accounts of the long and intertwined history of ergodic theory see Sklar (1993, Chs. 2 and 5) and von Plato (1991, 1994, Ch. 3).

Now let $f(x)$ be any complex-valued and Lebesgue-integrable function defined on X . Its space mean \bar{f} (sometimes also ‘phase space average’ or simply ‘phase average’) is defined as

$$\bar{f} := \int_X f(x) d\lambda, \quad (18)$$

and its time mean f^* (sometimes also ‘time average’) at $x_0 \in X$ is defined as

$$f^*(x_0) = \lim_{\tau \rightarrow \infty} (1/\tau) \int_{t_0}^{t_0+\tau} f[\phi_t(x_0)] dt. \quad (19)$$

The question is whether the time mean exists; the Birkhoff theorem asserts that it does:

Birkhoff Theorem. Let (X, λ, ϕ_t) be a dynamical system and f a complex-valued, λ -integrable function on X . Then the time average $f^*(x_0)$

(i) exists almost everywhere (i.e. everywhere except, perhaps, on a set of measure zero);

(ii) is invariant (i.e. does not depend on the initial time t_0): $f^*(x_0) = f^*(\phi_t(x_0))$ for all t ;

(iii) is integrable: $\int_X f^*(x_0) d\lambda = \int_X f(x) d\lambda$.

We can now state the central definition:

Ergodicity. A dynamical system is ergodic iff for every complex-valued, λ -integrable function f on X we have $f^*(x_0) = \bar{f}$ almost everywhere; that is, everywhere except, perhaps, on a set of measure zero.

Two consequences of ergodicity are worth emphasising. First, if a system is ergodic, then for almost all trajectories, the fraction of time a trajectory spends in a region R equals the fraction of the area of X that is occupied by R . This can easily be seen by considering $f(x) = \chi_R(x)$, where $\chi_R(x)$ is the characteristic function of the region R : $\chi_R(x) = 1$ if $x \in R$ and $\chi_R(x) = 0$ otherwise. We then have $\bar{f} = \lambda(R) = f^*(x)$, meaning that the fraction of time the system spends in R equals $\lambda(R)$, which is the fraction of the area of X that is occupied by R .

Second, almost all trajectories (i.e. trajectories through almost all initial conditions) come arbitrarily close to any point on the energy hypersurface infinitely many times; or to put it another way, almost all trajectories pass through every subset of X that has positive measure infinitely many times. This follows from the fact that the time mean equals the space mean, which implies that the time mean cannot depend on the initial condition x_0 . Hence a system can be ergodic only if its trajectory may access all parts of the energy hypersurface.

The latter point is also closely related to the decomposition theorem. We first define:

Decomposability. A system is decomposable (sometimes also ‘metrically decomposable’ or ‘metrically intransitive’) iff there exist two regions X_1 and X_2 of non-zero measure such that $X_1 \cap X_2 = \emptyset$ and $X_1 \cup X_2 = X$, which are invariant under the dynamics of the system: $\phi_t(X_1) \subseteq X_1$ and $\phi_t(X_2) \subseteq X_2$ for all t . A system that is not decomposable is indecomposable (‘metrically indecomposable’ or ‘metrically transitive’).

Then we have:

Decomposition Theorem. A dynamical system is ergodic if and only if it is indecomposable; i.e. if every invariant measurable set has either measure zero or one.

The ergodic measure is unique, up to a continuity requirement, in the sense that there is only one measure invariant under the dynamics. We first define:

Absolute Continuity. A measure λ' is absolutely continuous with respect to λ iff for any measurable region $A \subseteq X$: if $\lambda(A) = 0$ then $\lambda'(A) = 0$.

We then have:

Uniqueness Theorem. Assume that (X, λ, ϕ_t) is ergodic and λ is normalised. Let λ' be another measure on X which is normalised, invariant under ϕ_t , and absolutely continuous with respect to λ . Then $\lambda = \lambda'$.

For what follows it will also be important to introduce the notion of mixing.

Mixing. A system is mixing³³ if and only if for all measurable subsets A and B of X : $\lim_{t \rightarrow \infty} \mu(\phi_t B \cap A) = \mu(A)\mu(B)$.

The meaning of this concept can be visualised as follows. Think of the phase space as a glass of water to which a shot of scotch has been added. The volume of the cocktail X (scotch + water) is $\mu(X)$ and the volume of scotch is $\mu(B)$; hence the concentration of scotch in X is $\mu(B)/\mu(X)$. Now stir. Mathematically, the time evolution operator ϕ_t represents the stirring, meaning that $\phi_t(B)$ is the region occupied by the scotch after time t . The cocktail is mixed, if the concentration of scotch equals $\mu(B)/\mu(X)$ not only with respect to the *entire* ‘glass’ X , but with respect to any arbitrary (but non-zero measure) region A in that volume; that is, it is mixed if $\mu(\phi_t(B) \cap A)/\mu(A) = \mu(B)/\mu(X)$ for any finite volume A . This condition reduces to $\mu(\phi_t(B) \cap A)/\mu(A) = \mu(B)$ for any region B because, by assumption, $\mu(X) = 1$. If we now assume that mixing is achieved only for $t \rightarrow \infty$ we obtain the above condition.

One can then prove the following two theorems.

Implication Theorem. Every dynamical system that is mixing is also ergodic, but not vice versa.

Convergence Theorem. Let (X, λ, ϕ_t) be a dynamical system and let ρ be a measure on X that is absolutely continuous with λ (but otherwise arbitrary). Define $\rho_t(A) := \rho(\phi_t(A))$ for all measurable $A \subseteq X$. Let $f(x)$ be a bounded measurable function on X . If the system is mixing, then $\rho_t \rightarrow \lambda$ as $t \rightarrow \infty$ in the sense that for all such f :

$$\lim_{t \rightarrow \infty} \int f(x) d\rho_t = \int f(x) d\lambda. \quad (20)$$

³³Strictly speaking this property is called ‘strong mixing’ since there is a similar condition called ‘weak mixing’. The differences between these need not occupy us here. For details see Arnold and Avez (1968, Ch. 2).

2.4.2 Promises

Assuming that the system in question is ergodic seems to provide us with neat responses to Issues 1 and 2, if macro-probabilities are considered.

Thus, let us ask the question of how are we to understand statements about the probability of a macro-state? That is, how are we to interpret the probabilities introduced in Equation (4)? A natural suggestion is that probabilities should be understood as time averages. More specifically, the suggestion is that the probability of a macro-state M is the fraction of time that the system's state spends in Γ_M (the so-called sojourn time):

$$p(M) = \frac{1}{\tau} \int_{t_0}^{t_0+\tau} \chi_{\Gamma_M}[\phi_t(x)] dt, \quad (21)$$

where χ_{Γ_M} is the characteristic function (as defined above) and $[t_0, t_0 + \tau]$ is some suitable interval of time.

This definition faces some *prima facie* problems. First, what is the suitable interval of time? Second, does this time average exist? Third, as defined in Equation (21), $p(M)$ exhibits an awkward dependence on the initial condition x . These difficulties can be overcome by assuming that the system is ergodic. In this case the relevant time interval is infinity; the existence question is resolved by Birkhoff's theorem, which states that the infinite time limit exists almost everywhere; and the awkward dependence on the initial condition vanishes because in an ergodic system the infinite time means equals the space means for almost all initial conditions, and hence a fortiori the time mean, does not depend on the initial condition x (for almost all x).

This puts the time average interpretation of SM probabilities on a solid foundation and at the same time also offers a response to the problem of the mechanical foundation of the PP. The combinatorial considerations in the last subsection have shown that the equilibrium state occupies by far the largest part of Γ_γ . Combining this with the fact that the time a system spends in a given region of Γ_γ is proportional to its measure provides a faultless mechanical justification of PP.³⁴

In sum, if the system is ergodic, we seem to have a neat mechanical explanation of the system's behaviour as well as a clear interpretation of the

³⁴For a further discussion of ergodicity and issues in the interpretation of probability in the Boltzmann approach see von Plato (1981, 1982, 1988, 1989), Gutmann (1999), van Lith (2003), Emch (2005).

probabilities that occur in the PP.

2.4.3 Problems

The Ergodic programme faces serious difficulties. To begin with, it turns out to be extremely difficult to prove that the systems of interest really are ergodic. Contrary to what is sometimes asserted, not even a system of n elastic hard balls moving in a cubic box with hard reflecting walls has been proven to be ergodic for arbitrary n ; it has been proven to be ergodic only for $n \leq 4$. Moreover, hard ball systems are highly idealised (molecules do not behave like hard balls) and it is still an open question whether systems with more realistic interaction potentials (e.g. Lenard-Jones potentials) are ergodic.³⁵

What is worse than the absence of proofs that the systems of interest are ergodic is that there are systems that show the appropriate behaviour and yet are known not to be ergodic. For instance, in a solid the molecules oscillate around fixed positions in a lattice, and as a result the phase point of the system can only access a small part of the energy hypersurface (Uffink 2007, 1017). Bricmont (2001) investigates the Kac Ring Model (Kac 1959) and a system of n uncoupled anharmonic oscillators of identical mass, and points out that both systems exhibit thermodynamic behaviour— and yet they fail to be ergodic. And most notably, a system of non-interacting point particles is known not to be ergodic; yet ironically it is exactly this system on which the combinatorial argument is based (Uffink 1996b, 381). Hence, ergodicity is not necessary for thermodynamic behaviour.³⁶ But, as Earman & Redei (1996, 70) and van Lith (2001a, 585) point out, if ergodicity is not necessary for thermodynamic behaviour, then ergodicity cannot provide a satisfactory explanation for this behaviour. Either there must be properties other than ergodicity that explain thermodynamic behaviour in cases in which the system is not ergodic, or there must be an altogether different explanation for

³⁵For further discussions of this issue see Sklar (1993, Ch. 5), Earman & Redei (1996, Sec. 4), Uffink (2007, Sec. 6), Emch & Liu (2002, Chs. 7-9), and Berkovitz *et al.* (2006, Sec. 4).

³⁶It has been argued that ergodicity is not sufficient either because there are systems that are ergodic but don't show an approach to equilibrium, for instance two hard spheres in a box (Sklar 1973, 209). This is, of course, correct. But this problem is easily fixed by adding the qualifying clause that if we consider a system *of interest in the context of SM*—i.e. one consisting of something like 10^{23} particles—then if the system is ergodic it shows SM behaviour.

the approach to equilibrium even for systems which are ergodic.³⁷

But even if a system turns out to be ergodic, further problems arise. All results and definitions of ergodic theory come with the qualification ‘almost everywhere’: the Birkhoff theorem ensures that f^* exists *almost everywhere* and a dynamical system is said to be ergodic iff for every complex-valued, Lebesgue-integrable function f the time mean equals the space mean *almost everywhere*. This qualification is usually understood as suggesting that sets of measure zero can be neglected or ignored. This, however, is neither trivial nor evidently true. What justifies the neglect of these sets? This has become known as the ‘measure zero problem’. The idea seems to be that points falling in a set of measure zero are ‘sparse’ and this is why they can be neglected. This view receives a further boost from an application of the Statistical Postulate, which assigns probability zero to events associated with such sets. Hence, so goes the conclusion, what has measure zero simply doesn’t happen.³⁸

This is problematic for various reasons. First, sets of measure zero can be rather ‘big’; for instance, the rational numbers have measure zero within the real numbers. Moreover, a set of measure zero need not be (or even appear) negligible if sets are compared with respect to properties other than their measures. For instance, we can judge the ‘size’ of a set by its cardinality or Baire category rather than by its measure, which leads us to different conclusions about the set’s size (Sklar 1993, 182-188).

Furthermore it is a mistake to assume that an event with measure zero cannot occur. In fact, having measure zero and being impossible are distinct notions. Whether or not the system at some point was in one of the special initial conditions for which the space and time mean fail to be equal is a factual question that cannot be settled by appeal to measures; pointing out that such points are scarce in the sense of measure theory does not do much, because it does not imply that they are scarce in the world as well.³⁹ All we can do is find out what was the case, and if the system indeed was in one of these initial conditions then considerations based on this equality break down. The fact that SM works in so many cases suggests that they indeed

³⁷The term ‘explanation’ here is used in a non-technical sense; for a discussion of how the use of ergodicity ties in with certain influential philosophical views about explanation see Sklar (1973) and Quay (1978).

³⁸This piece of ‘received wisdom’ is clearly explained but not endorsed in Sklar (2000a, 265-6).

³⁹Sklar (1973, 210-211) makes a very similar point when discussing the Gibbs approach.

are scarce, but this is a matter of fact about the world and not a corollary of measure theory.⁴⁰ Hence, an explanation of SM behaviour would have to consist of the observation that the system is ergodic and that it additionally started in an initial condition which is such that space and time means are equal.

For these reasons a time average interpretation of macro-probabilities is problematic. However, alternative interpretations do not fare better. Frequentism is ruled out by the fact that the relevant events in SM do not satisfy the requirement of von Mises' theory (van Lith 2001, 587), and a propensity interpretation (Popper 1959) fails because the existence of propensities is ultimately incompatible with a deterministic underlying micro theory (Clark 2001).⁴¹

A peculiar way around the problem of interpreting probabilities is to avoid probabilities altogether. This is the strategy pursued, among others, by Goldstein (2001), Lebowitz (1993b), Goldstein & Lebowitz (2004) and Zanghì (2005) in their presentation of the Boltzmannian account. The leading idea of this approach is that equilibrium states are 'typical' while non-equilibrium states are 'atypical', and that the approach to equilibrium can be understood as a transition from atypical to typical states. For a discussion of this approach to SM see Frigg (2007b).

2.5 The Past Hypothesis

2.5.1 The Past Hypothesis Introduced

Let us now turn to Issues 2 and 3, and base our discussion on micro-probabilities. The two problems we have to solve are (a) that high to low entropy transitions are allowed by the dynamics (by the reversibility objection) and (b) that most trajectories compatible with a given non-equilibrium state are ones that have evolved into that state from a state of *higher* entropy (which is a consequence of SP and the time reversal invariance of the micro dynamics).

There is a common and now widely accepted solution to these problems which relies on the fact that a system's actual behaviour is determined by its dynamical laws *and* its initial condition. Hence there need not be a contradiction between time reversal invariant laws and the fact that high

⁴⁰For a further discussion of this issue see Friedman (1976).

⁴¹For a further discussion of this issue see Butterfield (1987) and Clark (1987; 1989; 1995).

to low entropy transitions do not (or only very rarely) occur *in our world*. All we have to do is to assume that the relevant systems in our world have initial conditions which are such that the system's history is indeed one that is characterised by low to high entropy transitions. That initial conditions of this kind are scarce is irrelevant; all that matters is that the system *de facto* started off in one of them. If this is the case, we find the irreversible behaviour that we expect. However, this behaviour is now a consequence not only of the laws governing the system, but also of its special initial condition.

The question is at what point in time the relevant low entropy initial condition is assumed to hold. A natural answer would be that the beginning of an experiment is the relevant instant; we prepare the gas such that it sits in the left half of the container before we open the shutter and this is the low entropy initial condition that we need. The problem with this answer is that the original problem recurs if we draw an entropy curve for the system we find that the low entropy state at the beginning of the experiment evolved another high entropy state. The problem is obvious by now: whichever point in time we chose to be the point for the low entropy initial condition to hold, it follows that the overwhelming majority of trajectories compatible with this state are such that their entropy was higher in the past. An infinite regress looms large. This regress can be undercut by assuming that there is an instant that simply has no past, in which case it simply does not make sense to say that the system has evolved into that state from another state. In other words, we have to assume that the low entropy condition holds at the beginning of the universe.

At this point modern cosmology enters the scene: proponents of Boltzmannian SM take cosmology to inform us that the universe was created in the big bang a long but finite time ago and that it then was in a low entropy state. Hence, modern cosmology seems to provide us with exactly what we were looking for. This is a remarkable coincidence, so remarkable that Price sees in it 'the most important achievement of late-twentieth-century physics' (2004, 228). The posit that the universe has come into existence in a low entropy state is now (following Albert 2000) commonly referred to as the 'Past Hypothesis' (PH); let us call the state that it posits the 'Past State'. In Albert's formulation PH is the claim

[...] that the world first came into being in whatever particular low-entropy highly condensed big-bang sort of macrocondition it is that the normal inferential procedures of cosmology will even-

tually present to us' (2000, 96).

This idea can be traced back to Boltzmann (see Uffink 2007, 990) and has since been advocated, among others, by Feynman (1965, Ch. 5), Penrose (1989, Ch. 7; 2006, Ch. 27), Price (1996, 2004, 2006), Lebowitz (1993a, 1993b, 1999), Albert (2000), Goldstein (2001), Callender (2004a, 2004b), and Wald (2006).

There is a remarkable consensus on the formulation and content of PH; different authors diverge in what status they attribute to it. For Feynman, Goldstein, and Penrose PH, seems to have the status of a law, which we simply add to the laws we already have. Whether such a position is plausible depends on one's philosophical commitments as regards laws of nature. A discussion of this issue would take us too far afield; surveys of the philosophical controversies surrounding the concept of a law of nature can be found in, among others, Armstrong (1983), Earman (1984) and Cartwright and Alexandrova (2006). Albert regards PH as something like a Kantian regulative principle in that its truth has to be assumed in order to make knowledge of the past possible at all. On the other hand, Callender, Price, and Wald agree that PH is not a law, but just a contingent matter of fact; but they have conflicting opinions about whether this fact is in need of explanation.⁴² Thus for Price (1996, 2004) the crucial question in the foundation of SM is not so much why entropy increases, but rather why it ever got to be so low in the first place. Hence, what really needs to be explained is why the universe shortly after the big bang was in the low entropy state that PH posits. Callender (1998, 2004a, 2004b) argues that this quest is wrong. PH simply specifies initial conditions of a process because initial conditions, irrespective of whether they are special or not, are not the kind of thing that is in need of explanation. Similar concerns have also been raised by Sklar (1993, 309-318).

2.5.2 Problems and Criticisms

PH has recently come under attack. Earman (2006) argues that what at first glance looks like a great discovery—that modern cosmology posits exactly the kind of Past State that the Boltzmannian account requires—turns out

⁴²Notice that this view has the consequence that the Second Law of thermodynamics, or rather its 'statistical cousin', Boltzmann's Law, becomes a de facto regularity and is thus deprived of its status as a law properly speaking.

to be ‘not even false’ (400). Earman first investigates a particular Friedman-Robertson-Walker model of cosmology suggested by Hawking and Page and shows that in this model probabilities are typically ill-defined or meaningless, and he then argues that this result is not an artefact of the idealisations of the models and would crop up equally in more realistic models (417-418). Hence, for the cosmologies described in general relativity there is no well-defined sense in which the Boltzmann entropy has a low value. And worse, even if quantum gravity or some other yet to be discovered theory came to rescue and made it possible to give a well-defined expression for the Boltzmann entropy at the beginning of the universe, this would be of little help because the dynamics of the cosmological models does not warrant the claim that there will be monotonic increase in entropy (418-420). For these two reasons, Earman concludes, the past hypothesis is untenable.

Whatever the eventual verdict of Earman’s critique of PH, there is a further problem is that the Boltzmann entropy is a global quantity characterising the macro-state of an entire system, in this case the entire universe. The fact that this quantity is low does not imply that the entropy of a particular small subsystem of interest is also low. And what is worse, just because the overall entropy of the universe increases it need not be the case that the entropy in a small subsystem also increases. A decrease in the entropy in one part of the universe may be balanced by an increase in entropy in some other part of the universe and hence is compatible with an increase in the overall entropy. Hence, SM cannot explain the behaviour of small systems like gases in laboratories. Winsberg (2004a, 499-504) addresses this problem and argues that the only way to avoid it is to make a further conjecture about the theory (he calls it ‘Principle 3’), which in effect rules out local ‘entropic misbehaviour’. However, as he points out, this principle is clearly false and hence there is no way for the Boltzmannian to rule out behaviour of this kind.

it is not the time to notice that a radical shift has occurred at the beginning of this subsection. We started with a pledge to explain the behaviour of homely systems like a vessel full of gas and ended up talking about the Big Bang and the universe as a whole. At least to some, this looks like using a sledgehammer to crack nuts, and not a very wise move because most of the problems that it faces are caused by the move to the cosmological scale. The natural reaction to this is to downsize again and talk about laboratory scale systems. This is what happens in the so-called ‘branch systems approach’, which is inspired by Reichenbach’s (1956) discussion of the direction of time,

and is fully articulated in Davies (1974) and discussed in Sklar (1993, 318-332).

The leading idea is that the isolated systems relevant to SM have neither been in existence forever, nor continue to exist forever after the thermodynamic processes took place. Rather, they separate off from the environment at some point (they ‘branch’) then exist as energetically isolated systems for a while and then usually merge again with the environment. Such systems are referred to as ‘branch systems’. For instance, the system consisting of a glass and an ice cube comes into existence when someone puts the ice cube into the water, and it ceases to exist when someone pours it into the sink. So the question becomes why a branch system like the water with the ice cube behaves in the way it does. An explanation can be given along the lines of the past hypothesis, with the essential difference that the initial low entropy state has to be postulated not for the beginning of the universe but only for the state of the system immediately after the branching. Since the system, by stipulation, did not exist before that moment, there is also no question of whether the system has evolved into the current state from a higher entropy state. This way of looking at things is in line with how working physicists think about these matters for the simple reason that low entropy states are routinely prepared in laboratories—hence Lebowitz’s (1993b, 11) remark that the origin of low entropy initial states is no problem in laboratory situations.

Albert dismisses this idea as ‘sheer madness’ (2000, 89) for three reasons. First, it is impossible to specify the precise moment at which a particular system comes into being; that is, we cannot specify the precise branching point. Second, there is no unambiguous way to individuate the system. Why does the system in question consist of the glass with ice, rather than the glass with ice and the table on which the glass stands, or the glass and ice and the table and the person watching it, or ... And this matters because what we regard as a relevant low entropy state depends on what we take the system to be. Third, it is questionable whether we have any reason to assume, or whether it is even consistent to claim, that SP holds for the initial state of the branch system.⁴³

The first and the second criticism do not seem to be successful. Why should the system’s behaviour have anything to do with our inability to *de-*

⁴³As we shall see in the next subsection, it is necessary to assume that SP holds for the initial state. Proponents of the past hypothesis and of the branch systems approach differ in what they regard as the beginning.

cide at what instant the system becomes energetically isolated? So Albert's complaint must be that there is no matter of the fact about when a system becomes isolated. If this was true, it would indeed be a problem. But there does not seem to be a reason why this should be so. If we grant that there is such a thing as being isolated from one's environment (an assumption not challenged in the first criticism), then there does not seem to be a reason to claim that becoming isolated at some point in time should be more problematic than the lights going off at some point in time, or the game beginning at some point in time, or any other event happening at some instant. The second criticism does not cut any ice either (see Winsberg 2004b, 715). Being energetically isolated from the rest of the universe is an objective feature of certain things and not others. The glass and its contents are isolated from the rest of the universe and this is what makes them a branch system; the table, the observer, the room, the house, etc. are not, and this is why they are not branch systems. There is nothing subjective or arbitrary about this division. One can, of course, question whether systems ever really are isolated (we come to this in Subsection 3.5.2). But this is a different point. If one goes down that road, then there simply are no branch systems; but then there is no individuation problem either.

The third criticism leads us into deep waters. Why would we want to deny that SP applies to the branch system at the instance of its creation? Although Albert does not dwell on this point, his reasoning seems to be something like the following (see Winsberg 2004b, 715-717). Take the universe at some particular time. Now things happen: someone opens the freezer, takes an ice cube and puts it into a glass of lukewarm water. These are physical processes governed by the laws of mechanics; after all, at the micro level all that happens is that swarms of particles move around in some specific way. But then the micro-state of the glass with ice is determined by the laws of mechanics and the micro-condition at the earlier point of time and we can't simply 'reset' the glass' state and postulate that it is now such that SP, or any other condition for that matter, holds. In brief, the glass' state at some point is dictated by the laws of the theory and is not subject to stipulations of any kind.

Whether or not one finds this criticism convincing depends on one's philosophical commitments as regards the nature of laws. The above argument assumes that laws are universal and valid all the time; it assumes that not only the behaviour of the water and the ice, but also of the table, the room, the fridge and, last but not least, the person putting the ice into the water

and everything else in the universe are governed by the laws of mechanics. If one shares this view, then Albert's third criticism is valid. However, this view of laws is not uncontroversial. It has been argued that the domain of applicability of laws is restricted: we are making a mistake if we assume them to be universal. To someone of the latter persuasion the above argument has no force at all against branch systems. This conflict surfaces again when we discuss the interventionist approach to SM in Subsection 3.5.2 and for this reason I postpone till then a more detailed discussion of the issue of the scope of laws.

2.6 Micro-Probabilities Revisited

As we have seen above, SP gives us wrong retrodictions and this needs to be fixed. PH, as introduced in the last subsection, seems to provide us with the means to reformulate SP so that this problem no longer arises (Unit 2.6.1). Once we have a rule that assigns correct probabilities to past states, we come back to the question of how to interpret these probabilities (2.6.2) and then address the reversibility and recurrence objections (Unit 2.6.3).

2.6.1 Conditionalising on PH

PH, if true, ensures that the system indeed starts in the desired low entropy state. But, as we have seen in Unit 2.3.2, our probabilistic machinery tells us that this is overwhelmingly unlikely. Albert (2000, Ch. 4) argues that this is unacceptable since it just cannot be that the *actual* past is overwhelmingly unlikely for this would lead us to believe wrong things about the past.⁴⁴ The source of this problem is that we have (tacitly) assumed that SP is valid at all times. Hence this assumption must be renounced and a postulate other than SP must be true at some times.

Albert (2000, 94-96) suggests the following remedy: SP is valid only for the Past State (the state of universe just after the Big Bang); for all later

⁴⁴In fact, Albert (2000, Chs. 4 and 6) even sees this as a fundamental problem threatening the very notion of having knowledge of the past. Leeds (2003) takes the opposite stance and points out that this conclusion is not inevitable since it depends on the view that we explain an event by its having a high probability to occur. Explaining the past, then, involves showing that the actual past has high probability. However, if we deny that we are in the business of explaining the past on the basis of the present and the future, then this problem looks far less dramatic. For a further discussion of Albert's view on past knowledge and intervention see Frisch (2005) and Parker (2005).

states the correct probability distribution is the one that is uniform (with respect to the Lebesgue measure) over the set of those conditions that are compatible with the current macro-state *and* the fact that the original macro-state of the system (at the very beginning) was the Past State. In brief, the suggestion is that we conditionalise on the Past Hypothesis and the current macro-state.

More precisely, let M_P be the macro-state of the system just after the big bang (the Past State) and assume (without loss of generality) that this state obtains at time $t = 0$; let M_t be the system's macro-state at time t and let $\Gamma_t := \Gamma_{M_t}$ be the parts of $\Gamma_{\gamma,a}$ that corresponds to M_t . Then we have:

Past Hypothesis Statistical Postulate (PHSP): SP is valid for the Past State. For all times $t > 0$, the probability at t that the fine-grained micro-state of the system lies in a subset A of Γ_t is

$$\mu_{L,t}(A) := \frac{\mu_L(A \cap R_t)}{\mu_L(R_t)} \quad (22)$$

whenever $R_t \neq \emptyset$, where $R_t := \Gamma_t \cap \phi_t(\Gamma_P)$ and $\phi_t(\Gamma_P)$, as above, is the image of Γ_P under the dynamics of the system after time t has elapsed.

This is illustrated in Figure 5. Now, by construction, those fine-grained micro-states in Γ_t which have a high entropy past have probability zero, which is what we needed.

[Insert Figure 5 (appended at the end of the document).]

Figure 5: Illustration of the past hypothesis statistical postulate.

However, PHSP needs to be further qualified. There might be a ‘conspiracy’ in the system to the effect that states with a low entropy past and ones with a low entropy future are clumped together. Let $\Gamma_{t,f}$ be the subregions of Γ_t occupied by states with a low entropy future. If it now happens that these lie close to those states compatible with PH, then PHSP— wrongly—predicts that a low entropy future is very likely despite the fact that the fraction of Γ_t

occupied by $\Gamma_{t,f}$ is tiny and that SP—correctly—predicts that a low entropy future is very unlikely (see Figure 6).

[Insert Figure 6 (appended at the end of the document).]

Figure 6: Illustration of a conspiracy involving clumping together of low entropy past and low entropy future states.

This problem can be avoided by requiring that $\Gamma_{t,f}$ is scattered in tiny clusters all over Γ_t (see Albert 2000, 67, 81-85) so that the fraction of $\Gamma_{t,f}$ that comes to lie in R_t is exactly the same as the fraction of Γ_t taken up by $\Gamma_{t,f}$, i.e. $\mu_L(\Gamma_{t,f})/\mu_L(\Gamma_t) = \mu_L(\Gamma_{t,f} \cap R_t)/\mu_L(R_t)$ (see Figure 7).

[Insert Figure 7 (appended at the end of the document).]

Figure 7: Scattering condition as solution to the conspiracy problem.

Let us call this the ‘scattering condition’. If this condition falls in place, then the predictions of PHSP and SP coincide and the problem is solved.

In sum, replacing SP by PHSP and requiring that the scattering condition holds for all times t is sufficient to get both predictions and retrodictions right.

The remaining question is, of course, whether the scattering condition holds. Albert simply claims that it is plausible to assume that it holds, but he does so without presenting, or even mentioning, a proof. Since this condition concerns mathematical facts about the system, we need a proof, or at least a plausibility argument, that it holds. Such a proof is not easy to get because the truth of this condition depends on the dynamics of the system.

2.6.2 Interpreting Micro-Probabilities

How are we to interpret the probabilities defined by PHSP? Frequentism, time averages and the propensity interpretation are unworkable for the same reasons as in the context of macro-probabilities. Loewer (2001, 2004) suggested that the way out of the impasse is to interpret PHSP probabilities as Humean chances in Lewis' (1994) sense. Consider all deductive systems that make true assertions about what happens in the world and also specify probabilities of certain events. The *best* system is the one that strikes the best balance between simplicity, strength and fit, where the fit of a system is measured by how likely the system regards it that things go the way they actually do. Lewis then proposes as an analysis of the concept of a law of nature that laws are the regularities of the best system and chances are whatever the system asserts them to be. Loewer suggests that the package of classical mechanics, PH and PHSP is a putative best system of the world and that therefore the chances that occur in this system can be understood as chances in Lewis' sense.

Frigg (2006, 2007a) argues that this suggestion faces serious difficulties. First, Lewis' notion of fit is modeled on the frequentist notion of a sequence and cannot be carried over to a theory with continuous time. Second, even when discretising time in order to be able to calculate fit, it turns out that Loewer's putative best system is not the best system because there are distributions over the initial conditions that lead to a better fit of the system than the distribution posited in PHSP. The details of these arguments suggest that PHSP probabilities are best understood as epistemic probabilities of some sort.

2.6.3 Loschmidt's and Zermelo's Objections

We now return to Loschmidt's and Zermelo's objections and discuss in what way the micro probability approach can address them.

Reversal Objection: Consider the same scenario as in Unit 2.3.3. Denote by Γ_{if} the subset of Γ_{M_i} consisting of all points that evolve into Γ_{M_f} during the interval Δ , and likewise let Γ_{fi} be set of all points in Γ_{M_f} that evolve into Γ_{M_i} during Δ . We then have $\Gamma_{fi} = R(\phi_\Delta(\Gamma_{if}))$, where ϕ_Δ is the time evolution of the system during time span Δ . Therefore $\mu(\Gamma_{fi})/\mu(\Gamma_{M_f}) = \mu(R(\phi_\Delta(\Gamma_{if})))/\mu(\Gamma_{M_f}) = \mu(\Gamma_{if})/\mu(\Gamma_{M_f})$, because $\mu(RA) = \mu(A)$ for all sets A . By assumption $\mu(\Gamma_{M_f}) > \mu(\Gamma_{M_i})$ (because M_f has higher entropy than

M_i), hence $\mu(\Gamma_{if})/\mu(\Gamma_{M_f}) < \mu(\Gamma_{if})/\mu(\Gamma_{M_i})$. Assuming that conditionalising on PH would not upset these proportions, it follows that the system is more likely to evolve from low to high entropy than it is to evolve from high to low entropy. Now take M_i and M_f to be, respectively, the state of a gas confined to the left half of container and the state of the gas spread out evenly over the entire available space. In this case $\mu(\Gamma_{M_f})/\mu(\Gamma_{M_i}) \approx 10^n$ (n being the number of particles in the system), and hence the system is 10^n times more likely to evolve from low to high entropy than vice versa. This is what BL asserts.⁴⁵

Recurrence Objection: Roughly speaking, the recurrence objection (see Subsection 2.3.4) states that entropy cannot always increase because every mechanical system returns arbitrarily close to its initial state after some *finite* time (Poincaré’s Recurrence Theorem). The common response (Callender 1999, 370; Bricmont 1996, Sec. 4) to the recurrence objection has a somewhat empiricist flavour and points out that, according to the Past Hypothesis, the universe is still today in a low entropy state far away from equilibrium and recurrence will therefore presumably not occur within all relevant observation times. This, of course, is compatible with there being periods of decreasing entropy at some later point in the history of the universe. Hence, we should not view BL as valid at all times.

2.7 Limitations

There are serious questions about the use of coarse graining, i.e. partitions, in the combinatorial argument (issue 5) and the scope of the theory (issue 6). I now discuss these problems one at a time.

How can coarse-graining be justified? The standard answer is an appeal to knowledge: we can never observe the precise value of a physical quantity because measuring instruments invariably have a finite resolution (just as do human observation capabilities); all we can assert is that the result lies within a certain range. This, so the argument goes, should be accounted for in what we assert about the system’s state and the most natural way to do this is to choose a partition whose cell size reflects what we can reasonably hope to know about the system.

This argument is problematic because the appeal to observation introduces a kind of subjectivity into the theory that does not belong there. Sys-

⁴⁵See Bricmont (1996, Sec. 3) for a more detailed discussion.

tems approach equilibrium irrespective of what we happen to know about them. Hence, so the objection concludes, any reference to incomplete knowledge is out of place.⁴⁶

Another line of argument is that there exists an objective separation of relevant scales—in that context referred to as ‘micro’ and ‘macro’⁴⁷—and that this justifies coarse-graining.⁴⁸ The distinction between the two scales is considered objective in much the same way as, say, the distinction between dark and bright: it may not be clear where exactly to draw the line, but there is no question that there is a distinction between dark and bright. From a technical point of view, the separation of scales means that a macro description is bound to use a finite partition (whose cell size depends on where exactly one draws the line between the micro and macro scales). This justifies Boltzmannian coarse-graining.

The question is whether there really is an objective micro-macro distinction of this kind. At least within the context of classical mechanics this is not evidently the case. In quantum mechanics Planck’s constant gives a natural limit to how confined a state can be in both position and momentum, but classical mechanics by itself does not provide any such limit. So the burden of proof seems to be on the side of those who wish to uphold that there is an objective separation between micro and macro scales.

And this is not yet the end of the difficulties. Even if the above arguments were successful, they would remain silent about the questions surrounding the choice of the ‘right’ partition. Nothing in either the appeal to the limits of observation or the existence of an objective separation of scales explains why coarse-graining energy is ‘bad’ while coarse-graining position and momentum is ‘good’.

These problems are not easily overcome. In fact, they seem so serious that they lead Penrose to think that ‘entropy has the status of a “convenience”, in present day theory, rather than being “fundamental”’ (2006, 692) and that

⁴⁶Many authors have criticised approaches to SM that invoke limited knowledge as deficient. Since these criticisms have mainly been put forward against Gibbsian approaches to SM, I will come back to this point in more detail below.

⁴⁷Notice that this use of the terms ‘micro’ and ‘macro’ does not line up with how these terms have been used above, where both fine-grained and coarse-grained states were situated at the ‘micro’ level (see Subsection 2.1).

⁴⁸This point of view is often alluded to by physicists but rarely explained, let alone defended. It also seems to be what Goldstein has in mind when he advises us to ‘partition the 1-particle phase space (the q, p -space) into macroscopically small but microscopically large cells Δ_α ’ (2001, 42).

it only would acquire a ‘more fundamental status’ in the light of advances in quantum theory, in particular quantum gravity, as only quantum mechanics provides the means to compartmentalise phase space (*ibid.*).

In the light of these difficulties the safe strategy seems to be to renounce commitment to coarse-graining by downgrading it to the status of a mere expedient, which, though instrumentally useful, is ultimately superfluous. For this strategy to be successful the results of the theory would have to be robust in the limit $\delta\omega \rightarrow 0$.

But this is not the case. The terms on the right hand side of Equation (13) diverge in the limit $\delta\omega \rightarrow 0$. And this is not simply a ‘technical accident’ that one can get straight given enough mathematical ingenuity. On the contrary, the divergence of the Boltzmann entropy is indicative of the fact that the whole argument is intimately tied to there being finitely many cells which serve as the starting point for a combinatorial argument. Using combinatorics simply does not make sense when dealing with a continuum; so it is only natural that the argument breaks down in the continuum limit.

Let us now turn to the limitations of the formalism, which are intimately connected to the Boltzmannian conception of equilibrium. The equilibrium macro-state, by definition, is the one for which S_B is maximal. *Per se* this is just a definition and its physical relevance needs to be shown. This is done in two steps. First, we use the combinatorial argument to explicitly construct the macro-regions as those parts of the energy hypersurface that correspond to a certain distribution, and then show that the largest macro-region is the one that corresponds to the Maxwell-Boltzmann distribution. But why is this the equilibrium distribution of a physical system? This is so, and this is the second step, because (a) predictions made on the basis of this distribution bear out in experiments, and (b) Maxwell showed in 1860 that this distribution can be derived from symmetry considerations that are entirely independent of the use of a partition (see Uffink (2007, 943-948) for a discussion of Maxwell’s argument). This provides the sought-after justification of the proposed definition of equilibrium.

The problem is that this justification is based on the assumption that there is no interaction between the particles in the system and that therefore the total energy of the system is the sum of the ‘individual’ particle energies. While not a bad characterisation of the situation in dilute gases, this assumption is radically false when we consider systems with non-negligible interactions such as liquids, solids, or gravitating systems. Hence, the above justification for regarding the macro-state for which S_B is maximal as the

equilibrium state is restricted to dilute gases, and it is not clear whether the equilibrium macro-state can be defined in the same way in systems that are not of this kind.

There is a heuristic argument for the conclusion that this is problematic. Consider a system of gravitating particles. These particles attract each other and hence have the tendency to clump together. So if it happens that a large amount of these are distributed evenly over a bounded space, then they will move together and eventually form a lump. However, the phase volume corresponding to a lump is much smaller than the one corresponding to the original spread out state, and hence it has lower Boltzmann entropy.⁴⁹ So we have here a system that evolves from a high to a low entropy state. This problem is usually ‘solved’ by declaring that things are different in a gravitating system and that we should, in such cases, regard the spread out state as one of low entropy and the lump as one of high entropy. Whether or not this *ad hoc* move is convincing may well be a matter of contention. But even if it is, it is of no avail to the Boltzmannian. Even if one redefines entropy such that the lump has high and the spread out state low entropy, it is still a fact that the phase volume corresponding to the spread out state is substantially larger than the one corresponding to the lump, and Boltzmannian explanations of thermodynamic behaviour typically make essential use of the fact that the equilibrium macro-region is the largest of all macro regions.

Hence macro-states need to be defined differently in the context of interacting systems. Goldstein & Lebowitz (2004, 60-63) discuss the problem of defining macro-states for particles interacting with a two-body potential $\phi(q_i - q_j)$, where q_i and q_j are the position coordinates of two particles, and they develop a formalism for calculating the Boltzmann entropy for systems consisting of a large number of such particles. However, the formalism yields analytical results only for the special case of a system of hard balls. Numerical considerations also provide results for (two-dimensional) particles interacting with a cutoff Lennard-Jones potential, i.e. a potential that has the Lennard-Jones form for $|q_i - q_j| \leq r_c$ and is zero for all $|q_i - q_j| > r_c$, where r_c is a cutoff distance (Garrido, Goldstein & Lebowitz 2004, 2).

⁴⁹A possible reply to this is that the loss in volume in configuration space is compensated by an increase in volume in momentum space. Whether this argument is in general correct is an open question; there at least seem to be scenarios in which it is not, namely ones in which all particles end up moving around with almost the same velocity and hence only occupy a small volume of momentum space.

These results are interesting, but they do not yet provide the sought-after generalisation of the Boltzmann approach to more realistic systems. Hard ball systems are like ideal gases in that the interaction of the particles do not contribute to the energy of the system; the only difference between the two is that hard balls are extended while the ‘atoms’ of an ideal gas are point particles. Similarly, the cutoff Lennard-Jones potential also represents only a small departure from the idea of the ideal gas as the cutoff distance ensures that no long range interactions contribute to the energy of the system. However, typical realistic interactions such as gravity and electrostatic attraction/repulsion are long range interactions. Hence, it is still an open question whether the Boltzmann formalism can be extended to systems with realistic interactions.

2.8 Reductionism

Over the past decades, the issue of reductionism has attracted the attention of many philosophers and a vast body of literature on the topic has grown; Kim (1998) presents a brief survey; for a detailed discussion of the different positions see Hooker (1981) and Batterman (2002, 2003); Dupré (1993) expounds a radically sceptical perspective on reduction. This enthusiasm did not resonate with those writing on the foundations of SM and the philosophical debates over the nature (and even desirability) of reduction had rather little impact on work done on the foundations of SM (this is true for both the Boltzmannian and Gibbsian traditions). This is not the place to make up for this lack of interaction between two communities, but it should be pointed out that it might be beneficial to both those interested in reduction as well as those working on the foundations of SM to investigate whether, and if so how, philosophical accounts of reduction relate to SM and what consequences certain philosophical perspectives on reduction would have on how we think about the aims and problems of SM.

One can only speculate about what the reasons for this mutual disinterest are. A plausible explanation seems to be that reductionism has not been perceived as problematic by those working on SM and hence there did not seem to be a need to turn to the philosophical literature. A look at how reductionism is dealt with in the literature on SM confirms this suspicion: by and large there is agreement that the aim of SM is to derive, fully and rigorously, the laws of TD from the underlying micro theory. This has a familiar ring to it for those who know the philosophical debates over reductionism. In fact,

it is precisely what Nagel (1961, Ch. 11) declared to be the aim of reduction. So one can say that the Nagelian model of reduction is the (usually unquestioned and unacknowledged) ‘background philosophy’ of SM. This sets the agenda. I will first introduce Nagel’s account of reduction, discuss some of its problems, mention a possible ramification, and then examine how well the achievements of SM square with this conception of reduction. At the end I will mention some further issues in connection with reduction.

The core idea of Nagel’s theory of reduction is that a theory T_1 reduces a theory T_2 (or T_2 is reduced to T_1) only if the laws of T_2 are derivable from those of T_1 ; T_1 is then referred to as the ‘reducing theory’ and T_2 as the ‘reduced theory’. In the case of a so-called homogeneous reduction both theories contain the same descriptive terms and use them with (at least approximately) the same meaning. The derivation of Kepler’s laws of planetary motion and Galileo’s law of free fall from Newton’s mechanics are proposed as paradigm cases of reductions of this kind. Things get more involved in the case of so-called ‘heterogeneous’ reductions, when the two theories do not share the same descriptive vocabulary. The reduction of TD belongs to this category because both TD and SM contain concepts that do not form part of the other theory (e.g. temperature is a TD concept that does not appear in the core of SM, while trajectories and phase functions are foreign to TD), and others are used with very different meanings (entropy is defined in totally dissimilar ways in TD and in SM). In this case so-called ‘bridge laws’ need to be introduced, which connect the vocabulary of both theories. More specifically, Nagel requires that for every concept C of T_2 that does not appear in T_1 there be a bridge law connecting C to concepts of T_1 (this is the so-called ‘requirement of connectability’). The standard example of a bridge law is the equipartition relation $\langle E \rangle = 3/2k_B T$, connecting temperature T with the mean kinetic energy $\langle E \rangle$.

Bridge laws carry with them a host of interpretative problems. What status do they have? Are they linguistic conventions? Or are they factual statements? If so, of what sort? Are they statements of constant conjunction (correlation) or do they express nomic necessities or even identities? And depending on which option one chooses the question arises of how a bridge law is established. Is it a factual discovery? By which methods is it established? Moreover, in what sense has T_1 reduced T_2 if the reduction can only be carried out with the aid of bridge laws which, by definition, do not belong to T_1 ? Much of the philosophical discussions on Nagelian reduction has centred around these issues.

Another problem is that strict derivability often is too stringent a requirement because only approximate versions of the T_2 -laws can be obtained. For instance, it is not possible to derive strict universal laws from a statistical theory. To make room for a certain mismatch between the two theories, Schaffner (1976) introduced the idea that concepts of T_2 often need to be modified before they can be reduced to T_1 . More specifically, Schaffner holds that T_1 reduces T_2 only if there is a corrected version T_2^* of T_2 such that T_2^* is derivable from T_1 given that (1) the primitive terms of T_2^* are associated via bridge laws with various terms of T_1 , (2) T_2^* corrects T_2 in the sense that T_2^* makes more accurate predictions than T_2 and (3) T_2^* and T_2 are strongly analogous.

With this notion of reduction in place we can now ask whether Boltzmannian SM reduces TD in this sense. This problem is usually narrowed down to the question of whether the Second Law of TD can be deduced from SM. This is of course an important question, but it is by no means the only one; I come back to other issues below. From what has been said so far it is obvious that the Second Law cannot be derived from SM. The time reversal invariance of the dynamics and Poincaré recurrence imply that the Boltzmann entropy does not increase monotonically at all times. In fact, when an SM system has reached equilibrium it fluctuates away from equilibrium every now and then. Hence, a strict Nagelian reduction of TD to SM is not possible. However, following Schaffner, this is anyway too much to ask for; what we should look for is a corrected version TD^* of TD, which satisfies the above-mentioned conditions and which can be reduced to SM. Callender (2001, 542-45) argues that this is precisely what we should do because trying to derive the *exact* Second Law would amount to ‘taking thermodynamics too seriously’; in fact, what we need to derive from SM is an ‘analogue’ of the Second Law.⁵⁰ One such analogue is BL, although there may be other candidates.

The same move helps us to reduce thermodynamic irreversibility. Callender (1999, 359, 364-67) argues that it is a mistake to try to deduce strict irreversibility from SM. All we need is an explanation of how phenomena that are irreversible on an appropriate time scale emerge from SM, where what is appropriate is dictated by the conditions of observation. In other words,

⁵⁰The same problem crops up when reducing the notions of equilibrium (Callender 2001, 545-547) and the distinction between intensive and extensive TD variables (Yi 2003, 1031-2) to SM: a reduction can only take place if we first present a suitably revised version of TD.

what we need to recover from SM is the phenomena supporting TD, not a strict reading of the TD laws.

Given this, the suggestion is that S_B can plausibly be regarded as the SM counterpart of the entropy of TD*. This is a plausible suggestion, but it seems that more needs to be said by way of justification. Associating S_B with the entropy of TD* effectively amounts to introducing a bridge law that defines the TD* entropy in terms of the logarithm of the phase volume of macro regions. This brings back all the above questions about the nature of bridge laws. What justifies the association of TD* entropy with its SM counterpart? Of what kind is this association? The discussion of the relation between the two entropies is usually limited to pointing out that the values of the two coincide in relevant situations. This certainly is an important point, but it does not answer the deeper questions about the relationship between the two concepts.

Although the second law occupies centre stage in TD, it is not the only law that needs to be reduced; in particular, we need to account for how the First Law of TD reduces to SM. And in this context a further problem crops up (Sklar 1999, 194). To explain how systems of very different kinds can transfer energy to one another, we need to assume that these systems have temperatures. This, in turn, implies that temperature can be realised in radically different ways; in other words, temperature is multiply realisable. How can that be? How do the various ‘realisers’ of temperature relate to one another? What exactly makes them realisers of this concept and why can we give them a uniform treatment in the theory?⁵¹

Similar problems also appear when we reduce more ‘local’ laws and properties to SM. For instance, the relation between pressure, volume and temperature of an ideal gas is given by the equation $pV = nk_B T$, the so called ‘ideal gas law’. In order to derive this law we need to make associations, for instance between pressure and mechanical properties like mass and momentum transfer, that have the character of bridge laws. How are these justified? Sklar (1993, 349-350) points out how complex even this seemingly straightforward case is.

And then there are those TD concepts that SM apparently remains silent about. Most importantly the concept of a quasi-static transformation (or process), which lies at the heart of TD. The laws of TD only apply to equi-

⁵¹For a further discussion of temperature see Sklar (1993, 351-354), Uffink (1996, 383-386) and Yi (2003, 1032-1036).

librium situations and therefore changes in a system have to be effected in a way that never pushes the system out of equilibrium, i.e. by so-called quasi-static transformations (see Uffink (2001) for discussion of this concept). But what does it mean in SM to perform a quasi-static transformation on a system?⁵²

Furthermore, one of the alleged payoffs of a successful reduction is explanation, i.e. the reduction is supposed to explain the reduced theory. Does SM explain TD and if so in what sense? This question is clearly stated by Sklar (1993, 148-154; 2000, 740) Callender (1999, 372-373) and Hellman (1999, 210), but it still awaits an in-depth discussion.

3 The Gibbs Approach

At the beginning of the Gibbs approach stands a radical rupture with the Boltzmann programme. The object of study for the Boltzmannians is an individual system, consisting of a large but finite number of micro constituents. By contrast, within the Gibbs framework the object of study is a so-called *ensemble*, an uncountably infinite collection of independent systems that are all governed by the same Hamiltonian but distributed over different states. Gibbs introduces the concept as follows:

We may imagine a great number of systems of the same nature, but differing in the configurations and velocities which they have at a given instant, and differing not only infinitesimally, but it may be so as to embrace every conceivable combination of configuration and velocities. And here we may set the problem, not to follow a particular system through its succession of configurations, but to determine how the whole number of systems will be distributed among the various conceivable configurations and velocities at any required time, when the distribution has been given for some one time. (Gibbs 1902, v)

Ensembles are fictions, or ‘mental copies of the one system under consideration’ (Schrödinger 1952, 3); they do not interact with each other, each system has its own dynamics, and they are not located in space and time.

⁵²Thanks to Wolfgang Pietsch for drawing my attention to this point.

Ensembles should not be confused with collections of micro-objects such as the molecules of a gas. The ensemble corresponding to a gas made up of n molecules, say, consists of an infinite number of copies of the *entire* gas; the phase space of each system in the ensemble is the $6n$ -dimensional γ -space of the gas as a whole.

3.1 The Gibbs Formalism

Consider an ensemble of systems. The instantaneous state of one system of the ensemble is specified by one point in its γ -space, also referred to as the system's micro-state.⁵³ The state of the ensemble is therefore specified by an everywhere positive density function $\rho(q, p, t)$ on the system's γ -space.⁵⁴ The time evolution of the ensemble is then associated with changes in the density function in time.

Within the Gibbs formalism $\rho(q, p, t)$ is regarded as a probability density, reflecting the probability of finding the state of a system chosen at random from the entire ensemble in region $R \subseteq \Gamma$ ⁵⁵ at time t :

$$p_t(R) = \int_R \rho(q, p, t) d\Gamma \quad (23)$$

For this reason the distribution has to be normalised:

$$\int_{\Gamma} \rho(q, p, t) d\Gamma = 1. \quad (24)$$

Now consider a real valued function $f : \Gamma \times t \rightarrow R$. The phase average (some times also 'ensemble average') of this function is given by:

$$\bar{f}(t) = \int_{\Gamma} f(q, p, t) \rho(q, p, t) d\Gamma. \quad (25)$$

Phase averages occupy centre stage in the Gibbs formalism because it is these that, according to the formalism, we observe in experiments. More specif-

⁵³To be more precise, the system's fine-grained micro-state. However, within the Gibbs approach coarse-graining enters the stage only much later (in Subsection 3.5) and so the difference between coarse-grained and fine-grained micro-states need not be emphasised at this point.

⁵⁴That is, $\rho(q, p, t) \geq 0$ for all $(q, p) \in \Gamma_{\gamma}$ and all instants of time t .

⁵⁵The μ -space of a system does not play any rôle in the Gibbs formalism. For this reason I from now on drop the subscript ' γ ' and only write ' Γ ' instead of ' Γ_{γ} ' when referring to a system's γ -space.

ically, the Gibbs formalism postulates that to every experimentally observable quantity $F(t)$ (with the exception of absolute temperature and entropy) there corresponds a phase function $f(q, p, t)$ such that Equation (25) yields the value that we should expect to find in an experiment: $F(t) = \bar{f}(t)$.

Using the principles of Hamiltonian mechanics one can then prove that the total derivative of the density function equals zero,

$$\frac{d\rho}{dt} = 0, \quad (26)$$

which is commonly referred to as ‘Liouville’s theorem’ in this context. Intuitively, this theorem says that ρ moves in phase space like an incompressible fluid. With Equation (44) in the Appendix it follows that the time evolution of ρ is given by Liouville’s equation:

$$\frac{\partial\rho}{\partial t} = -\{\rho, H\}, \quad (27)$$

where $\{\cdot, \cdot\}$ is the Poisson bracket and H the Hamiltonian governing the system’s dynamics. By definition, a distribution is stationary iff $\partial\rho/\partial t = 0$ for all t .

Given that observable quantities are associated with phase averages and that equilibrium is defined in terms of the constancy of the macroscopic parameters characterising the system, it is natural to regard the stationarity of the distribution as defining equilibrium because a stationary distribution yields constant averages.⁵⁶ For this reason Gibbs refers to stationarity as the ‘condition of statistical equilibrium’.

Among all stationary distributions⁵⁷ those satisfying a further requirement, the Gibbsian maximum entropy principle, play a special rôle. The fine-grained Gibbs entropy (sometimes also ‘ensemble entropy’) is defined as

$$S_G(\rho) := -k_B \int_{\Gamma} \rho \log(\rho) d\Gamma. \quad (28)$$

The Gibbsian maximum entropy principle then requires that $S_G(\rho)$ be maximal, given the constraints that are imposed on the system.

⁵⁶Provided that the observable f itself is not explicitly time dependent, in which case one would not require equilibrium expectation values to be constant.

⁵⁷As Gibbs notes, every distribution that can be written as a function of the Hamiltonian is stationary.

The last clause is essential because different constraints single out different distributions. A common choice is to keep both the energy and the particle number in the system fixed: $E=\text{const}$ and $n=\text{const}$ (while also assuming that the spatial extension of the system is finite). One can prove that under these circumstances $S_G(\rho)$ is maximal for what is called the ‘microcanonical distribution’ (or ‘microcanonical ensemble’), the distribution which is uniform on the energy hypersurface $H(q, p) = E$ and zero elsewhere:

$$\rho(q, p) = C \delta[E - H(q, p)], \quad (29)$$

where C is some suitable normalisation constant and δ is Dirac’s delta function.⁵⁸

If we choose to hold the number of particles constant while allowing for energy fluctuations around a given mean value we obtain the so-called canonical distribution; if we also allow the particle number to fluctuate around a given mean value we find the so-called grand-canonical distribution (for details see, for instance, Tolman 1938, Chs. 3 and 4).

Consider now a system out of equilibrium, characterised by a density $\rho(t)$. This density is not stationary and its entropy is not maximal. Given the laws of TD we would expect this density to approach the equilibrium density as time evolves (e.g. in the case of a system with constant energy and constant particle number we would expect $\rho(t)$ to approach the microcanonical distribution), which would also be reflected in an increase in entropy.

This expectation is frustrated. Using Liouville’s equation one can prove that $\rho(t)$ does *not* approach the microcanonical distribution (I will come back to this point below); and what seems worse, the entropy does not increase at all. In fact, it is straightforward to see that S_G is a constant of the motion (Zeh 2001, 48-9); that is, $dS_G(\rho(t))/dt = 0$, and hence $S_G(\rho(t)) = S_G(\rho(0))$ for all times t .

This precludes an explanation of the approach to equilibrium along the lines explored so far. Hence the main challenge for Gibbsian non-equilibrium theory is either to get S_G moving somehow, or to explain why this is not necessary after all.

⁵⁸This distribution is sometimes referred to as the ‘super microcanonical distribution’ while the term ‘microcanonical distribution’ is used to refer to a slightly different distribution, namely one that is constant on a thin but finite ‘sheet’ around the accessible parts of the energy hypersurface and zero elsewhere. It turns out that the latter distribution is mathematically more manageable.

3.2 Problems and Tasks

In this subsection I list the issues that need to be addressed in the Gibbs programme and make some remarks about how they differ from the problems that arise in the Boltzmann framework. Again, these issues are not independent of each other and the response to one bears on the responses to the others.

3.2.1 Issue 1: Ensembles and Systems

The most obvious problem concerns the use of ensembles. The probability distribution in the Gibbs approach is defined over an ensemble, the formalism provides ensemble averages, and equilibrium is regarded as a property of an ensemble. But what we are really interested in is the behaviour of a single system. What can the properties of an ensemble, a fictional entity consisting of infinitely many copies of a system, tell us about the one real system that we investigate? And how are we to reconcile the fact that the Gibbs formalism treats equilibrium as a property of an ensemble with physical common sense and thermodynamics, both of which regard an individual system as the bearer of this property?

These difficulties raise the question of whether the commitment to ensembles could be renounced. Are ensembles really an irreducible part of the Gibbsian scheme or are they just an expedient, or even a pedagogical ploy, of no fundamental significance? If so, how can the theory be reformulated without appeal to ensembles?

These questions are of fundamental significance, not least because it is the use of ensembles that frees the Gibbs approach from some of the most pressing problems of the Boltzmann approach, namely the reversal and the recurrence objections. These arise exactly because we are focussing on what happens in an individual system; in an ensemble recurrence and reverse behaviour are no problem because it can be accepted that some systems in the ensemble will behave non-thermodynamically, provided that their contribution to the properties of the ensemble as a whole is taken into account when calculating ensemble averages. So some systems behaving strangely is no objection as this does not imply that the ensemble as a whole behaves in a strange way too.

3.2.2 Issue 2: The Connection with Dynamics and the Interpretation of Probability

The microcanonical distribution has been derived from the Gibbsian maximum entropy principle and the requirement that the equilibrium distribution be stationary. Neither of these requirements make reference to the dynamics of the system. However, as in the case of the combinatorial argument, it seems odd that equilibrium conditions can be specified without any appeal to the dynamics of the systems involved. That equilibrium can be characterised by a microcanonical distribution must, or so it seems, have something to do with facts about the system in question. Understanding the connection between the properties of a system and the Gibbsian probability distribution is complicated by the fact that the distribution is one pertaining to an ensemble rather than an individual system. What, if anything, in the dynamics gives rise to, or justifies, the use of the microcanonical distribution? And if there is no such justification, what is the reason for this?

Closely related to the question of how the probability distribution relates to the system's dynamics is the problem of interpreting these probabilities. The options are the same as in Unit 2.3.2 and need not be repeated here. What is worth emphasising is that, as we shall see, different interpretations of probability lead to very different justifications of the maximum entropy requirement and its connection to the dynamics of the system; in fact, in non-equilibrium theory they lead to very different formalisms. Thus, this is a case where philosophical commitments shape scientific research programmes.

3.2.3 Issue 3: Why does Gibbs phase averaging work?

The Gibbs formalism posits that what we observe in actual experiments are phase averages. Practically speaking this method works just fine. But why does it work? Why do averages over an ensemble coincide with the values found in measurements performed on an actual physical system in equilibrium? There is no obvious connection between the two and if Gibbsian phase averaging is to be more than a black-box technique then we have to explain what the connection between phase averages and measurement values is.

3.2.4 Issue 4: The Approach to Equilibrium

Phase averaging only applies to equilibrium systems and even if we have a satisfactory explanation of why this procedure works, we are still left with the question of why and how the system reaches equilibrium at all if, as it often happens, it starts off far from equilibrium.

Gibbsian non-equilibrium theory faces two serious problems. The first is that, as we have seen above, the Gibbs entropy is constant. This precludes a characterisation of the approach to equilibrium in terms of increasing Gibbs entropy. Hence, either such a characterisation has to be given up (at the cost of being fundamentally at odds with thermodynamics), or the formalism has to be modified in a way that makes room for entropy increase.

The second problem is the characterisation of equilibrium in terms of a stationary distribution. The Hamiltonian equations of motion, which govern the system, preclude an evolution from a non-stationary to a stationary distribution: if, at some point in time, the distribution is non-stationary, then it will remain non-stationary for all times and, conversely, if it is stationary at some time, then it must have been stationary all along (van Lith 2001a, 591-2). Hence, if a system is governed by Hamilton's equation, then a characterisation of equilibrium in terms of stationary distributions contradicts the fact that an approach to equilibrium takes place in systems that are not initially in equilibrium.

Clearly, this is a *reductio* of a characterisation of equilibrium in terms of stationary distributions. The reasoning that led to this characterisation was that an equilibrium state is one that remains unchanged through time, which, at the mechanical level, amounts to postulating an unchanging, i.e. stationary, distribution. This was too quick. Thermodynamic equilibrium is defined as a state in which all macro-parameters describing the system are constant. So all that is needed for equilibrium is that the distribution be such that mean values of the functions associated with thermodynamic quantities are constant in time (Sklar 1978, 191). This is a much weaker requirement because it can be met by distributions that are not stationary. Hence we have to come to a more 'liberal' characterisation of equilibrium; the question is what this characterisation is.⁵⁹

⁵⁹Leeds (1989, 328-30) also challenges as too strong the assumption that a physical system in an equilibrium state has a precise probability distribution associated with it. Although this may well be true, this seems to be just another instance of the time-honoured problem of how a precise mathematical description is matched up with a piece of physical

3.2.5 Issue 5: Reductionism

Both the Boltzmannian and the Gibbsian approach to SM eventually aim to account for the TD behaviour of the systems under investigation. Hence the questions for the Gibbs approach are exactly the same as the ones mentioned in Unit 2.3.7, and the starting point will also be Nagel's model of reduction (introduced in Subsection 2.8).

3.2.6 Plan

As mentioned above, the methods devised to justify the use of the micro-canonical distribution and the legitimacy of phase averaging, as well as attempts to formulate a coherent non-equilibrium theory are radically different depending on whether probabilities are understood ontically or epistemically. For this reason it is best to discuss these two families of approaches separately. Subsection 3.3 presents arguments justifying Gibbs phase averaging on the basis of an ontic understanding of the probabilities involved. What this understanding might be is discussed in Subsection 3.4. I turn to this question only after a discussion of different justifications of phase averaging because although an ontic understanding of probabilities is clearly assumed, most writers in this tradition do not discuss this assumption explicitly and one can only speculate about what interpretation of probability they might endorse. Subsection 3.5 is concerned with different approaches to non-equilibrium that are based on this interpretation of probabilities. In Subsection 3.6 I discuss the epistemic approach to the Gibbs formalism. I close this Section with a discussion of reductionism in the Gibbs approach (Subsection 3.7).

3.3 Why Does Gibbs Phase Averaging Work?

Why do phase averages coincide with values measured in actual physical systems? There are two families of answers to this question, one based on ergodic theory (using ideas we have seen in the Boltzmann Section), the other building on the notion of a thermodynamic limit. For reasons of space we will treat this second approach much more briefly.

reality that is not intrinsically mathematical. This issue is beyond the scope of this review.

3.3.1 Time Averages and Ergodicity

Common wisdom justifies the use of phase averages as follows.⁶⁰ The Gibbs formalism associates physical quantities with functions on the system's phase space. Making an experiment to measure one of these quantities takes some time. So what measurement devices register is not the instantaneous value of the function in question, but rather its time average over the duration of the measurement; hence it is time averages that are empirically accessible. Then, so the argument continues, although measurements take an amount of time that is short by human standards, it is long compared to microscopic time scales on which typical molecular processes take place (sometimes also referred to as 'microscopic relaxation time'). For this reason the actually measured value is approximately equal to the *infinite* time average of the measured function. This by itself is not yet a solution to the initial problem because the Gibbs formalism does not provide us with time averages and calculating these would require an integration of the equations of motion, which is unfeasible. This difficulty can be circumvented by assuming that the system is ergodic. In this case time averages equal phase averages, and the latter can easily be obtained from the formalism. Hence we have found the sought-after connection: the Gibbs formalism provides phase averages which, by ergodicity, are equal to infinite time averages, and these are, to a good approximation, equal to the finite time averages obtained from measurements.

This argument is problematic for at least two reasons (Malament & Zabell 1980, 342-343; Sklar 1973, 211). First, from the fact that measurements take some time it does not follow that what is actually measured are time averages. Why do measurements produce time averages and in what way does this depend on how much time measurements take?

Second, even if we take it for granted that measurements do produce finite time averages, then equating these with infinite time averages is problematic. Even if the duration of the measurement is very long (which is often not the case as actual measurement may not that take much time), finite and infinite averages may assume very different values. And the infinity is crucial: if we replace infinite time averages by finite ones (no matter how long the relevant period is taken to be), then the ergodic theorem does not hold any more and

⁶⁰This view is discussed but not endorsed, for instance, in Malament & Zabell (1980, 342), Bricmont (1996, 145-6), Earman & Redei (1996, 67-69), and van Lith (2001a, 581-583).

the explanation is false.

Besides, there is another problem once we try to apply the Gibbs formalism to non-equilibrium situations. It is a simple fact that we do observe how systems approach equilibrium, i.e. how macroscopic parameter values change, and this would be impossible if the values we observed were infinite time averages.

These criticisms seem decisive and call for a different strategy in addressing Issue 3. Malament & Zabell (1980) respond to this challenge by suggesting a new way of explaining the success of equilibrium theory, at least for the microcanonical ensemble. Their method still invokes ergodicity but avoids altogether appeal to time averages and only invokes the uniqueness of the measure (see Subection 2.4). Their explanation is based on two Assumptions (*ibid.*, 343).

Assumption 1. The phase function f associated with a macroscopic parameter of the system exhibits small dispersion with respect to the microcanonical measure; that is, the set of points on the energy hypersurface Γ_E at which f assumes values that differ significantly from its phase average has vanishingly small microcanonical measure. Formally, for any ‘reasonably small’ $\varepsilon > 0$ we have

$$\lambda\left(\left\{x \in \Gamma_E : \left|f(x) - \int_{\Gamma_E} f d\lambda\right| \geq \varepsilon\right\}\right) \approx 0, \quad (30)$$

where λ is the microcanonical measure (i.e. the measure that is constant on the accessible part of the energy hypersurface and normalised).

Assumption 2. At any given time, the microcanonical measure represents the probability of finding the system in a particular subset of the phase space: $p(A) = \lambda(A)$, where A is a measurable but otherwise arbitrary subset of Γ_E . These two assumptions jointly imply that, at any given time, it is overwhelmingly likely that the system’s micro-state is one for which the value of f coincides with, or is very close to, the phase average.

The question is how these assumptions can be justified. In the case of Assumption 1 Malament & Zabell refer to a research programme that originated with the work of Khinchin. The central insight of this programme is that phase functions which are associated with macroscopic parameters satisfy strong symmetry requirements and as a consequence turn out to have small dispersion on the energy surface for systems with a large number of constituents. This is just what is needed to justify Assumption 1. This programme will be discussed in the next subsection; let us assume for now that

it provides a satisfactory justification of Assumption 1.

To justify Assumption 2 Malament & Zabell introduce a new postulate: the equilibrium probability measure $p(\cdot)$ of finding a system's state in a particular subset of Γ_E must be absolutely continuous with respect to the microcanonical measure λ (see Subsection 2.4). Let us refer to this as the 'Absolute Continuity Postulate' (ACP).⁶¹

Now consider the dynamical system (X, ϕ, λ) , where X is Γ_E , ϕ is the flow on Γ_E induced by the equations of motion governing the system, and λ is the microcanonical measure on Γ_E . Given this, one can present the following argument in support of Assumption 2 (*ibid.* 345):

(P1) (X, ϕ, λ) is ergodic.

(P2) $p(\cdot)$ is invariant in time because this is the defining feature of equilibrium probabilities.

(P3) by ACP, $p(\cdot)$ is absolutely continuous with λ .

(P4) According to the uniqueness theorem (see Subsection 2.4.1), λ is the only measure invariant in time.

Conclusion: $p(\cdot) = \lambda$.

Hence the microcanonical measure is singled out as the one and only correct measure for the probability of finding a system's micro-state in a certain part of phase space.

The new and crucial assumption is ACP and the question is how this principle can be justified. What reason is there to restrict the class of measures that we take into consideration as acceptable equilibrium measures to those that are absolutely continuous with respect to the microcanonical measure? Malament & Zabell respond to this problem by introducing yet another principle, the 'displacement principle' (DP). This principle posits that if of two measurable sets in Γ_E one is but a small displacement of the other, then it is plausible to believe that the probability of finding the system's micro-state in one set should be close to that of finding it in the other (*ibid.*, 346).

⁶¹I formulate ACP in terms of λ because this simplifies the argument to follow. Malament & Zabell require that $p(\cdot)$ be absolutely continuous with μ_E , the Lebesgue measure μ on Γ restricted to Γ_E . However, on Γ_E the restricted Lebesgue measure and the microcanonical measure only differ by a constant: $\lambda = c\mu_E$, where $c := 1/\mu_E(\Gamma_E)$ and hence whenever a measure is absolutely continuous with μ_E it is also with λ and vice versa.

This principle is interesting because one can show that it is equivalent to the claim that probability distributions are absolutely continuous with respect to the Lebesgue measure, and hence the microcanonical measure, on Γ_E (*ibid.*, 348-9).⁶²

To sum up, the advantages of this method over the ‘standard account’ are that it does not appeal to measurement, that it takes into account that SM systems are ‘large’ (via Assumption 1), and that it does not make reference to time averages at all. In fact, ergodicity is used only to justify the uniqueness of the microcanonical measure.

The remaining question is what reasons there are to believe in DP. Malament & Zabell offer little by way of justification: they just make some elusive appeal to the ‘method by which the system is prepared or brought to equilibrium’ (*ibid.*, 347.) So it is not clear how one gets from some notion of state preparation to DP. But even if it was clear, why should the success of equilibrium SM depend on the system being *prepared* in a particular way? This seems to add an anthropocentric element to SM which, at least if one is not a proponent of the ontic approach (referred to in Subsection 2.3.2), seems to be foreign to it.

The argument in support of Assumption 2 makes two further problematic assumptions. First, it assumes equilibrium to be defined in terms of a stationary distribution, which, as we have seen above, is problematic because it undercuts a dynamical explanation of the approach to equilibrium (variants of this criticism can be found in Sklar (1978) and Leeds (1989)).

Second, it is based on the premise that the system in question is ergodic. As we have seen above, many systems that are successfully dealt with by the formalism of SM are not ergodic and hence the uniqueness theorem, on which the argument in support of Assumption 2 is based, does not apply.

To circumvent this difficulty Vranas (1998) has suggested replacing ergodicity with what he calls ε -ergodicity. The leading idea behind this move is to challenge the commonly held belief that even if a system is just a ‘little bit’ non-ergodic, then the uniqueness theorem fails completely (Earman & Redei 1996, 71). Vranas points out that there is a middle ground between holding and failing completely and then argues that this middle ground actually provides us with everything we need.

⁶²As Leeds (1989, 327) points out, Malament & Zabell’s proof is for R^n and they do not indicate how the proof could be modified to apply to the energy hypersurface, where translations can take one off the surface.

Two measures λ_1 and λ_2 are ε -close iff for every measurable set A : $|\lambda_1(A) - \lambda_2(A)| \leq \varepsilon$, where ε is a small but finite number. The starting point of Vranas' argument is the observation that we do not need $p(\cdot) = \lambda$: to justify Gibbsian phase averaging along the lines suggested by Malament & Zabell all we need is that $p(\cdot)$ and λ are ε -close, as we cannot tell the difference between a probability measure that is exactly equal to λ and one that is just a little bit different from it. Hence we should replace Assumption 2 by Assumption 2', that statement that $p(\cdot)$ and λ are ε -close. The question now is: how do we justify Assumption 2'?

If a system is non-ergodic then its phase space X is decomposable; that is, there exist two sets, $A \subseteq X$ and $B := X \setminus A$, with measure greater than zero which are invariant under the flow. Intuitively, if the system is 'just a little bit non-ergodic', then the system is ergodic on B and $\lambda(A) \ll \lambda(B)$ (where, again, λ is the microcanonical measure). This motivates the following definition: A dynamical system (X, λ, ϕ) is ε -ergodic iff the system's dynamics is ergodic on a subset Y of X with $\lambda(Y) = 1 - \varepsilon$.⁶³ Strict ergodicity then is the limiting case of $\varepsilon = 0$. Furthermore, given two small but finite numbers ε_1 and ε_2 , Vranas defines λ_2 to be ' $\varepsilon_1/\varepsilon_2$ -continuous' with λ_1 iff for every measurable set A : $\lambda_2(A) \leq \varepsilon_2$ if $\lambda_1(A) \leq \varepsilon_1$ (Vranas 1998, 695).

Vranas then proves an ' ε -version' of the uniqueness theorem, the ε -equivalence theorem (*ibid.*, 703-5): if λ_1 is ε_1 -ergodic and λ_2 is $\varepsilon_1/\varepsilon_2$ -continuous with respect to λ_1 and invariant, then λ_1 and λ_2 are ε_3 -close with $\varepsilon_3 = 2\varepsilon_2 + \varepsilon_1(1 - \varepsilon_1)^{-1}$.

Given this, the Malament & Zabell argument can be rephrased as follows:

(P1') (X, ϕ, λ) is ε -ergodic.

(P2) $p(\cdot)$ is invariant in time because this is the defining feature of equilibrium probabilities.

(P3') ε -ACP: $p(\cdot)$ is $\varepsilon/\varepsilon_2$ -continuous with respect to λ .

(P4') The ε -equivalence theorem.

Conclusion': $p(\cdot)$ and λ are ε_3 -close with $\varepsilon_3 = 2\varepsilon_2 + \varepsilon(1 - \varepsilon)^{-1}$.

⁶³Vranas (1998, 695) distinguishes between ' ε -ergodic' and 'epsilon-ergodic', where a system is epsilon-ergodic if it is ε -ergodic with ε tiny or zero. In what follows I always assume ε to be tiny and hence do not distinguish between the two.

The assessment of this argument depends on what can be said in favour of (P1') and (P3'), since (P4') is a mathematical theorem and (P2) has not been altered. In support of (P1') Vranas (*ibid.*, 695-98) reviews computational evidence showing that systems of interest are indeed ε -ergodic. In particular, he mentions the following cases. A one-dimensional system of n self-gravitating plane parallel sheets of uniform density was found to be strictly ergodic as n increases (it reaches strict ergodicity for $n = 11$). The Fermi-Pasta-Ulam system (a one dimensional chain of n particles with weakly nonlinear nearest-neighbor interaction) is ε -ergodic for large n . There is good evidence that a Lennard-Jones gas is ε -ergodic for large n and in the relevant energy range, i.e. for energies large enough so that quantum effects do not matter. From these Vranas draws the tentative conclusion that the dynamical systems of interest in SM are indeed ε -ergodic. But he is clear about the fact that this is only a tentative conclusion and that it would be desirable to have theoretical results.

The justification of (P3') is more difficult. This does not come as a surprise because Malament & Zabell did not present a justification for ACP either. Vranas (*ibid.*, 700-2) presents some arguments based on the limited precision of measurement but admits that this argument invokes premises that he cannot justify.

To sum up, this argument enjoys the advantage over previous arguments that it does not have to invoke strict ergodicity. However, it is still based on the assumption that equilibrium is characterised by a stationary distribution, which, as we have seen, is an obstacle when it comes to formulating a workable Gibbsian non-equilibrium theory. In sum, it is still an open question whether the ergodic programme can eventually explain in a satisfactory way why Gibbsian SM works.

3.3.2 Khinchin's Programme and the Thermodynamic Limit

Ergodic theory works at a general level in that it makes no assumptions about the number of degrees of freedom of the system under study and does not restrict the allowable phase functions beyond the requirement that they be integrable. Khinchin (1949) points out that this generality is not only unnecessary; actually it is the source of the problems that this programme encounters. Rather than studying dynamical systems at a general level, we should focus on those cases that are relevant in statistical mechanics. This involves two restrictions. First, we only have to consider systems with a large

number of degrees of freedom; second, we only need to take into account a special class of phase function, so-called sum functions. A function is a sum function if it can be written as a sum over one-particle functions:

$$f(x) = \sum_{i=1}^n f_i(x_i), \quad (31)$$

where x_i is the vector containing the position and momentum coordinates of particle i (that is, $x_i \in \mathbf{R}^6$ while $x \in \mathbf{R}^{6n}$) Under the assumption that the Hamiltonian of the system is a sum function as well, Khinchin can prove the following theorem:

Khinchin's Theorem. For all sum functions f there are positive constants k_1 and k_2 such that

$$\lambda\left(\left\{x \in \Gamma_E : \left|\frac{f^*(x) - \bar{f}}{\bar{f}}\right| \geq k_1 n^{-1/4}\right\}\right) \leq k_2 n^{-1/4}, \quad (32)$$

where λ is the microcanonical measure.

This theorem is sometimes also referred to as ‘Khinchin’s ergodic theorem’; let us say that a system satisfying the condition specified in Khinchin’s theorem is ‘K-ergodic’.⁶⁴ For a summary and a discussion of the proof see Batterman (1998, 190-198), van Lith (2001b, 83-90) and Badino (2006). Basically the theorem says that as n becomes larger, the measure of those regions on the energy hypersurface where the time and the space means differ by more than a small amount tends towards zero. For any finite n , K-ergodicity is weaker than ergodicity in the sense that the region where time and phase average do not coincide can have a finite measure, while it is of measure zero if the system is ergodic; this discrepancy vanishes for $n \rightarrow \infty$. However, even in the limit for $n \rightarrow \infty$ there is an important difference between ergodicity and K-ergodicity: if K-ergodicity holds, it only holds for a very special class of phase functions, namely sum-functions; ergodicity, by contrast, holds for any λ -integrable function.

A number of problems facing an explanation of equilibrium SM based on K-ergodicity need to be mentioned. First, like the afore-mentioned approaches based on ergodicity, Khinchin’s programme associates the outcomes

⁶⁴K-ergodicity should not be conflated with the property of being a K-system; i.e. being a system having the Kolmogorov property.

of measurements with infinite time averages and is therefore vulnerable to the same objections. Second, ergodicity's measure zero problem turns into a 'measure $k_2 n^{-1/4}$ problem', which is worse because now we have to justify that a part of the energy hypersurface of *finite* measure (rather than measure zero) can be disregarded. Third, the main motivation for focussing attention on sum-functions is the claim that all relevant functions, i.e. the ones that correspond to thermodynamic quantities, are of that kind. Batterman (1998, 191) points out that this is too narrow as there are functions of interest that do not have this form.

A further serious difficulty is what Khinchin himself called the 'methodological paradox' (Khinchin 1949, 41-43). The proof of the above theorem assumes the Hamiltonian to be a sum function (and this assumption plays a crucial rôle in the derivation of the theorem). However, for an equilibrium state to arise to begin with, the particles have to interact (collide), which cannot happen if the Hamiltonian is a sum function. Khinchin's response is to assume that there are only short range interactions between the molecules (which is the case, for instance, in a hard ball gas). If this is the case, Khinchin argues, the interactions are effective only on a tiny part of the phase space and hence have no significant effect on averages.

This response has struck many as unsatisfactory and ad hoc, and so the methodological paradox became the starting point for a research programme now known as the 'thermodynamic limit', investigating the question of whether one can still prove 'Khinchin-like' results in the case of Hamiltonians *with* interaction terms. Results of this kind can be proven in the limit for $n \rightarrow \infty$, if also the volume V of the system tends towards infinity in such a way that the numberdensity n/V remains constant. This programme, championed among others by Lanford, Mazur, Ruelle, and van der Linden, has reached a tremendous degree of mathematical sophistication and defies summary in simple terms. Classic statements are Ruelle (1969, 2004); surveys and further references can be found in Compagner (1989), van Lith (2001b, 93-101) and Uffink (2007, 1020-28).

A further problem is that for finite n K-ergodic systems need not be metrically transitive. This calls into question the ability of an approach based on K-ergodicity to provide an answer to the question of why measured values coincide with microcanonical averages. Suppose there is some global constant of motion other than H , and as a result the motion of the system remains confined to some part of the energy hypersurface. In this case there is no reason to assume that microcanonical averages with respect to the *entire*

energy hypersurface coincide with measured values. Faced with this problem one could argue that each system of that kind has a decomposition of its energy hypersurface into different regions of non-zero measure, some ergodic and others not, and that, as n and V get large, the average values of relevant phase functions get insensitive towards the non-ergodic parts.

Earman & Rédei (1996, 72) argue against this strategy on the grounds that it is straightforward to construct an infinity of normalised invariant measures that assign different weights to these regions than does the microcanonical measure. However, phase averages with respect to these other measures can deviate substantially from microcanonical averages, and it is to be expected that these predictions turn out wrong. But why? In a non-ergodic system there is no reason to grant the microcanonical measure a special status and Khinchin's approach does not provide a reason to expect microcanonical averages rather than any other average value to correspond to measurable quantities.

Batterman (1998) grants this point but argues that there is another reason to expect correspondence with observed values; but this reason comes from a careful analysis of renormalisation group techniques and their application to the case at hand, rather than any feature of either Khinchin's approach or the thermodynamic limit. A discussion of these techniques is beyond the scope of this review; the details of the case at hand are considered in Batterman (1998), and a general discussion of renormalisation and its relation to issues in connection with reductionism and explanation can be found in Batterman (2002).

3.4 Ontic Probabilities in Gibbs' Theory

Two ontic interpretations of Gibbsian probabilities have been suggested in the literature: frequentism and time averages. Let us discuss them in turn.

3.4.1 Frequentism

A common way of looking at ensembles is to think about them in analogy with urns, but rather than containing balls of different colours they contain systems in different micro-states. This way of thinking about ρ was first suggested in a notorious remark by Gibbs (1902, 163), in which he observes that '[w]hat we know about a body can generally be described most accurately and most simply by saying that it is one taken at random from a great num-

ber (ensemble) of bodies which are completely described'. Although Gibbs himself remained non-committal as regards an interpretation of probability, this point of view naturally lends itself to a frequentist analysis of probabilities. In this vein Malament & Zabell (1980, 345) observe that one can regard Gibbsian probabilities as representing limiting relative frequencies within an infinite ensemble of identical systems.

First appearances notwithstanding, this is problematic. The strength of frequentism is that it grounds probabilities in facts about the world. There are some legitimate questions for those who associate probabilities with infinite limiting frequencies, as these are not experimental facts. However, frequentists of any stripe agree that one single outcome is not enough to ground a probability claim. But this is the best we can ever get in Gibbsian SM. The ensemble is a fictitious entity; what is real is only the one system in the laboratory and so we can make at most one draw from this ensemble. All the other draws would be hypothetical. But on what grounds do we decide what the result of these draws would be? It is obvious that these hypothetical draws do not provide a basis for a frequentist interpretation of probabilities.

Another way of trying to ground a frequency interpretation is to understand frequencies as given by consecutive measurements made on the actual system. This move successfully avoids the appeal to hypothetical draws. Unfortunately this comes at the price of another serious problem. Von Mises' theory requires that successive trials whose outcomes make up the sequence on which the relative frequencies are defined (the collective) be independent. This, as von Mises himself pointed out, is generally not the case if the sequence is generated by one and the same system.⁶⁵ So making successive measurements on the same system does not give us the kind of sequences needed to define frequentist probabilities.

3.4.2 Time Averages

Another interpretation regards Gibbsian probabilities as time averages of the same kind as the ones we discussed in Subsection 2.4. On this view, $p_t(R)$ in Equation (23) is the average time that the system spends in region R . As

⁶⁵Von Mises discussed this problem in connection with diffusion processes and suggested getting around this difficulty by reconstructing the sequence in question, which is not a collective, as a combination of two sequences that are collectives (von Mises 1939, Ch. 6). Whether this is a viable solution in the context at hand is an open question.

in the case of Boltzmannian probabilities, this is in need of qualification as a relevant interval over which the time average is taken has to be specified and the dependence on initial conditions has to vanish. If, again, we assume that the system is ergodic on the energy hypersurface we obtain neat answers to these questions (just as in the Boltzmann case).

Assuming the system to be ergodic solves two problems at once. For one, it puts the time average interpretation on solid grounds (for the reasons discussed in Subsection 2.4.2 in the context of the Boltzmannian approach). For another, it offers an explanation of why the microcanonical distribution is indeed the right distribution; i.e. it solves the uniqueness problem. This is important because even if all interpretative issues were settled, we would still be left with the question of which among the infinitely many possible distributions would be the correct one to work with. The uniqueness theorem of ergodic theory answers this question in an elegant way by stating that the microcanonical distribution is the only distribution absolutely continuous with respect to the Lebesgue measure (although some argument still would have to be provided to establish that every acceptable distribution has to be absolutely continuous with respect to the Lebesgue measure).

However, this proposal suffers from all the difficulties mentioned in Subsection 2.4.3, which, as we saw, are not easily overcome. A further problem is that it undercuts an extension of the approach to non-equilibrium situations. Interpreting probabilities as infinite time averages yields stationary probabilities. As a result, phase averages are constant. This is what we expect in equilibrium, but it is at odds with the fact that we witness change and observe systems approaching equilibrium departing from a non-equilibrium state. This evolution has to be reflected in a change of the probability distribution, which is impossible if it is stationary by definition. Hence the time average interpretation of probability together with the assumption that the system is ergodic make it impossible to account for non-equilibrium behaviour (Sklar 1973, 211; Jaynes 1983, 106; Dougherty 1993, 846; van Lith 2001a, 586).

One could try to circumvent this problem by giving up the assumption that the system is ergodic and define $p_t(R)$ as a finite time average. However, the problem with this suggestion is that it is not clear what the relevant time interval should be, and the dependence of the time average on the initial condition would persist. These problems make this suggestion rather unattractive. Another suggestion is to be a pluralist about the interpretation of probability and hold that probabilities in equilibrium have to

be interpreted differently than probabilities in non-equilibrium. Whatever support one might muster for pluralism about the interpretation of probability in other contexts, it seems out of place when the equilibrium versus non-equilibrium distinction is at stake. At least in this case one needs an interpretation that applies to both cases alike (van Lith 2001a, 588).

3.5 The Approach to Equilibrium

The main challenge for Gibbsian non-equilibrium theory is to find a way to get the Gibbs entropy moving. Before discussing different solutions to this problem, let me again illustrate what the problem is. Consider the by now familiar gas that is confined to the left half of a container (V_{left}). Then remove the separating wall. As a result the gas will spread and soon evenly fill the entire volume (V_{total}). From a Gibbsian point of view, what *seems* to happen is that the equilibrium distribution with respect to the left half evolves into the equilibrium distribution with respect to the entire container; more specifically, what *seems* to happen is that the microcanonical distribution over all micro-states compatible with the gas being in V_{left} , Γ_{left} , evolves into the microcanonical distribution over all states compatible with the gas being in V_{total} , Γ_{total} . The problem is, that this development is ruled out by the laws of mechanics for an isolated system. The time evolution of an ensemble density is subject to Liouville's equation (27), according to which the density moves in phase space like an incompressible liquid, and therefore it is not possible that a density that was uniform over Γ_{left} at some time can be uniform over Γ_{total} at some later point. Hence, as it stands, the Gibbs approach cannot explain the approach to equilibrium.

3.5.1 Coarse-Graining

The 'official' Gibbsian proposal is that this problem is best addressed by coarse-graining the phase space; the idea is introduced in Chapter XII of Gibbs (1902) and has since been endorsed, among others, by Penrose (1970), Farquhar (1964), and all supporters of the programme of stochastic dynamics discussed below. The procedure is exactly the same as in the Boltzmann case (Subsection 2.2), with the exception that we now coarse-grain the system's γ -space rather than its μ -space.

The so-called coarse-grained density $\bar{\rho}$ is defined as the density that is uniform within each cell, taking as its value the average value in this cell of

the original continuous density ρ :

$$\bar{\rho}_\omega(q, p, t) := \frac{1}{\delta\omega} \int_{\omega(q,p)} \rho(q', p', t) d\Gamma', \quad (33)$$

where $\omega(q, p)$ is the cell in which the point (q, p) lies and $\delta\omega$ is the Lebesgue measure of a cell. Whether we work with $\bar{\rho}_\omega$ or ρ is of little importance to the practitioner because for any phase function that does not fluctuate on the scale of $\delta\omega$ (which is true of most physically relevant phase functions) the phase average with respect to $\bar{\rho}_\omega$ and ρ are approximately the same.

We can now define the coarse-grained entropy S_ω :

$$S_\omega(\rho) := S_G(\bar{\rho}_\omega) = -k_B \int_\Gamma \bar{\rho}_\omega \log(\bar{\rho}_\omega) d\Gamma \quad (34)$$

One can prove that the coarse-grained entropy is always greater or equal to the fine-grained entropy: $S_\omega(\rho) \geq S_G(\rho)$; the equality holds only if the fine grained distribution is uniform over the cells of the coarse-graining (see Uffink 1995b, 155; Wehrl 1978, 229; Lavis 2004, 672).

What do we gain by working with $\bar{\rho}_\omega$ rather than with ρ ? The main point is that the coarse-grained density $\bar{\rho}_\omega$ is not governed by Liouville's equation and hence is not subject to the restrictions mentioned above. So it is, at least in principle, possible for $\bar{\rho}_\omega$ to evolve in such a way that it will be uniform over the portion of the phase space available to the system in equilibrium. This state is referred to as 'coarse-grained equilibrium' (Ridderbos 2002, 69). The approach to coarse-grained equilibrium happens if under the dynamics of the system ρ becomes so scrambled that an equal portion of it is located in every cell of the partition. Because the averaged density is 'blind' to differences within each cell, the spread out states of the initial equilibrium condition will, on the averaged level, look like a homogenous distribution. This is illustrated in Figure 8 for the example mentioned at the beginning of this subsection, where the initial density is constant over Γ_{left} while the final density is expected to be constant over Γ_{total} (this figure is adapted from Uffink 1995b, 154).

[Insert Figure 8 (appended at the end of the document).]

Figure 8: Evolution into a quasi-equilibrium distribution.

A fine-grained distribution which has evolved in this way, i.e. which appears to be uniform at the coarse-grained level, is said to be in a quasi-equilibrium (Blatt 1959, 749; Ridderbos 2002, 73). On the coarse-graining view, then, all that is required to explain the approach to equilibrium in the Gibbs approach is a demonstration that an arbitrary initial distribution indeed evolves into a quasi-equilibrium distribution (Ridderbos 2002, 73).

The question then is under what circumstances this happens. The standard answer is that the system has to be mixing (see Subsection 2.4.1 for a discussion of mixing). This suggestion has some intuitive plausibility given the geometrical interpretation of mixing, and it receives further support from the convergence theorem (Equation (20)). In sum, the proposal is that we coarse-grain the system's phase space and then consider the coarse-grained entropy, which indeed increases if the system is mixing.

What can be said in support of this point of view? The main thrust of arguments in favour of coarse-graining is that even if there are differences between the fine-grained and the coarse-grained density, we cannot empirically distinguish between them and hence there is no reason to prefer one to the other. There are various facets to this claim; these are discussed but not endorsed in Ridderbos (2002, 73). First, measurements have finite precision and if $\delta\omega$ is chosen so that it is below that precision, no measurement that we can perform on the system will ever be able to tell us whether the true distribution is ρ or $\bar{\rho}_\omega$.

Second, as already observed above, the values of macroscopic variables calculated using the coarse-grained density coincide with those calculated using the fine-grained density (if the relevant phase function does not fluctuate so violently as to fluctuate on the scale of $\delta\omega$). This is all we need because thermodynamic equilibrium is defined in terms of the values of macroscopic parameters and as long as these coincide there is no reason to prefer the fine-grained to a coarse-grained density.

This programme faces several serious difficulties. To begin with, there is the problem that mixing is only defined, and so only achieved, for $t \rightarrow \infty$, but thermodynamic systems seem to reach equilibrium in finite time. One might try to mitigate the force of this objection by saying that it is enough for a system to reach an 'almost mixed' state in the relevant finite time. The problem with this suggestion is that from the fact that a system is mixing nothing follows about how fast it reaches a mixed state and hence it is not clear whether it becomes 'almost mixed' over the relevant observation times (see Berkovitz *et al.* 2006, 687). Moreover, mixing is too stringent

a requirement for many realistic systems. Mixing implies ergodicity and, a fortiori, if a system is not ergodic it cannot be mixing (see Subsection 2.4.1). But there are relevant systems that fail to be ergodic, and hence also fail to be mixing (as we have seen in Unit 2.4.3). This is a serious difficulty and unless it can be argued—as Vranas did with regards to ergodicity—that systems which fail to be mixing are ‘almost mixing’ in some relevant sense and reach some ‘almost mixed state’ in some finite time, an explanation of the approach to equilibrium based on mixing is not viable.

Second, there is a consistency problem, because we now seem to have two different definitions of equilibrium (Ridderbos 2002, 73). One is based on the requirement that the equilibrium distribution be stationary; the other on apparent uniformity. These two concepts of equilibrium are not co-extensive and so we face the question of which one we regard as the constitutive one. Similarly, we have two notions of entropy for the same system. Which one really is the system’s entropy? However, it seems that this objection need not really trouble the proponent of coarse-graining. There is nothing sacrosanct about the formalism as first introduced above and, in keeping with the revisionary spirit of the coarse-graining approach, one can simply declare that equilibrium is defined by uniformity relative to a partition and that S_ω is the ‘real’ entropy of the system.

Third, as in the case of Boltzmannian coarse-graining, there is a question about the justification of the introduction of a partition. The main justification is based on the finite accuracy of observations, which can never reveal the precise location of a system’s micro-state in its γ -space. As the approach to equilibrium only takes place on the coarse-grained level, we have to conclude that the emergence of thermodynamic behaviour depends on there being limits to the observer’s measurement resolution. This, so the objection continues, is misguided because thermodynamics does not appeal to observers of any sort and thermodynamic systems approach equilibrium irrespective of what those witnessing this process can know about the system’s micro-state.

This objection can be challenged on two grounds. First, one can mitigate the force of this argument by pointing out that micro-states have no counterpart in thermodynamics at all and hence grouping some of them together on the basis of experimental indistinguishability cannot possibly lead to a contradiction with thermodynamics. All that matters from a thermodynamic point of view is that the macroscopic quantities come out right, and this is the case in the coarse-graining approach (Ridderbos 2002, 71). Second, the above suggestion does not rely on there being actual observers, or

actual observations taking place. The claim simply is that the fine-grained distribution has to reach quasi-equilibrium. The concept is defined relative to a partition, but there is nothing subjective about that. Whether or not a system reaches quasi-equilibrium is an objective matter of fact that depends on the dynamics of the system, but has nothing to do with the existence of observers.

Those opposed to coarse-graining reply that this is besides the point because the very justification for introducing a partition to begin with is an appeal to limited observational capacities so that whether or not quasi-equilibrium is an objective property *given* a particular partition is simply a non-issue. So, at bottom, the disagreement seems to be over the question of whether the notion of equilibrium is essentially a macroscopic one. That is, does the notion of equilibrium make sense to creatures with unlimited observational powers? Or less radically: do they need this notion? It is at least conceivable that for them the gas indeed does not approach equilibrium but moves around in some very complicated but ever changing patterns, which only look stable and unchanging to those who cannot (or simply do not) look too closely. Whether or not one finds convincing a justification of coarse-graining by appeal to limited observational powers depends on how one regards this possibility.

Fourth, one can question the central premise of the argument for regarding $\bar{\rho}_\omega$ as the relevant equilibrium distribution, namely that $\bar{\rho}_\omega$ and ρ are empirically indistinguishable. Blatt (1959) and Ridderbos & Redhead (1998) argue that this is wrong because the spin-echo experiment (Hahn 1950) makes it possible to empirically discriminate between ρ or $\bar{\rho}$, even if the size of the cells is chosen to be so small that no direct measurement could distinguish between states within a cell. For this reason, they conclude, replacing ρ with $\bar{\rho}_\omega$ is illegitimate and an appeal to coarse-graining to explain the approach to equilibrium has to be renounced.

In the spin-echo experiment, a collection of spins is placed in a magnetic field \vec{B} pointing along the z -axis and the spins are initially aligned with this field (Figure 9.1).

[Insert Figure 9.1 (appended at the end of the document).]

Figure 9.1: Spins aligned with a magnetic field.

Then the spins are subjected to a radio frequency pulse, as a result of which they are tilted by 90 degrees so that they now point in the x -direction (Figure 9.2).

[Insert Figure 9.2 (appended at the end of the document).]

Figure 9.2: Spins shifted 90° by a pulse.

Due to the presence of the magnetic field \vec{B} , the spins start precessing around the z -axis and in doing so emit an oscillating electromagnetic pulse, the ‘free induction decay signal’ (Figure 9.3; the curved dotted arrows indicate the direction of rotation).

[Insert Figure 9.3 (appended at the end of the document).]

Figure 9.3: Precession of spins.

This signal is the macroscopic evidence for the fact that all spins are aligned and precess around the same axis. After some time this signal decays, indicating that the spins are now no longer aligned and point in ‘random’ directions (Figure 9.4).

[Insert Figure 9.4 (appended at the end of the document).]

Figure 9.4: Signal decays and spins point in random directions.

The reason for this is that the precession speed is a function of the field strength of \vec{B} and it is not possible to create an exactly homogeneous magnetic field. Therefore the precession frequencies of the spins differ slightly,

resulting in the spins pointing in different directions after some time $t = \tau$ has elapsed. At that point a second pulse is applied to the system, tilting the spins in the $x - z$ plane by 180 degrees (Figure 9.5; the straight dotted arrows indicate the direction of the spins before the pulse).

[Insert Figure 9.5 (appended at the end of the document).]

Figure 9.5: Spins after a second pulse.

The result of this is a reversal of the order of the spins in the sense that the faster spins that were ahead of the slower ones are now behind the slower ones (Figure 9.6; s_1 and s_2 are two spins, s'_1 and s'_2 their ‘tilted versions’).

However, those that were precessing faster before the second pulse keep doing so after the pulse and hence ‘catch up’ with the slower ones. After time $t = 2\tau$ all spins are aligned again and the free induction decay signal reappears (the ‘echo pulse’). This is the macroscopic evidence that the original order has been restored.⁶⁶

[Insert Figure 9.6 (appended at the end of the document).]

Figure 9.6: Order of spins (in terms of speed) is reversed.

At time $t = \tau$, when all spins point in random directions, $\bar{\rho}$ is uniform and the system has reached its coarse-grained equilibrium. From a coarse-grainer’s point of view this is sufficient to assert that the system *is* in equilibrium, as

⁶⁶It is often said that this experiment is the empirical realisation of a Loschmidt velocity reversal (in which a ‘Loschmidt demon’ instantaneously transforms the velocities \vec{v}_i of all particles in the system into $-\vec{v}_i$). This is incorrect. The directions of precession (and hence the particles’ velocities) are *not* reversed in the experiment. The reflection of the spins in the $x - z$ plane results in a reversal of their ordering while leaving their velocities unaltered. The grain of truth in the standard story is that a reversal of the ordering with unaltered velocities is in a sense ‘isomorphic’ to a velocity reversal with unaltered ordering.

we cannot distinguish between true and coarse-grained equilibrium. So according to Blatt and Redhead & Ridderbos the spin-echo experiment shows that this rationale is wrong because we actually *can* distinguish between true and coarse-grained equilibrium. If the system was in true equilibrium at $t = \tau$ the second radio pulse flipping the spins by 180 degrees would not have the effect of aligning the spins again at $t = 2\tau$; this only happens because the system is merely in a coarse-grained equilibrium. Hence equilibrium and quasi-equilibrium distributions can be shown to display radically different behaviour. Moreover, this difference is such that we can experimentally detect it without measuring microdynamical variables: we simply check whether there is an echo-pulse at $t = 2\tau$. This pulls the rug from under the feet of the coarse-grainer and we have to conclude that it is therefore not permissible to base fundamental arguments in statistical mechanics on coarse-graining (Blatt 1959, 746).

What is the weight of this argument? Ridderbos (2002, 75) thinks that the fact that we can, after all, experimentally distinguish between $\bar{\rho}$ and ρ , and hence between ‘real’ equilibrium and quasi-equilibrium, is by itself a sufficient reason to dismiss the coarse-graining approach. Others are more hesitant. Ainsworth (2005, 626-7) points out that, although valid, this argument fails to establish its conclusion because it assumes that for coarse-graining approach to be acceptable $\bar{\rho}$ and ρ must be empirically indistinguishable. Instead, he suggests appealing to the fact, proffered by some in support of Boltzmannian coarse-graining, that there is an objective separation of the micro and macro scales (see Subsection 2.7). He accepts this point of view as essentially correct and submits that the same response is available to the Gibbsian: coarse-graining can be justified by an appeal to the separation of scales rather than by pointing to limitations of what we can observe. As the notion of equilibrium is one that inherently belongs to the realm of the macroscopic, coarse-grained equilibrium is the correct notion of equilibrium, irrespective of what happens at the micro scale. However, as I have indicated above, the premise of this argument is controversial since it is not clear whether there is indeed an objective separation of micro and macro scales.

Ridderbos & Redhead make their case against coarse-graining by putting forward two essentially independent arguments. Their first argument is based on theoretical results. They introduce a mathematical model of the experiment and then show that the coarse-grained distribution behaves in a way that leads to false predictions. They show that the system reaches a uniform coarse-grained distribution over the entire phase space at $t = \tau$ (as one would

expect), but then fails to evolve back into a non-equilibrium distribution under reversal, so that, in coarse-grained terms, the system is still described by a uniform distribution at $t = 2\tau$ (1998, 1250). Accordingly, the coarse-grained entropy reaches its maximum at $t = \tau$ and does not decrease as the spins evolve back to their initial positions. Hence, the coarse-grained entropy is still maximal when the echo pulse occurs and therefore the occurrence of the echo is, from a coarse-grained perspective, completely miraculous (1998, 1251).

Their second argument is based on the assumption that we can, somehow, experimentally observe the coarse-grained entropy (as opposed to calculating it in the model). Then we face the problem that observational results seem to tell us that the system has reached equilibrium at time $t = \tau$ and that after the application of the second pulse at that time evolves away from equilibrium; that is, we are led to believe that the system behaves anti-thermodynamically. This, Ridderbos and Redhead (1998, 1251) conclude, is wrong because the experiments do not actually contradict the Second Law.

So the experimental results would stand in contradiction both with the theoretical results predicting that the coarse-grained entropy assumes its maximum value at $t = 2\tau$ and with the second law of thermodynamics, which forbids high to low entropy transitions in isolated systems (and the spin echo system is isolated after the second pulse). This, according to Ridderbos & Redhead, is a *reductio* of the coarse-graining approach.⁶⁷

These arguments have not gone unchallenged. The first argument has been criticised by Lavis (2004) on the grounds that the behaviour of $\bar{\rho}$ and the coarse-grained entropy predicted by Ridderbos & Redhead is an artifact of the way in which they calculated these quantities. There are two methods for calculating $\bar{\rho}$. The first involves a coarse-graining of the fine-grained distribution at each instant of time; i.e. the coarse-grained distribution at time t is determined by first calculating the fine-grained distribution at time t (on the basis of the time evolution of the system and the initial distribution) and then coarse-graining it. The second method is based on re-coarse-graining as time progresses; i.e. the coarse-grained distribution at time t is calculated by evolving the coarse-grained distribution at an earlier time and then re-

⁶⁷Their own view is that the fine grained entropy is the correct entropy and that we were wrong to believe that the entropy ever increased. Despite appearances, the thermodynamic entropy does not increase between $t = 0$ and $t = \tau$ and hence there is no need for it to decrease after $t = \tau$ in order to resume its initial value at $t = 2\tau$; it is simply constant throughout the experiment. However, this view is not uncontroversial (Sklar 1993, 253-4).

coarse-graining. Lavis points out that Ridderbos & Redhead use the second method: they calculate $\bar{\rho}$ at time $t = 2\tau$ by evolving $\bar{\rho}$ at time $t = \tau$ forward in time. For this reason, the fact that they fail to find $\bar{\rho}$ returning to its initial distribution is just a manifestation of the impossibility of ‘un-coarse-graining’ a coarse-grained distribution. Lavis then suggests that we should determine that coarse-grained distribution at some time t by using the first method, which, as he shows, yields the correct behaviour: the distribution returns to its initial form and the entropy decreases in the second half of the experiment, assuming its initial value at $t = 2\tau$. Hence the echo-pulse does not come as a surprise after all.

The question now is which of the two coarse-graining methods one should use. Although he does not put it quite this way, Lavis’ conclusion seems to be that given that there are no physical laws that favour one method over the other, the principle of charity should lead us to choose the one that yields the correct results. Hence Ridderbos & Redhead’s result has no force against the coarse-graining.

As regards the second argument, both Lavis (2004) and Ainsworth (2005) point out that the decrease in entropy during the second half of the experiment need not trouble us too much. Ever since the work of Maxwell and Boltzmann ‘entropy increase in an isolated system is taken to be highly probable but not certain, and the spin-echo model, along with simulations of other simple models, is a nice example of the working of the law’ (Lavis 2004, 686). On this view, the spin-echo experiment simply affords us one of these rare examples in which, due to skilful engineering, we can prepare a system in one of these exceptional states which evolve from high to low entropy.

3.5.2 Interventionism

One of the crucial assumptions, made more or less tacitly so far, is that the systems under consideration are isolated. This, needless to say, is an idealising assumption that can never be realised in practice. Real systems cannot be perfectly isolated from their environment and are always subject to interactions; for instance, it is impossible to shield a system from gravitation. Blatt (1959) suggested that taking systems to be isolated not only fails to be the harmless idealisation that it is generally believed to be; it actually is the source of the problem. This recognition is the starting point for the interventionist programme, at the heart of which lies the idea that real systems are open in that they are constantly subject to outside perturbations, and

that it is exactly these perturbations that drive the system into equilibrium.

In more detail, the leading idea is that every system interacts with its environment (the gas, for instance, collides with the wall of the container and the walls interact in many different ways with their surroundings), and that these interactions are ‘*in principle* not amenable to a causal description, and *must of necessity* be described in statistical terms’ (Blatt 1959, 751, original emphasis). The perturbations from outside serve as a kind of ‘stirring mechanism’ or ‘source of randomness’ that drives the system around randomly in the phase space, in much the same way as it would be the case if the system was mixing. As a consequence, the observable macroscopic quantities are soon driven towards their equilibrium values. This includes the Gibbs entropy; in an open system Liouville’s theorem no longer holds and there is nothing to prevent the Gibbs entropy from increasing.⁶⁸

Of course, from the fact that the Gibbs entropy can increase it does not follow that it actually does increase; whether or not this is the case depends on the system as well as the properties of the outside perturbations. Blatt (1959) and Ridderbos & Redhead (1998) assure us that in realistic model systems one can prove this to be the case. Granting this, we have an elegant explanation of why and how systems approach equilibrium, which also enjoys the advantage that no revision of the classical laws is needed.⁶⁹

A common objection against this suggestion points out that we are always free to consider a larger system, consisting of our ‘original’ system *and* its environment. For instance, we can consider the ‘gas cum box’ system, which, provided that classical mechanics is a universal theory, is also governed by classical mechanics. So we are back to where we started. Interventionism, then, seems wrong because it treats the environment as a kind of *deus ex machina* that is somehow ‘outside physics’; but the environment is governed by the fundamental laws of physics just as the system itself is and so it cannot do the job that the interventionist has singled out for it to do.

The interventionist might now reply that the ‘gas cum box’ system has

⁶⁸It is a curious fact about the literature on the subject that interventionism is always discussed within the Gibbs framework. However, it is obvious that interventionism, if true, would also explain the approach to equilibrium in the Boltzmannian framework as it would explain why the state of the system wanders around randomly on the energy surface, which is needed for it to ultimately end up in the equilibrium region (see Unit 2.3.1).

⁶⁹For a discussion of interventionism and time-reversal see Ridderbos & Redhead (1998, 1259-62) and references therein.

an environment as well and it is *this* environment that effects the desired perturbations. This answer does not resolve the problems, of course. We can now consider an even larger system that also encompasses the environment of the ‘gas cum box’ system. And we can keep expanding our system until the relevant system is the entire universe, which, by assumption, has no environment any more that might serve as a source of random perturbations.

Whether this constitutes a reductio of the interventionist programme depends on one’s philosophical commitments. The above argument relies on the premise that classical mechanics (or quantum mechanics, if we are working within quantum SM) is a universal theory, i.e. one that applies to everything that there is without restrictions. This assumption, although widely held among scientists and philosophers alike, is not uncontroversial. Some have argued that we cannot legitimately claim that laws apply universally. In fact, laws are always tested in highly artificial laboratory situations and claiming that they equally apply outside the laboratory setting involves an inductive leap that is problematic. Hence we have no reason to believe that classical mechanics applies to the universe as a whole; see for instance Reichenbach (1956) and Cartwright (1999) for a discussion of this view. This, if true, successfully undercuts the above argument against interventionism.⁷⁰

There is a way around the above objection even for those who do believe in the generality of laws, namely to deny Blatt’s assumption that the environment needs to be genuinely stochastic. *Pace* Blatt, that the environment be genuinely stochastic (i.e. as governed by indeterministic laws rather than classical mechanics) is not an indispensable part of the interventionist programme. As Ridderbos & Redhead (1998, 1257) point out, all that is required is that the system loses coherence, which can be achieved by dissipating correlations into the environment. For observations restricted to the actual system, this means that correlational information is not available. But the information is not lost; it has just been ‘dislocated’ into the degrees of freedom pertaining to the environment.

The question then becomes whether the universe as a whole is expected to approach equilibrium, or whether thermodynamic behaviour is only required

⁷⁰Interventionists are sometimes charged with being committed to an instrumentalist take on laws, which, the critics continue, is an unacceptable point of view. This is mistaken. Whatever one’s assessment of the pros and cons of instrumentalism, all the interventionist needs is the denial that the laws (or more specifically, the laws of mechanics) are universal laws. This is compatible with realism about laws understood as providing ‘local’ descriptions of ‘parts’ of the universe (a position sometime referred to as ‘local realism’).

to hold for a subsystem of the universe. Those who hold that the ‘dissipation’ of correlational information into environmental degrees of freedom is enough to explain the approach to equilibrium are committed to this view. Ridderbos & Redhead are explicit about this (1998, 1261-2). They hold that the fine-grained Gibbs entropy of the universe is indeed constant since the universe as a whole has no outside, and that there is no approach to equilibrium at the level of the universe. Moreover, this does not stand in conflict with the fact that cosmology informs us that the entropy of the universe is increasing; cosmological entropies are coarse-grained entropies and, as we have seen above, there is no conflict between an increase in coarse-grained entropy and the constancy of the fine-grained Gibbs entropy. Ridderbos & Redhead acknowledge that the question now is whether the claim that the Gibbs entropy of the universe is constant is true, which is an issue that has to be settled empirically.

3.5.3 Changing the notion of Equilibrium

One of the main problems facing Gibbsian non-equilibrium theory is that under a Hamiltonian time evolution a non-stationary distribution cannot evolve into a stationary one (see Unit 3.2.4). Hence strict stationarity is too stringent a requirement for equilibrium. Nevertheless, it seems plausible to assume that an equilibrium distribution has to approximate a stationary distribution in some relevant sense. What is this relevant sense?

Van Lith suggested turning the desired result into a definition and replacing strict stationarity with the requirement that the distribution be such that the phase average of every function in a physically relevant set of functions only fluctuates mildly around its average value (van Lith 1999, 114). More precisely, let Ω be a class of phase functions $f(x)$ corresponding to macroscopically relevant quantities. Then the system is in equilibrium from time τ onwards iff for every function $f(x) \in \Omega$ there is a constant c_f such that:

$$\left| \int f(x)\rho_t(x)d\Gamma - c_f \right| \leq \varepsilon_f, \quad (35)$$

where ε_f is a small number (which can be different for every f). This definition of equilibrium seems to have the advantage of preserving all the desirable features of equilibrium while no longer running into the problem that equilibrium can never be reached.

However, from the fact that an arbitrary non-equilibrium distribution

can reach equilibrium thus defined it does not follow that it actually *does*. What conditions does the dynamics of a system have to meet in order for the approach to equilibrium to take place? Van Lith points out that being mixing is a sufficient condition (van Lith 1999, 114) because the Convergence Theorem (see Subsection 2.4.1) states that in the limit all time averages converge to the microcanonical averages, and hence they satisfy the above definition.

But this proposal suffers from various problems. First, as van Lith herself points out (1999, 115), the proposal does not contain a recipe to get the (fine-grained) Gibbs entropy moving; hence the approach to equilibrium need not be accompanied by a corresponding increase in the Gibbs entropy.

Second, as we have seen above, mixing is too stringent a condition: it is not met by many systems of interest. Remedy for this might be found in the realisation that less than full-fledged mixing is needed to make the above suggestion work. In fact, all we need is a condition that guarantees that the Convergence Theorem holds (Earman & Redei 1996, 74; van Lith 1999, 115). One condition of that sort is that the system has to be mixing for all $f \in \Omega$. The question then is, what this involves. This question is difficult, if not impossible to answer, before Ω is precisely specified. And even then there is the question of whether the convergence is sufficiently rapid to account for the fact that thermodynamic systems reach equilibrium rather quickly.⁷¹

3.5.4 Alternative Approaches

Before turning to the epistemic approach, I would like to briefly mention three other approaches to non-equilibrium SM; lack of space prevents me from discussing them in more detail.

Stochastic Dynamics. The leading idea of this approach is to replace the Hamiltonian dynamics of the system with an explicitly probabilistic law of evolution. Characteristically this is done by coarse-graining the phase space and then postulating a probabilistic law describing the transition from one cell of the partition to another one. Liouville's theorem is in general not true for such a dynamics and hence the problem of the constancy of the Gibbs entropy does not arise. Brief introductions can be found in Kreuzer (1981, Ch. 10), Reif (1985, Ch. 15) and Honerkamp (1998, Ch. 5); detailed

⁷¹Another alternative definition of equilibrium, which also applies to open systems, has been suggested by Pitowsky (2001, 2006), but for a lack of space I cannot further discuss this suggestion here.

expositions of the approach include Penrose (1970; 1979), Mackey (1989; 1992), and Streater (1995).

The main problem with this approach is that its probabilistic laws are put in ‘by hand’ and are not derived from the underlying dynamics of the system; that is, it is usually not possible to derive the probabilistic laws from the underlying deterministic evolution and hence the probabilistic laws are introduced as independent postulates. However, unless one can show how the transition probabilities postulated in this approach can be derived from the Hamiltonian equations of motion governing the system, this approach does not shed light on how thermodynamical behaviour emerges from the fundamental laws governing a system’s constituents. For critical discussions of the stochastic dynamics programme see Sklar (1993, Chs. 6 and 7), Callender (1999, 358-364) and Uffink (2007, 1038-1063).

The Brussels School (sometimes also ‘Brussels-Austin School’). An approach closely related to the Stochastic Dynamics programme has been put forward by the so-called Brussels School, led by Ilya Prigogine. The central contention of this programme is that if the system exhibits sensitive dependence on initial conditions (and most systems do) the very idea of a precise micro-state given by a point in phase space ceases to be meaningful and should be replaced by an explicitly probabilistic description of the system in terms of open regions of the phase space, i.e. by a Gibbs distribution function. This programme, if successful, can be seen as providing the sought after justification for the above-mentioned shift from a Hamiltonian micro dynamics to an explicitly probabilistic scheme. These claims have been challenged on different grounds; for presentations and critical discussions of the ideas of the Brussels School see Batterman (1991), Bricmont (1996), Karakostas (1996), Lombardi (1999, 2000), Edens (2001) and Bishop (2004).

An approach that is similar to the programme of the Brussels School in that it denies that the conceptual framework of classical mechanics, in particular the classical notion of a state, is adequate to understand SM, has been suggested by Krylov. Unfortunately he died before he could bring his programme to completion, and so it not clear what form his ideas would have taken in the end. For philosophical discussions Krylov’s programme see Batterman (1990), Rédei (1992) and Sklar (1993, 262-69).

The BBGKY Hierarchy. The main idea of the BBGKY (after Bogolyubov, Born, Green, Kirkwood, and Yvon) approach is to describe the evolution of an ensemble by dint of a reduced probability density and then derive (something like) a Boltzmann equation for this density, which yields the approach

to equilibrium. The problem with the approach is that, just as in the case of the Boltzmann's (early) theory, the irreversibility is a result of (something like) the *Stosszahlansatz*, and hence all its difficulties surface again at this point. For a discussion of this approach see Uffink (2007, 1034-1038).

3.6 The Epistemic Approach

The approaches discussed so far are based on the assumption that SM probabilities are ontic (see Unit 2.3.2). It is this assumption that those who argue for an epistemic interpretation deny. They argue that SM probabilities are an expression of what we know about a system, rather than a feature of a system itself. This view can be traced back to Tolman (1938) and has been developed into an all-encompassing approach to SM by Jaynes in a series of papers published (roughly) between 1955 and 1980, some of which are gathered in Jaynes (1983).⁷²

At the heart of Jaynes' approach to SM lies a radical reconceptualisation of what SM is. On his view, SM is about our knowledge of the world, not about the world itself. The probability distribution represents our state of knowledge about the system at hand and not some matter of fact about the system itself. More specifically, the distribution represents our lack of knowledge about a system's micro-state given its macro condition; and, in particular, entropy becomes a measure of how much knowledge we lack. As a consequence, Jaynes regards SM as a part of general statistics, or 'statistical inference', as he puts it:

'Indeed, I do not see Predictive Statistical Mechanics and Statistical Inference as different subjects at all; the former is only a particular realization of the latter [...] Today, not only do Statistical Mechanics and Statistical Inference not appear to be two different fields, even the term 'statistical' is not entirely appropriate. Both are special cases of a simple and general procedure that ought to be called, simply, "inference".' (Jaynes 1983, 2 - 3)

The questions then are: in what way a probability distribution encodes a lack of knowledge; according to what principles the correct distribution is

⁷²In this subsection I focus on Jaynes' approach. Tolman's view is introduced in his (1938, 59-70); for a discussion of Tolman's interpretation of probability see Uffink (1995b, 166-7).

determined; and how this way of thinking about probabilities sheds any light on the foundation of SM. The first and the second of these questions are addressed in Unit 3.6.1; the third is discussed in 3.6.2.

3.6.1 The Shannon Entropy

Consider a random variable x which can take any of the m discrete values in $X = \{x_1, \dots, x_m\}$ with probabilities $p(x_i)$; for instance, x can be the number of spots showing on the next roll of a die, in which case $X = \{1, 2, 3, 4, 5, 6\}$ and the probability for each even is $1/6$. The Shannon entropy of the probability distribution $p(x_i)$ is defined (Shannon 1949) as:

$$S_s(p) := - \sum_{i=1}^m p(x_i) \log(p(x_i)), \quad (36)$$

which is a quantitative measure for the uncertainty of the outcome. If the probability for one particular outcome is one while the probabilities for all other outcomes are zero, then there is no uncertainty and S_s equals zero; S_s reaches its maximum for a uniform probability distribution, i.e. $p(x_i) = 1/m$ for all i , in which case we are maximally uncertain about the outcome; an accessible discussion of the relation between the Shannon entropy and uncertainty can be found Jaynes (1994, Ch. 11); see Cover and Thomas (1991) for a detailed treatment.

Sometimes we are given X but fail to know the $p(x_i)$. In this case Jaynes's maximum entropy principle (MEP) instructs us to choose that distribution $p(x_i)$ for which the Shannon entropy is maximal (under the constraint $\sum_{i=1}^m p(x_i) = 1$). For instance, from this principle it follows immediately that we should assign $p = 1/6$ to each number of spots when rolling the die. If there are constraints that need to be taken into account then MEP instructs us to choose that distribution for which S_s is maximal under the given constraints. The most common type of constraint is that the expectation value for a particular function f has a given value c :

$$\langle f \rangle := \sum_{i=1}^m f(x_i) p(x_i) = c. \quad (37)$$

This can be generalised to the case of a continuous variable, i.e. $X = (a, b)$, where (a, b) is an interval of real numbers (the boundaries of this interval can be finite or infinite). The continuous Shannon entropy is

$$S_s(p) := - \int_a^b p(x) \log[p(x)] dx, \quad (38)$$

where $p(x)$ is a probability density over (a, b) .⁷³ So for a continuous variable the most common type of constraint is

$$\langle f \rangle := \int_a^b f(x)p(x)dx = c, \quad (39)$$

and MEP tells us to choose $p(x)$ such that it maximises $S_s(p)$ under the given constraints.

Why is MEP compelling? The intuitive idea is that we should always choose the distribution that corresponds to a maximal amount of uncertainty, i.e. is maximally non-committal with respect to the missing information. But why is this a sound rule? In fact MEP is fraught with controversy; and, to date, no consensus on its significance, or even cogency, has been reached. However, debates over the validity of MEP belong to the foundations of statistical inference in general and as such they are beyond the scope of this review; for discussions see, for instance, Lavis (1977), Denbigh and Denbigh (1985), Lavis and Milligan (1985, Sec. 5), Shimony (1985), Seidenfeld (1986), Uffink (1995a; 1996a), Howson and Urbach (2006, 276-288).

In what follows let us, for the sake of argument, assume that MEP can be justified satisfactorily and discuss what it has to offer for the foundations of SM. But before moving on, a remark about the epistemic probabilities here employed is in place. On the current view, epistemic probabilities are not subjective, i.e. they do not reduce to the personal opinion of individual observers, as would be the case in a personalist Bayesian theory (such as de Finetti's). On the contrary Jaynes advocates an 'impersonalism' that bases probability assignments solely on the available data and MEP; anybody's personal opinions do not enter the scene at any point. Hence, referring to Jaynes' position as 'subjectivism' is a—frequently used—misnomer.

⁷³This generalisation is problematic in many respects; and for the continuum limit to be taken properly, a background measure and the relative entropy need to be introduced. In the simplest case where the background measure is the Lebesgue measure we retrieve Equation (38). For a discussion of this issue see Uffink (1995a, 235-239).

3.6.2 MEP and SM

The appeal of MEP for equilibrium SM lies in the fact that the continuous Shannon entropy is equivalent to the Gibbs entropy (28) up to the multiplicative constant k_B if in Equation (38) we take X to be the phase space and ρ the probability distribution. Gibbsian equilibrium distributions are required to maximise S_G under certain constraints and hence, trivially, they also satisfy MEP. For an isolated system, for instance, the maximum entropy distribution is the microcanonical distribution. In fact, even more has been achieved: MEP not only coincides with the Gibbsian maximum entropy principle introduced in Subsection 3.1; on the current view, this principle, which above has been postulated without further explanation, is justified because it can be understood as a version of MEP.

As we have seen at the beginning of this subsection, Jaynes sees the aim of SM as making predictions, as drawing inferences. This opens a new perspective on non-equilibrium SM, which, according to Jaynes, should refrain from trying to explain the approach to equilibrium by appeal to dynamical or other features of the system and only aim to make predictions about the system's future behaviour (1983, 2). Once this is understood, the puzzle of the approach to equilibrium has a straightforward two-step answer. In Sklar's (1993, 255-257) reconstruction, the argument runs as follows.

The first step consists in choosing the initial distribution. Characteristic non-equilibrium situations usually arise from the removing of a constraint (e.g. the opening of a shutter) in a particular equilibrium situation. Hence the initial distribution is chosen in the same way as an equilibrium distribution, namely by maximising the Shannon entropy S_S relative to the known macroscopic constraints. Let $\rho_0(q, p, t_0)$ be that distribution, where t_0 is the instant of time at which the constraint in question is removed. Assume now that the experimental set-up is such that a set of macroscopic parameters corresponding to the phase functions f_i , $i = 1, \dots, k$, are measured. At time t_0 these have as expected values

$$\bar{f}_i(t_0) = \int f_i(q, p) \rho_0(q, p, t_0) d\Gamma, \quad i = 1, \dots, k. \quad (40)$$

Furthermore we assume that the entropy which we determine in an actual experiment, the experimental entropy S_e , at time t_0 is equal to the Gibbs entropy: $S_e(t_0) = S_S(\rho_0(t_0))$.

The second step consists in determining the distribution and the entropy of a system at some time $t_1 > t_0$. To this end we first use Liouville's equation

to determine the image of the initial distribution under the dynamics of the system, $\rho_0(t_1)$, and then calculate the expectation values of the observable parameters at time t_1 :

$$\bar{f}_i(t_1) = \int f_i(q, p) \rho_0(q, p, t_1) d\Gamma, \quad i = 1, \dots, k. \quad (41)$$

Now we calculate a new density $\rho_1(q, p, t_1)$, which maximises the Shannon entropy under the constraints that

$$\int f_i(q, p) \rho_1(q, p, t_1) d\Gamma = \bar{f}_i(t_1), \quad i = 1, \dots, k. \quad (42)$$

The experimental entropy of the system at t_1 then is $S_e(t_1) = S_s(\rho_1(t_1))$. This entropy is greater than or equal to $S_e(t_0)$ for the following reason. By Liouville's theorem we have $S_s(\rho_0(t_0)) = S_s(\rho_0(t_1))$. Both $S_s(\rho_0(t_1))$ and $S_s(\rho_1(t_1))$ satisfy the constraints in Equation (42). By construction, $S_s(\rho_1(t_1))$ is maximal relative to these constraints; this need not be the case for $S_s(\rho_0(t_1))$. Therefore $S_e(t_0) \leq S_e(t_1)$. This is Jaynes' justification of the Second Law.

Jaynes' epistemic approach to SM has several interesting features. Unlike the approaches that we have discussed so far, it offers a clear and cogent interpretation of SM probabilities, which it views as rational degrees of belief. This interpretation enjoys the further advantage over its ontic competitors that it can dispense with ensembles. On Jaynes' approach there is only one system, the one on which we are performing our experiments, and viewing probabilities as reflecting our lack of knowledge about this system rather than some sort of frequency renders ensembles superfluous. And most importantly, the problems so far that beset non-equilibrium theory no longer arise: the constancy of the Gibbs entropy becomes irrelevant because of the 're-maximising' at time t_1 , and the stationarity of the equilibrium distribution is no longer an issue because the dynamics of the probability distribution is now a function of both our epistemic situation and the dynamics of the system, rather than *only* Liouville's equation. And last but not least—and this is a point that Jaynes himself often emphasised—all this is achieved without appealing to complex mathematical properties like ergodicity or even mixing.

3.6.3 Problems

Let us discuss Jaynes' approach to non-equilibrium SM first. Consider a sequence $t_0 < t_1 < t_2 < \dots$ of increasing instants of time and consider the

entropy $S_e(t_j)$, $j = 0, 1, \dots$ at these instants; all the $S_e(t_j)$, $j \geq 2$ are calculated with Equation (42) after substituting ρ_j for ρ_1 . Conformity with the Second Law would require that $S_e(t_0) \leq S_e(t_1) \leq S_e(t_2) \leq \dots$. However, this is generally not the case (Lavis and Milligan 1985, 204; Sklar 1993, 257-258) because the experimental entropy S_e is not necessarily a monotonically increasing function. Jaynes' algorithm to calculate the $S_e(t_j)$ can only establish that $S_e(t_0) \leq S_e(t_j)$, for all $j > 0$ but it fails to show that $S_e(t_i) \leq S_e(t_j)$, for all $0 < i < j$; in fact, it is indeed possible that $S_e(t_i) > S_e(t_j)$ for some $i < j$.

A way around this difficulty would be to use $\rho_{j-1}(t_{j-1})$ to calculate $\rho_j(t_j)$, rather than $\rho_0(t_0)$. This would result in the sequence becoming monotonic, but it would have the disadvantage that the entropy curve would become dependent on the sequence of instants of time chosen (Lavis and Milligan *ibid.*). This seems odd even from a radically subjectivist point of view: why should the value of S_e at a particular instant of time, depend on earlier instants of time at which we chose to make predictions, or worse, why should it depend on us having made any predictions at all?

In equilibrium theory, a problem similar to the one we discussed in connection with the ergodic approach (Subsection 3.3) arises. As we have seen in Equation (40), Jaynes also assumes that experimental outcomes correspond to phase averages as given in Equation (25). But why should this be the case? It is correct that we should rationally expect the mean value of a sequence of measurements to coincide with the phase average, but *prima facie* this does not imply anything about individual measurements. For instance, when throwing a die we expect the mean of a sequence of events to be 3.5; but we surely don't expect the die to show 3.5 spots after any throw! So why should we expect the outcome of a measurement of a thermodynamic parameter to coincide with the phase average? For this to be the case a further assumption seems to be needed, for instance (something like) Khinchin's assumption that the relevant phase functions assume almost the same value for almost all points of phase space (see Subsection 3.3.2)

A further problem is that the dynamics of the system does not play any rôle in Jaynes' derivation of the microcanonical distribution (or any other equilibrium distribution that can be derived using MEP). This seems odd because even if probability distributions are eventually about our (lack of) knowledge, it seems that what we can and cannot know must have something to do with how the system behaves. This point becomes particularly clear from the following considerations (Sklar 1993, 193-194). Jaynes repeat-

edly emphasised that ergodicity—or the failure thereof—does not play any rôle in his account. This cannot be quite true. If a system is not ergodic then the phase space decomposes into two (or more) invariant sets (see Unit 2.4.1). Depending on what the initial conditions are, the system’s state may be confined to some particular invariant set, where the relevant phase functions have values that differ from the phase average; as a consequence MEP leads to wrong predictions. This problem can be solved by searching for the ‘overlooked’ constants of motion and then controlling for them, which yields the correct results.⁷⁴ However, the fact remains that our *original* probability assignment was wrong, and this was because we have ignored certain important dynamical features of the system. Hence the correct application of MEP depends, after all, on dynamical features of the system. More specifically, the micro canonical distribution seems to be correct only if there are no such invariant subsets, i.e. if the system is ergodic.

A final family of objections has to do with the epistemic interpretation of probabilities itself (rather than with ‘technical’ problems in connection with the application of the MEP formalism). First, the Gibbs entropy is defined in terms of the distribution ρ , and if ρ pertains to our epistemic situation rather than to (aspects of) the system, it, strictly speaking, does not make any sense to say that entropy is a property of the *system*; rather, entropy is a property of our knowledge of the system. Second, in the Gibbs approach equilibrium is defined in terms of specific properties that the distribution ρ must possess at equilibrium (see Subsection 3.1). Now the same problem arises: if ρ reflects our epistemic situation rather than facts about the system, then it does not make sense to say that the *system* is in equilibrium; if anything, it is our knowledge that is in equilibrium. This carries over the non-equilibrium case. If ρ is interpreted epistemically, then the approach to equilibrium also pertains to our knowledge and not to the system. This has struck many commentators as outright wrong, if not nonsensical. Surely, the boiling of kettles or the spreading of gases has something to do with how the molecules constituting these systems behave and not with what we happen (or fail) to know about them (Redhead 1995, 27-28; Albert 2000, 64; Goldstein 2001, 48; Loewer 2001, 611). Of course, nothing is sacred, but further explanation is needed if such a radical conceptual shift is to appear plausible.

Against the first point Jaynes argues that entropy is indeed epistemic even in TD (1983, 85-86) because here is no such thing as *the* entropy of a

⁷⁴Quay (1978, 53-54) discusses this point in the context of ergodic theory.

physical system. In fact, the entropy is relative to what variables one chooses to describe the system; depending on how we describe the system, we obtain different entropy values. From this Ben-Menahem (2001, Sec. 3) draws the conclusion that, Jaynes' insistence on knowledge notwithstanding, one should say that entropy is relative to descriptions rather than to knowledge, which would mitigate considerably the force of the objection. This ties in with the fact (mentioned in Sklar 1999, 195) that entropy is only defined by its function in the theory (both in TD and in SM); we neither have a phenomenal access to it nor are there measurement instruments to directly measure entropy. These points do, to some extent, render an epistemic (or descriptive) understanding of entropy more plausible, but whether they in anyway mitigate the implausibility that attaches to an epistemic understanding of equilibrium and the approach to equilibrium remains an open question.

3.7 Reductionism

How does the Gibbsian approach fare with reducing TD to SM? The aim of a reduction is the same as in the Boltzmannian case: deduce a revised version of the laws of TD from SM (see Subsection 2.8). The differences lie in the kind of revisions that are made. I first discuss those approaches that proffer an ontic understanding of probabilities and then briefly discuss how reduction could be construed.

Boltzmann took over from TD the notion that entropy and equilibrium are properties of an individual system and sacrificed the idea that equilibrium (and the associated entropy values) are stationary. Gibbs, on the contrary, retains the stationarity of equilibrium, but at the price of making entropy and equilibrium properties of an ensemble rather than an individual system. This is because both equilibrium and entropy are defined in terms of the probability distribution ρ , which is a distribution over an ensemble and not over an individual system. Since a particular system can be a member of many different ensembles one can no longer assert that an individual system is in equilibrium. This 'ensemble character' carries over to other physical quantities, most notably temperature, which are also properties of an ensemble and not of an individual system.

This is problematic because the state of an individual system can change considerably as time evolves while the ensemble average does not change at all; so we cannot infer from the behaviour of an ensemble to the behaviour of an individual system. However, what we are dealing with in experimen-

tal contexts are individual systems; and so the shift to ensembles has been deemed inadequate by some. Maudlin (1995, 147) calls it a ‘Pyrrhic victory’ and Callender (1999) argues that this and related problems disqualify the Gibbs approach as a serious contender for a reduction of TD.

It is worth observing that Gibbs himself never claimed to have reduced TD to SM and only spoke about ‘thermodynamic analogies’ when discussing the relation between TD and SM; see Uffink (2007, 994-996) for a discussion. The notion of analogy is weaker than that of reduction, but it is at least an open question whether this is an advantage. If the analogy is based on purely algebraic properties of certain variables then it is not clear what, if anything, SM contributes to our understanding of thermal phenomena; if the analogy is more than a merely formal one, then at least some of the problems that we have been discussing in connection with reduction are bound to surface again.

4 Conclusion

Before drawing some general conclusions from the discussion in Sections 2 and 3, I would like to briefly mention some of the issues, which, for lack of space, I could not discuss.

4.1 Sins of Omission

SM and the Direction of Time. The discussion of irreversibility so far has focussed on the problem of the directionality of change *in* time. One can take this one step further and claim that this directionality in fact *constitutes* the direction of time itself (the ‘arrow of time’). Attempts to underwrite the arrow of time by an appeal to the asymmetries of thermodynamics and SM can be traced back to Boltzmann, and have been taken up by many since. The literature on the problem of the direction of time is immense and it is impossible to give a comprehensive bibliography here; instead I mention just some approaches that are closely related to SM. The modern locus classicus for a view that seeks to ground the arrow of time on the flow of entropy is Reichenbach (1956). Earman (1974) offers a sceptical take on this approach and provides a categorisation of the different issues at stake. These are further discussed in Sklar (1981; 1993, Ch. 10), Price (1996; 2002a; 2002b), Horwich (1987), Callender (1998), Albert (2000, Ch. 6), Brown (2000), North

(2002), Castagnino & Lombardi (2005), Hagar (2005) and Frisch (2006).

The Gibbs paradox. Consider a container that is split in two halves by a barrier in the middle. The left half is filled with gas G_1 , the right half with a different gas G_2 ; both gases have the same temperature. Now remove the shutter. As a result both gases start to spread and get mixed. We then calculate the entropy of the initial and the final state and find that the entropy of the mixture is greater than the entropy of the gases in their initial compartments. This is the result that we would expect. The paradox arises from the fact that the calculations do *not* depend on the fact that the gases are different; that is, if we assume that we have air of the same temperature on both sides of the barrier the calculations still yield an increase in entropy when the barrier is removed. This seems wrong since it would imply that the entropy of a gas depends on its history and cannot be a function of its thermodynamic state alone (as thermodynamics requires).

What has gone wrong? The standard ‘textbook solution’ of this problem is that classical SM gets the entropy wrong because it makes a mistake when counting states (see for instance Huang 1963, 153-4; Greiner *et al.* 1993, 206-8). The alleged mistake is that we count states that differ only by a permutation of two indistinguishable particles as distinct, while we should not do this. Hence the culprit is a flawed notion of individuality, which is seen as inherent to classical mechanics. The solution, so the argument goes, is provided by quantum mechanics, which treats indistinguishable particles in the right way.

This argument raises a plethora of questions concerning the nature of individuality in classical and quantum mechanics, the way of counting states in both the Boltzmann and the Gibbs approach, and the relation of SM to thermodynamics. These issues are discussed in Rosen (1964), Lande (1965), van Kampen (1984), Denbigh and Denbigh (1985, Ch. 4), Denbigh and Redhead (1989), Jaynes (1992), Redhead and Teller (1992), Mosini (1995), Costantini & Garibaldi (1997, 1998), Huggett (1999), Gordon (2002) and Saunders (2006).

Maxwell’s Demon. Imagine the following scenario, originating in a letter of Maxwell’s written in 1867. Take two gases of different temperature that are separated from one another only by a wall. This wall contains a shutter, which is operated by a demon who carefully observes all molecules. Whenever a particle moves towards the shutter from the *colder* side and the particle’s velocity is greater than the mean velocity of the particles in the *hotter* gas, then the demon opens the shutter, and so lets the particle pass through.

Similarly, when a particle heads for the shutter from within the *hotter* gas and the particle's velocity is lower than the mean velocity of the particles of the *colder* gas, then the demon lets the particle pass through the shutter. The net effect of this is that the hotter gas becomes even hotter and the colder one even colder. So we have a heat transfer from the cooler to the hotter gas, and this without doing any work; it is only the skill and intelligence of the demon, who is able to sort molecules, that brings about the heat transfer. But this sort of heat transfer is not allowed according to the Second Law of thermodynamics. So the conclusion is that the demon has produced a violation of the second law of thermodynamics.

In Maxwell's own interpretation, this thought experiment shows that the second law is not an exceptionless law; it rather describes a general tendency for systems to behave in a certain way, or, as he also puts it, it shows that the second law has only 'statistical certainty'. Since Maxwell, the demon had a colourful history. In particular, in the wake of Szilard's work, much attention has been paid to the entropy costs of processing and storing information. These issues are discussed in Daub (1970), Klein (1970), Leff & Rex (1990; 2003), Shenker (1997; 1999), Earman & Norton (1998, 1999), Albert (2000, Ch. 5), Bub (2001), Bennett (2003), Norton (2005), Maroney (2005) and Ladyman *et al.* (2007).

Entropy. There are a number of related but not equivalent concepts denoted by the umbrella term 'entropy': thermodynamic entropy, Shannon entropy, Boltzmann entropy (fine-grained and coarse-grained), Gibbs entropy (fine-grained and coarse-grained), Kolmogorov-Sinai entropy, von Neumann entropy and fractal entropy, to mention just the most important ones. It is not always clear how these relate to one another as well as to other important concepts such as algorithmic complexity and informational content. Depending on how these relations are construed and on how the probabilities occurring in most definitions of entropy are interpreted, different pictures emerge. Discussions of these issues can be found in Grad (1961), Jaynes (1983), Wehrl (1978), Denbigh & Denbigh (1985), Denbigh (1989b), Barrett & Sober (1992; 1994; 1995), Smith *et al.* (1992), Denbigh (1994), Frigg (2004), Balian (2005), and with a particular focus on entropy in quantum mechanics in Shenker (1999), Henderson (2003), Timpson (2003), Campisi (2005, 2008), Sorkin (2005) and Hemmo & Shenker (2006). The relation between entropy and counterfactuals is discussed in Elga (2001) and Kutach (2002).

Quantum Mechanics and Irreversibility. This review was concerned with

the problem of somehow ‘extracting’ time-asymmetric macro laws from time-symmetric classical micro laws. How does this project change if we focus on quantum rather than classical mechanics? *Prima facie* we are faced with the same problems because the Schrödinger equation is time reversal invariant (if we allow replacing the wave function by its complex conjugate when evolving it backwards in time). However, in response to the many conceptual problems of quantum mechanics new interpretations of quantum mechanics or even alternative quantum theories have been suggested, some of which are not time reversal invariant. Dynamical reduction theories (such as GRW theory) build state collapses into the fundamental equation, which thereby becomes non time-reversal invariant. Albert (1994a; 1994b; 2000, Ch. 7) has suggested that this time asymmetry can be exploited to underwrite thermodynamic irreversibility; this approach is discussed in Uffink (2002). Another approach has been suggested by Hemmo and Shenker who, in a series of papers, develop the idea that we can explain the approach to equilibrium by environmental decoherence (2001; 2003; 2005).

Phase Transitions. Most substances, for instance water, can exist in different phases (liquid, solid, gas). Under suitable conditions, so-called phase transitions can occur, meaning that the substance changes from, say, the liquid to the solid phase. How can the phenomenon of phase transitions be understood from a microscopic point of view? This question is discussed in Sewell (1986, Chs. 5-7), Lebowitz (1999), Liu (2001) and Emch & Liu (2002, Chs. 11-14).

SM methods outside physics. Can the methods of SM be used to deal with problems outside physics? In some cases it seems that this is the case. Constantini & Garibaldi (2004) present a generalised version of the Ehrenfest flea model and show that it can be used to describe a wide class of stochastic processes, including problems in population genetics and macroeconomics. The methods of SM have also been applied to markets, a discipline now known as ‘econophysics’; see Voit (2005) and Rickles (2008).

4.2 Summing Up

The foremost problem of the foundation of SM is the lack of a generally accepted and universally used formalism, which leads to a kind of schizophrenia in the field. The Gibbs formalism has a wider range of application and is mathematically superior to the Boltzmannian approach and is therefore the practitioner’s workhorse. In fact, virtually all practical applications of SM

are based on the Gibbsian machinery. The weight of successful applications notwithstanding, a consensus has emerged over the last decade and a half that the Gibbs formalism cannot explain why SM works and that when it comes to foundational issues the Boltzmannian approach is the only viable option (see Lavis (2005) and references therein). Hence, whenever the question arises of why SM is so successful, an explanation is given in Boltzmannian terms .

This is problematic for at least two reasons. First, at least in its current form, the Boltzmann formalism has a very limited range of applicability. The Boltzmann formalism only applies to non (or very weakly) interacting particles and at the same time it is generally accepted that the Past Hypothesis, an assumption about the *universe as a whole*, is needed to make it work. But the universe as a whole is not an a collection of weakly interacting system, not even approximately.

Second, even if the internal problems of the Boltzmann approach can be solved, we are left with the fact that what delivers the goodies in ‘normal science’ is the Gibbs rather than the Boltzmann approach. This would not be particularly worrisome if the two formalisms were intertranslatable or equivalent in some other sense (like, for instance, the Schrödinger and the Heisenberg picture in quantum mechanics). However, as we have seen above, this is not the case. The two frameworks disagree fundamentally over what the object of study is, the definition of equilibrium, and the nature of entropy. So even if all the internal difficulties of either of these approaches were to find a satisfactory solution, we would still be left with the question of how the two relate.

A suggestion of how these two frameworks could be reconciled has recently been presented by Lavis (2005). His approach involves the radical suggestion to give up the notion of equilibrium, which is binary in that systems either are or not in equilibrium, and to replace it by the continuous property of ‘commonness’. Whether this move is justified and whether it solves the problem is a question that needs to be discussed in the future.

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Appendix

A. Classical Mechanics

CM can be presented in various more or less but not entirely equivalent formulations: Newtonian mechanics, Lagrangean mechanics, Hamiltonian mechanics and Hamilton-Jacobi theory; for comprehensive presentations of these see Arnold (1978), Goldstein (1980), Abraham & Marsden (1980) and José & Saletan (1998). Hamiltonian Mechanics (HM) is best suited to the purposes of SM; hence this appendix focuses entirely on HM.

CM describes the world as consisting of point-particles, which are located at a particular point in space and have a particular momentum. A system's state is fully determined by a specification of each particle's position and momentum. Conjoining the space and momentum dimension of all particles of a system in one vector space yields the so-called phase space Γ of the system. The phase space of a system with m degrees of freedom is $2m$ dimensional; for instance, the phase space of a system consisting of n particles in three-dimensional space has $6n$ dimensions. Hence, the state of a mechanical system is given by the $2m$ -tuple $x := (q, p) := (q_1, \dots, q_m, p_1, \dots, p_m) \in \Gamma$. The phase space Γ is endowed with a Lebesgue measure μ_L , which, in the context of SM, is also referred to as the 'standard measure' or the 'natural measure'.

The time evolution of the system is governed by Hamilton's equation of motion:⁷⁵

$$\dot{q}_i = \frac{\partial H}{\partial p_i} \quad \text{and} \quad \dot{p}_i = -\frac{\partial H}{\partial q_i}, \quad i = 1, \dots, m, \quad (43)$$

where $H(q, p, t)$ is the so-called 'Hamiltonian' of the system. Under most circumstances the Hamiltonian is the energy of the system (this is not true in systems with time dependent boundary conditions, but these do not play a rôle in the present discussion).

⁷⁵The dot stands for the total time derivative: $\dot{f} := df/dt$.

If the Hamiltonian satisfies certain conditions (see Arnold (2006) for a discussion of these) CM is deterministic in the sense that the state x_0 of the system *at some particular* instant of time t_0 (the so-called ‘initial condition’) uniquely determines the state of the system at any other time t . Hence, each point in Γ lies on exactly one trajectory (i.e. no two trajectories in phase space can ever cross) and $H(q, p, t)$ defines a one parameter group of transformations ϕ_t , usually referred to as ‘phase flow’, mapping the phase space onto itself: $x \rightarrow \phi_t(x)$ for all $x \in \Gamma$ and all t .

A quantity f of the system is a function of the coordinates and (possibly) time: $f(q, p, t)$. The time evolution of f is given by

$$\dot{f} = \{f, H\} + \frac{\partial f}{\partial t}, \quad (44)$$

where $\{ , \}$ is the so-called Poisson bracket:

$$\{g, h\} := \sum_i \left[\frac{\partial g}{\partial q_i} \frac{\partial h}{\partial p_i} - \frac{\partial h}{\partial q_i} \frac{\partial g}{\partial p_i} \right], \quad (45)$$

for any two differentiable functions g and h on Γ .

From this it follows that H is a conserved quantity iff it does not explicitly depend on time. In this case we say that the motion is *stationary*, meaning that the phase flow depends only on the *time interval* between the beginning of the motion and ‘now’ but not on the choice of the initial time. If H is a conserved quantity, the motion is confined to a $2m - 1$ dimensional hypersurface Γ_E , the so-called ‘energy hypersurface’, defined by the condition $H(q, p) = E$, where E is the value of the total energy of the system.

Hamiltonian dynamics has three distinctive features, which we will now discuss.

Liouville’s theorem asserts that the Lebesgue measure (in this context also referred to as ‘phase volume’) is invariant under the Hamiltonian flow:

For any Lebesgue measurable region $R \subseteq \Gamma$ and for any time t :
 R and the image of R under the Hamiltonian flow, $\phi_t(R)$, have
the same Lebesgue measure; i.e. $\mu_L(R) = \mu_L(\phi_t(R))$.

In geometrical terms, a region R can (and usually will) change its shape but not its volume under the Hamiltonian time evolution.

This also holds true if we restrict the motion of the system to the energy hypersurface Γ_E , provided we choose the ‘right’ measure on Γ_E . We obtain

this measure, $\mu_{L,E}$, by restricting μ_L to Γ_E so that the $6n - 1$ dimensional hypervolume of regions in Γ_E is conserved under the dynamics. This can be achieved by dividing the surface element $d\sigma_E$ on Γ_E by the gradient of H (Kac 1959, 63):

$$\mu_{L,E}(R_E) := \int_{R_E} \frac{d\sigma_E}{\|\text{grad}H\|} \quad (46)$$

for any $R_E \subseteq \Gamma_E$, where

$$\|\text{grad}H\| := \left[\sum_{k=1}^n \left(\frac{\partial H}{\partial p_k} \right)^2 + \left(\frac{\partial H}{\partial q_k} \right)^2 \right]^{1/2}. \quad (47)$$

We then have $\mu_{L,E}(R_E) = \mu_{L,E}(\phi_t(R_E))$ for all $R_E \subseteq \Gamma_E$ and for all t .

Poincaré's recurrence theorem: Roughly speaking, Poincaré's recurrence theorem says that any system that has finite energy and is confined to a finite region of space must, at some point, return arbitrarily close to its initial state, and does so infinitely many times. The time that it takes the system to return close to its initial condition is called 'Poincaré recurrence time'. Using the abstract definition of a dynamical system introduced in Unit 2.4.1, the theorem can be stated as follows:

Consider an area-preserving mapping of the phase space X of a system onto itself, $\phi_t(X) = X$, and suppose that its measure is finite, $\mu(X) < \infty$. Then, for any measurable subset A with $\mu_L(A) > 0$ of X , almost every point x in A returns to A infinitely often; that is, for all finite times τ the set $B := \{x|x \in A \text{ and for all times } t \geq \tau : \phi_t x \notin A\}$ has measure zero.

The Hamiltonian systems that are of interest in SM satisfy the requirements of the theorem if we associate X with the accessible region of the energy hypersurface.

Time reversal invariance. Consider, say, a ball moving from left to right and record this process on videotape. Intuitively, time reversal amounts to playing the tape backwards, which makes us see a ball moving from right to left. Two ingredients are needed to render this idea precise, a transformation reversing the direction of time, $t \rightarrow -t$, and the reversal of a system's instantaneous state. In some contexts it is not obvious what the instantaneous

state of a system is and what should be regarded as its reverse.⁷⁶ In the case of HM, however, the ball example provides a lead. The instantaneous state of a system is given by (q, p) , and in the instant in which the time is reversed the ball suddenly ‘turns around’ and moves from right to left. This suggests the sought-after reversal of the instantaneous state amounts to changing the sign of the momentum: $R(q, p) := (q, -p)$, where R is the reversal operator acting on instantaneous states.

Now consider a system in the initial state (q_i, p_i) at time t_i that evolves, under the system’s dynamics, into the final state (q_f, p_f) at some later time t_f . The entire process (‘history’) is a parametrised curve containing all intermediates states: $h := \{(q(t), p(t)) | t \in [t_i, t_f]\}$, where $(q(t_i), p(t_i)) = (q_i, p_i)$ and $(q(t_f), p(t_f)) = (q_f, p_f)$. We can now define the time-reversed process of h as follows: $Th := \{R(q(-t), p(-t)) | t \in [-t_f, -t_i]\}$, where T is the time-reversal operator acting on histories. Introducing the variable $\tau := -t$ and applying R we have $Th = \{(q(\tau), -p(\tau)) | \tau \in [t_i, t_f]\}$. Hence, Th is a process in which the system evolves from state $R(q_f, p_f)$ to state $R(q_i, p_i)$ when τ ranges over $[t_i, t_f]$.

Call the class of processes h that are allowed by a theory A ; in the case of HM A contains all trajectories that are solutions of Hamilton’s equation of motion. A theory is time reversal invariant (TRI) iff for every h : if $h \in A$ then $Th \in A$ (that is, if A is closed under time reversal). Coming back to the analogy with videotapes, a theory is TRI iff a censor who has to ban films containing scenes which violate the law of the theory issues a verdict which is the same for either direction of playing the film (Uffink 2001, 314). This, however, does not imply that the processes allowed by a TRI theory are all palindromic in the sense that the processes *themselves* look the same when played backwards; this can but need not be the case.

HM is TRI in this sense. This can be seen by time-reversing the Hamiltonian equations: carry out the transformations $t \rightarrow \tau$ and $(q, p) \rightarrow R(q, p)$ and after some elementary algebraic manipulations you find $dq_i/d\tau = \partial H/\partial p_i$ and $dp_i/d\tau = -\partial H/\partial q_i$, $i = 1, \dots, m$. Hence the equations have the same form in either direction of time, and therefore what is allowed in one direction of

⁷⁶A recent controversy revolves around this issue. Albert (2000, Ch. 1) claims that, common physics textbook wisdom notwithstanding, neither electrodynamics, nor quantum mechanics nor general relativity nor any other fundamental theory turns out to be time reversal invariant once the instantaneous states and their reversals are defined correctly. This point of view has been challenged by Earman (2002), Uffink (2002) and Malament (2004), who defend common wisdom; for a further discussion see Leeds (2006).

time is also allowed in the other.⁷⁷

The upshot of this is that if a theory is TRI then the following holds: if a transition from state (q_i, p_i) to state (q_f, p_f) in time span $\Delta := t_f - t_i$ is allowed by the lights of the theory, then the transition from state $R(q_f, p_f)$ to state $R(q_i, p_i)$ in time span Δ is allowed as well, and vice versa. This is the crucial ingredient of Loschmidt's reversibility objection (see Subsection 2.3.3).

B. Thermodynamics

Thermodynamics is a theory about macroscopic quantities such as pressure, volume and temperature and it is formulated solely in terms of these; no reference to unobservable microscopic entities is made. At its heart lie two laws, the First Law and Second Law of TD. Classical presentations of TD include Fermi (1936), Callen (1960), Giles (1964) and Pippard (1966).

The first law of thermodynamics. The first law says that there are two ways of exchanging energy with a system, putting heat into it and doing work on it, and that energy is a conserved quantity:

$$\Delta U = \Delta Q + \Delta W, \tag{48}$$

where ΔU is the energy put into the system, and ΔQ and ΔW are, respectively, the heat and work that went into the system. Hence, put simply, the first law, says that one cannot create energy and thereby rules out the possibility of a perpetual motion machine.

The second law of thermodynamics. The First Law does not constrain the ways in which one form of energy can be transformed into another one and how energy can be exchanged between systems or parts of a system. For instance, according to the first law it is in principle possible to transform heat into work or work into heat according to one's will, provided the total amount of heat is equivalent to the total amount of work. However, it turns out that although one can always transform work in to heat, there are severe

⁷⁷There was some controversy over the question of whether classical mechanics really is TRI; see Hutchison (1993, 1995a, 1995b), Savitt (1994) and Callender (1995). However, the moot point in this debate was the status of frictional forces, which, unlike in Newtonian Mechanics, are not allowed in HM. So this debate has no implications for the question of whether HM is TRI.

limitations on the ways in which heat can be transformed into work. These limitations are specified by the Second Law.

Following the presentation in Fermi (1936, 48-55), the main tenets of the Second Law can be summarised as follows. Let A and B be two equilibrium states of the system. Then consider a quasi-static transformation (i.e. one that is infinitely gentle in the sense that it proceeds only through equilibrium states), which takes the system from A to B . Now consider the integral

$$\int_A^B \frac{dQ}{T}, \quad (49)$$

where T is the temperature of the system and dQ is the amount of heat quasi-statically absorbed by the system. One can then prove that the value of this integral does not depend on the sequence by which one gets from A to B ; it only depends on A and B themselves.

Now choose an arbitrary equilibrium state E of the system and call it the *standard state*. Then we can define the entropy of the state A as

$$S(A) = \int_E^A \frac{dQ}{T}, \quad (50)$$

where the integral is taken over a quasi-static transformation.

With this at hand we can formulate the Second Law of thermodynamics:

$$\int_A^B \frac{dQ}{T} \leq S(B) - S(A). \quad (51)$$

For a totally isolated system we have $dQ = 0$. In this case the second law takes the particularly intuitive form:

$$S(A) \leq S(B). \quad (52)$$

That is, for any transformation in an isolated system, the entropy of the final state can never be less than that of the initial state. The equality sign holds if, and only if, the transformation is quasi-static.

Thermodynamics is not free of foundational problems. The status of the Second Law is discussed in Popper (1957), Lieb & Yngvason (1999) and Uffink (2001); Cooper (1967), Boyling (1972), Moulines (1975, 2000), Day (1977) and Garrido (1986) examine the formalism of TD and possible axiomatisations. The nature of time in TD is considered in Denbigh (1953) and Brown and Uffink (2001); Rosen (1959), Roberts and Luce (1968) and

Liu (1994) discuss the compatibility of TD and relativity theory. Wickens (1981) addresses the issue of causation in TD.

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Figures

Fig. 1.1



Figs 1.2 and 1.3

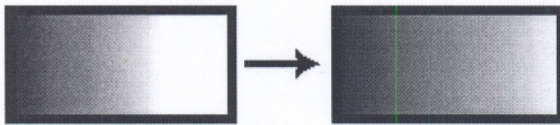


Fig 1.4

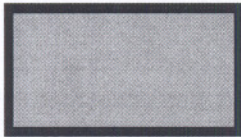


Fig. 2

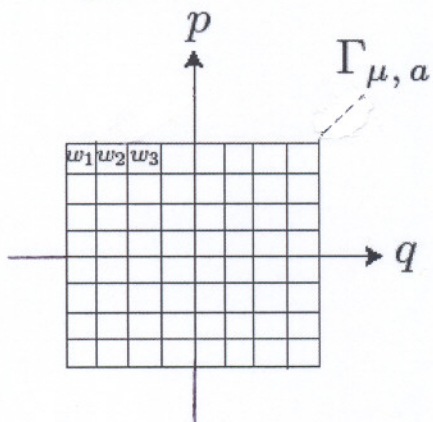


Fig. 3

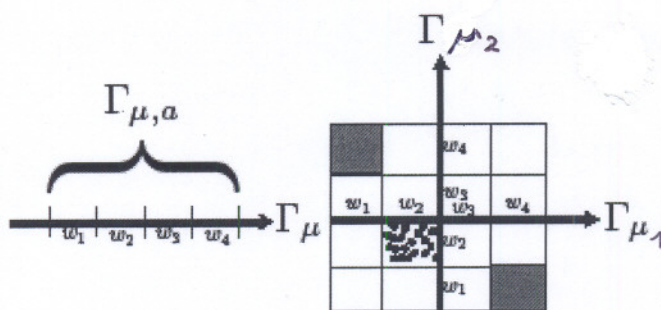


Fig. 4

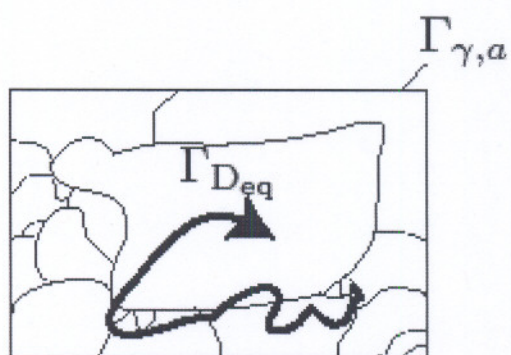


Fig. 5

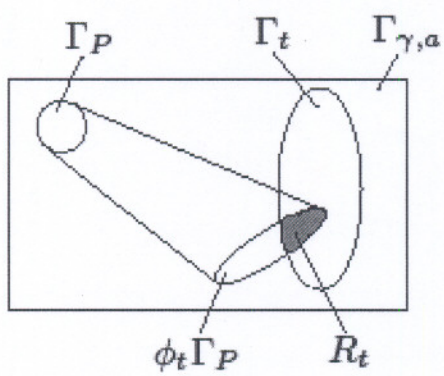


Fig. 6

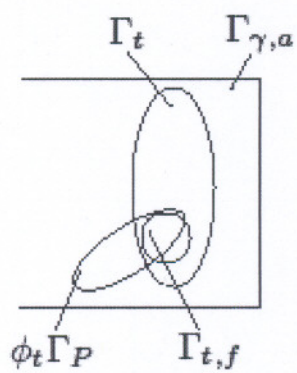


Fig. 7

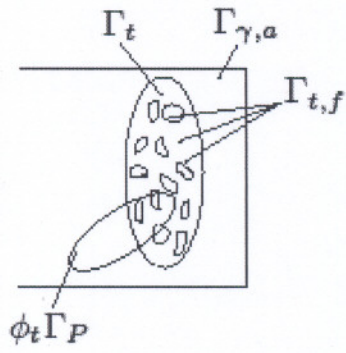


Fig. 8

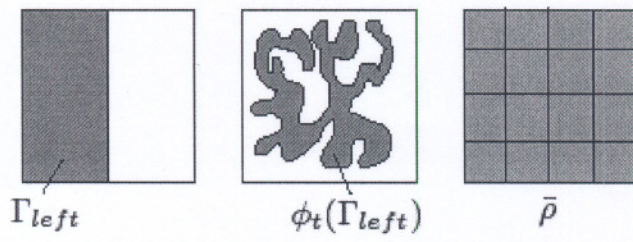


Fig. 9.1

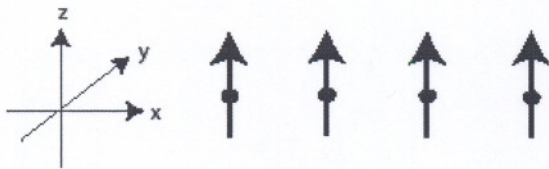


Fig. 9.2

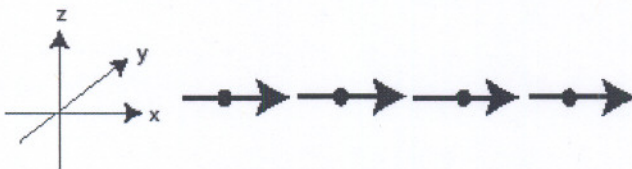


Fig. 9.3

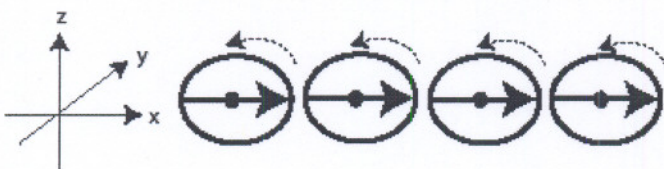


Fig. 9.4

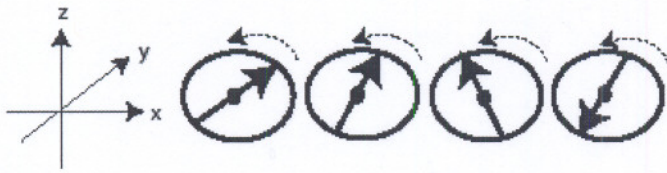


Fig. 9.5



Fig. 9.6

