Abstract

Thermodynamics has a clear arrow of time, characterized by the irreversible approach to equilibrium. This stands in contrast to the laws of microscopic theories, which are invariant under time-reversal. In this article, I show that the difficulty in solving this “problem of irreversibility” partly arises from the fact that it actually consists of five different sub-problems. These concern the source of irreversibility in thermodynamics, the definition of equilibrium and entropy, the justification of coarse-graining, the approach to equilibrium and the arrow of time. Not distinguishing them can lead and has led to terminological confusion, apparent contradictions between positions that are actually compatible, and difficulties in connecting the debates in physics and philosophy of physics.

1 Introduction

2 “The” Problem of Irreversibility

3 The Five Problems

4 First Problem: The Source of Irreversibility in Thermodynamics

5 Second Problem: The Definition of “Equilibrium” and “Entropy”

6 Third Problem: The Justification of Coarse-Graining

7 Fourth Problem: The (Irreversible) Approach to Equilibrium
1 Introduction

The temporal asymmetry of thermodynamics is one of the central problems in philosophy of physics. If a cup of hot coffee stands in a room, it will cool down until it has room temperature, but it will not spontaneously heat up by extracting heat from its environment. This is commonly seen as a consequence of the second law of thermodynamics, which assigns to each of these systems a quantity known as “entropy” that increases in these processes and that, most importantly, cannot decrease. This is often considered one of the most fundamental laws of physics (Callender [2001], p. 540).

However, as is also well-known, there is a conflict with the microscopic laws governing the motion of the individual particles that a macroscopic system consists of. These laws are invariant under time-reversal, which means that if a process can occur in one direction of time, it can also occur in the other direction. Thus, a cup of coffee at room temperature that spontaneously heats up would be in perfect agreement with the microscopic laws of physics, which makes it very difficult to explain why such a behaviour is never observed.

While an intense debate has thus emerged in philosophy of physics, physicists working on statistical mechanics do, in most cases, not worry a lot about this. In fact, there are well-established procedures which allow to systematically derive irreversible theories from the underlying reversible microphysics (see te Vrugt and Wittkowski ([2019b]) for an accessible introduction to methods of this type). If the problem of temporal asymmetry is discussed at all in such contexts, it is presented as having a relatively clear solution (Zwanzig [2001], pp. 193-197). At first sight, this suggests that either the physicists or the philosophers are overlooking

1When it comes to the most fundamental laws, this is not strictly true. The standard model of particle physics is invariant under CPT, which is a combination of charge-conjugation, mirror reflection and time-reversal. However, this effect is too small to account for the temporal asymmetry of thermodynamics (Wallace [2013], p. 270).
something important. Given that the philosophy of statistical mechanics has repeatedly been accused of not making enough contact with actual physical research on nonequilibrium statistical physics (see Wallace (2015, p. 286) and Luczak (2016, p. 394)), it is a worthwhile project to look for the origins of this disagreement.

Moreover, it would be helpful to get more structure into the debate around the origin of irreversibility that has emerged in both physics and philosophy. The approaches to this issue are very diverse. Some have suggested a relation to the limitations of human observers, others solve the problem by appealing to the entropy of the early universe (Albert 2000), the mathematical structure of its distribution function (Wallace 2011), external interventions (Blatt 1959), modifications of quantum mechanics (Albert 2000) or the autonomy of higher-level dynamics (Robertson 2018).

In this article, I will show that this lack of structure results from the fact that “the problem of irreversibility” is actually a conglomerate of five different problems. These are the source of irreversibility in thermodynamics, the definitions of “equilibrium” and “entropy”, the justification of coarse-graining, the approach to equilibrium and the arrow of time. Not distinguishing them can lead to difficulties, since positions are seen as being in conflict that are actually compatible. Moreover, certain approaches can be seen as solutions to “the problem” that only address one of the problems, or can be criticized for not solving a problem that they are not related to. Each problem has to be solved in order to achieve a solution of “the problem”, but as far as they are independent, they can be solved separately. In addition, such a distinction is required for connecting modern debates in physics and philosophy of physics, since physicists and philosophers tend to be interested in different questions. And finally, it turns out that a lack of clarity in the definitions can be a source of confusion in the debate. This paper aims to dissolve some of these confusions.

The article is structured as follows: In section 2, I explain how “the” problem of irreversibility arises, thereby also introducing the relevant physics. The five problems are introduced in section 3. In sections 4 to 8, each problem is explained in detail and related to approaches from the literature. I conclude in section 9.
2 “The” Problem of Irreversibility

In this section, I will introduce the standard view on what is commonly referred to as “the problem of irreversibility”. The presentation will be non-technical, focusing on conceptual issues that are relevant for the subsequent discussion. A more mathematical discussion can be found in Zeh ([1989]). For simplicity, I will focus on classical statistical mechanics. The situation in quantum mechanics is – as far as this problem is concerned – not much different. This section will present the standard view, such that all definitions and interpretations appearing in this section should be understood as preliminary - some problems are addressed in footnotes and in the later sections. I am not claiming that the view presented in this section is correct or terminologically precise.

The microscopic state of a many-particle system can be specified completely by giving position and momentum of every particle. These evolve according to Hamilton’s equations, which are the fundamental laws of classical mechanics and have the important property of being time-reversal invariant: If we record a movie of a process allowed by classical mechanics and play the movie backwards, then what we see will also be a process allowed by classical mechanics. The microscopic laws know no preferred direction of time. The microstate of a system is a point in the so-called phase space, which is the central playground of classical mechanics.

In practice, it is not possible to solve the microscopic equations of motion for every single particle for a macroscopic system, such that other methods are required. The macroscopic

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This view is not shared universally. For example, Albert ([2000], pp. 150-152) has suggested that the GRW theory – a modification of quantum mechanics that is not time-reversal-invariant (Friebe et al. [2018]) – might play a role in explaining the approach to equilibrium. However, the classification of the five questions proposed here is not affected by the possibility that quantum mechanics might provide an answer to one of them.

3 By a macroscopic system, I mean a system that contains about $10^{23}$ particles. Strictly speaking, the thermodynamic limit requires that a macroscopic systems has infinitely many particles (Thiele et al. [2019]). This idealization, however, leads to additional philosophical problems (Menon and Callender [2013]) that are not relevant here.
world is the domain of thermodynamics, which is a phenomenological theory that describes systems using a small number of macroscopic variables such as temperature or pressure. A quantity that is of particular interest here is entropy, which according to (a common interpretation of) the second law of thermodynamics can never decrease in a closed system. If the system has evolved to a state with maximal entropy, it will never leave this state - it has reached thermodynamic equilibrium.

Thus, there seems to be a contradiction between the microscopic laws of classical mechanics and the macroscopic laws of thermodynamics. In thermodynamics, systems are expected to evolve towards an equilibrium state, accompanied by an increase of entropy. This process is irreversible, since its time-reversal would involve a decrease of entropy and is therefore forbidden by the second law. The laws governing the microscopic constituents of the system, however, are time-reversal invariant and would therefore allow for such a process. Hence, we need to explain why we always observe an irreversible approach to equilibrium from past to future despite the fact that there is no difference between past and future in the microscopic laws.

The connection between the behaviour of the individual particles and the dynamics of the macroscopic system it consists of is studied in statistical mechanics. In the framework of Gibbs, one studies many-particle systems using ensembles. These are hypothetical sets of infinitely many copies of the system which evolve according to the same laws, but with different initial conditions. One then introduces a probability density that for each point in phase space, i.e., for each microscopic state, gives the probability that a system that is randomly chosen from the ensemble is in this state (see Frigg ([2008], pp. 153-154) and Frigg and Werndl ([2020], pp. 3-4)). It is helpful to think of this probability density as a fluid. In this framework, the entropy can then be given a microscopic definition as a function of the density - intuitively, it measures (the logarithm of) the volume of the fluid. The problem is now that one can easily prove using Hamilton’s equations that the volume of this “fluid” is constant, a result known as Liouville’s theorem. This implies, of course, that the entropy is also constant - in conflict with

\textsuperscript{4}An alternative is the Boltzmann framework, where only a single system is considered. See Frigg ([2008]) for a discussion of this approach and for a comparison.
thermodynamics, which demands that it increases during the approach to equilibrium.

In physics, this problem has not gone unnoticed, and is routinely solved through a procedure known as coarse-graining, which has originally been suggested by Gibbs ([1902]) (see also Robertson ([2018], pp. 3-5)). The starting point - in usual treatments - is that we are trying to describe macroscopic observations. Such observations do not allow to distinguish between certain microstates - if we shift a certain fluid molecule by a few nanometers, this will change the microstate of a bucket of full of water, but it will not make an observable difference. Therefore, we can simply group together the macroscopically indistinguishable microstates and average over them. Notably, this was done with an epistemic justification.

This replacement has important consequences: For the actual (fine-grained) distribution, Liouville’s theorem states that its volume has to be constant, which means that the entropy - which measures its volume - also has to be constant. But for the coarse-grained distribution, Liouville’s theorem does not hold. Hence, we can define, from the coarse-grained distribution, a coarse-grained entropy which measures its volume. This entropy is then allowed to increase. The coarse-graining can be mathematically implemented in various ways. I will denote by coarse-graining any procedure that involves replacing the microscopic distribution function of the system by an averaged one.

Procedures of this form are routinely and successfully used in physics to derive irreversible macroscopic transport equations from the underlying Hamiltonian dynamics. Nevertheless, they are discussed very controversally in philosophy. Redhead ([1995], pp. 27-31) even called coarse-graining a “ridiculous” and “disreputable procedure”. The objection is (roughly) that we have, through the coarse-graining, artificially introduced an asymmetry that has not been there originally. This has been justified by an appeal to our limited capability of observation, but the increase of entropy is a physical effect that should not depend on how good our microscopes are (Robertson [2018], pp. 17-19). An important alternative position is interventionism, defended by Blatt ([1959]) and Ridderbos ([2002]), where thermal phenomena are explained by influences from the environment.

Finally, whatever approach we might use to get an increasing Gibbs entropy, it can - because of the time-reversal invariance of the underlying laws - also be applied to the past.
This is a problem because our records of the past tell us that entropy used to be lower. A solution that is very popular in philosophy is the past hypothesis. The idea is that, since the asymmetry between past and future cannot be a consequence of the dynamical laws of the universe, it has to be a consequence of the boundary conditions. In particular, if we assume that the entropy of the early universe was very low, we could have an explanation for why it increases afterwards (Callender [2016]).

3 The Five Problems

It seems as if the debates concerning “the problem of irreversibility” in physics and philosophy do not really fit to each other. For example, if we ask why a statistical physicist who derives an irreversible transport equation for a macroscopic system from Hamilton’s equations can be successful, a philosopher supporting the past hypothesis might point to the entropy of the early universe. But the early universe plays no role in the considerations of a physicist who is looking at, say, a cup of coffee cooling down. Still, the physicist is successful.

The reason is, as I will show, that the physicist is aiming at a different problem than the philosopher. The debate around irreversibility is in fact not concerned with one, but with five different questions, namely

- **What is the source of irreversibility in thermodynamics? (Q1)**

  This question asks for the source of irreversibility within thermodynamics, i.e., purely on the level of the macroscopic theory. It is controversial which of the laws of thermodynamics is actually responsible here.

- **How should “equilibrium” and “entropy” be defined? (Q2)**

  Here, we look for a definition of the explanandum. The questions “Why do systems approach equilibrium?” and “Why does entropy increase?” are answered differently by persons who have different ideas on what equilibrium and entropy are, which is a common source of confusion.

- **(How) Can coarse-graining be justified? (Q3)**
Since coarse-graining, which is frequently used in explanations of the approach to equilibrium, is a controversial procedure, it needs to be clarified whether and how it can be justified. Moreover, coarse-graining is also used in situations without a relation to irreversibility.

- **Why do systems approach equilibrium? (Q4)**

  This question asks why systems, given an initial nonequilibrium state, (irreversibly) approach equilibrium.

- **Why does the arrow of time have the direction it has? (Q5)**

  Here, we look for an explanation for why our answer to Q4 does not apply to the past, i.e., we wish to find out why the entropy always increases from past to future.

Although they are, of course, to some degree connected, these are five different questions. The answer we give to one of them does not determine our position on the other ones. I have stated them here in a preliminary way to give an idea what they are about. The precise formulation depends, partly, on one’s position with respect to the other questions. For example we might ask for the approach to equilibrium (Q4) in a different way if we have a different idea of what equilibrium is (Q2), and we might or might not add the attribute “irreversible” to the approach to equilibrium.

Distinguishing between these questions allows to get more clarity into some aspects of the debate. Details are given below, but a good first example is the Boltzmann equation. This is an equation proposed by [Boltzmann](1872) that describes the behaviour of gases. Based on an assumption known as the *Stoßzahlansatz* - the velocities of colliding particles are not correlated before a collision - it is possible to derive the *H-theorem*, which states that there exists a certain function that cannot decrease and that is then identified with the entropy. This was seen as a first proof or microscopic explanation of the approach to equilibrium. Problems were quickly identified, including (but not limited to) the fact that presupposing the *Stoßzahlansatz* actually begs the question: It already introduces the direction of time we wish to get. If we had made the assumption that the velocities are uncorrelated after rather than before the collision, we would have obtained the reverse result. Thus, Boltzmann’s H-theorem is nowadays not regarded as
a satisfactory solution to the problem of irreversibility. (See Brown et al. ([2009]) for a more detailed discussion including the historical developments.)

However, as Wallace ([2015], p. 290) has pointed out, this might be too quick: The Boltzmann equation is applied very successfully in a variety of fields, giving quantitatively accurate predictions. The derivation of new “H-theorems” is common practice in statistical physics (Anero and Español [2007]), and the Stoßzahlansatz is a common ingredient of the modern theory of phase-space dynamics (Marini Bettolo Marconi and Tarazona [2006], p. 3). This makes it unlikely that it is a total failure. In the context of the five questions, we can understand in what respect the Boltzmann equation is successful: It provides, if we accept the assumptions involved in its derivation, an explanation of the approach to equilibrium, and gives a quantitative picture of how this approach to equilibrium proceeds. Hence, it is a useful approach for the solution of Q4. To explain why these assumptions can be made – more precisely, why we can make the assumption of uncorrelated collisions for before rather than after the collision – is the problem of Q5.

It varies how carefully these questions are distinguished in the debate. There is a good degree of awareness that the fourth and the fifth problem are different, distinctions of this form have been expressed by Boltzmann (see Brown and Uffink [2001], p. 530), Penrose ([1994], p. 218) and Price ([1996], pp. 47-48). On the other extreme, almost no one working on the philosophy of statistical mechanics seems to be aware that coarse-graining has, in general, nothing to do with thermodynamic irreversibility and is successfully applied to systems that do not and never will approach equilibrium. In any case, the example of the Boltzmann equation shows that it is important to get some clarity into this issue. In the following, I will attempt to do this in detail.

4 First Problem: The Source of Irreversibility in Thermodynamics

As discussed in section 2, entropy and irreversibility are studied in two fields of physics: Thermodynamics, which is a phenomenological theory of macroscopic states, and statistical mechanics, which describes macroscopic systems by developing a statistical description of its microscopic constituents. One of the aims of statistical mechanics - though by no means the only
one - is to provide a microscopic justification of the principles that are introduced as axioms on the level of thermodynamics. Here, we are interested in a particular property of thermodynamics, namely the existence of irreversible processes. If we wish to figure out where in the transition from microscopic to macroscopic physics these come into play, a good way to start is therefore to first figure out in which of the axioms of thermodynamics irreversibility can be found, and then to have a look at the microscopic foundations of that particular axiom.

Uffink (2001), pp. 313-315) attributes the difficulty of locating the temporal asymmetry in thermodynamics to the absence of equations of motion. A typical understanding of time-reversal symmetry is that a theory has this symmetry if, when we time-reverse a temporal evolution that is allowed by the theory, the resulting (backwards) evolution would also be allowed by the theory. It is not immediately clear how to apply this definition to a theory that is not primarily concerned with time evolutions.

Thermodynamics is based on four axioms (“Hauptsätze”), namely the zeroth law (transitivity of thermal equilibrium), the first law (conservation of energy), the second law (entropy cannot decrease) and the third law (a system cannot reach zero temperature). In physics textbooks, the second law typically gets credit for irreversibility. This law was stated in section 2 as “Entropy cannot decrease in a closed system”. Although this is a very common understanding, it is very difficult to state the content of the second law in a precise way since it exists in many different forms that are not fully equivalent and have differing connections to the arrow of time.

Uffink (2001), p. 306), who has analyzed this point in detail, has compared the interpretation of the second law to the interpretation of a work from Shakespeare. I will follow his treatment (Uffink [2001], pp. 315-319) and refer to it for a much more detailed discussion. The first point is a terminological one. A theory is time-reversal invariant if, whenever a process is allowed by the theory, its time-reversal is also allowed. Such processes are reversible.

5The adjective “time-reversal invariant” is used for both processes and theories. A process is time-reversal invariant if its time reversal is possible according to the theory (Luczak [2017], p. 247), a theory is time-reversal invariant if all processes allowed by the theory are.

“Time-reversal (non)invariant process” is one possible definition of “(ir)reversible process”.

10
variant and the process is said to be irreversible. In thermodynamics, however, “irreversible” is also used with other meanings. Often, a thermodynamic process is called “reversible” if it is quasi-static, i.e., if it is so slow that the system always remains very close to equilibrium. This idea of (ir)reversibility, employed in many early formulations of the second law, is, Uffink argues, not directly relevant for the arrow of time. For a second, more relevant interpretation of “irreversible” (which is not equivalent to time-reversal non-invariance), he proposes the term irrecoverable. A transition from a state \( A \) to a state \( B \) is irrecoverable if it cannot be fully undone once it has occurred. “Recovering” does not necessarily mean that the process that has led from \( A \) to \( B \) is exactly reversed step by step. It does however, mean that the transition is completely undone, including possible changes in the environment. Moreover, the recovery has to be possible in the actual world and not only in a model of the theory. Thus, irrecoverability and time-reversal non-invariance do not imply each other (see Uffink ([2001], pp. 317-318) for examples).

The relation of the second law to irreversibility then depends, as shown by Uffink ([2001]) in a detailed historical analysis, on its precise formulation. In an influential article, Brown and Uffink ([2001]) have argued that the temporal asymmetry of thermodynamics is more fundamental than and logically prior to the second law. Therefore, they propose a “minus first law” that should be added to the standard set of laws. It states that for each isolated system there exists a unique state of equilibrium that it will spontaneously enter. The time-asymmetry of thermodynamics (in contrast to statistical mechanics) then arises from the notion of equilibrium states, since these are spontaneously reached but not left without external intervention.

The distinction between the second and the minus first law, and the question where exactly irreversibility is to be located, is very important for providing a microscopic foundation of irreversible processes. At least, if we wish to find the microscopic origin of irreversibility, we need to first clarify whether we mean by “irreversibility” time-reversal-noninvariance or irrecoverability. This has been made clear in the discussion of this problem by Luczak ([2017]). He claims that minus first and the second law are logically independent statements. The minus first law predicts that systems initially in a nonequilibrium state will irreversibly approach an equilibrium state, where “irreversible” means “time-reversal-noninvariant”. The second law,
on the other hand, is concerned with transitions between equilibrium states, which can be irreversable in the sense of “making the initial state irrecoverable”. Statistical mechanics, he concludes, should aim at giving a microscopic underpinning to both the minus first and the second law, which requires two different solutions given that we have two different problems.

5 Second Problem: The Definition of “Equilibrium” and “Entropy”

If we wish to explain something, it is, in general, important to have a precise idea about what exactly the thing we need to explain is. In the case of thermodynamics, explaining the increase of entropy or the approach to equilibrium therefore requires that we have an idea about what precisely “entropy” and “equilibrium” are. As it turns out, this is not at all clear, which is problematic because different opinions on what equilibrium is lead to different positions about why and whether equilibrium is approached. In section 2 I have introduced some standard ideas: “Equilibrium is a state in which the system remains forever” and “equilibrium is a state in which the entropy reaches its maximum” (this reference is why we need to discuss entropy and equilibrium in one question). The first definition is extended in the minus first law to “unique state which a system spontaneously enters and remains in”. The second definition is commonly employed in equilibrium statistical mechanics in the determination of the equilibrium phase space distribution. These definitions are equivalent if we assume that the entropy of isolated systems always monotonously increases to its maximum value. Entropy has been introduced as a measure of the volume of the probability density, and thus comes in fine-grained and coarse-grained form, depending on whether we use the fine-grained or the coarse-grained density for its definition. This ambiguity then translates, if we define equilibrium in terms of entropy, in an ambiguity of what “equilibrium” means, which can make things very difficult. The definition of “entropy” is therefore not, as claimed by Price (2004, p. 5), a “pseudo-puzzle”, it has important implications for the further discussion.

In this section, I will first discuss how definitions based on fine-grained and coarse-grained entropy lead to different meanings of Q4. Then, I will present the various ambiguities involved in different types of definitions. Finally, I will briefly address the relation of microscopic and macroscopic definitions.
Let us start by considering the argument formulated by Blatt ([1959]) and Ridderbos and Redhead ([1998]) against the coarse-graining procedure, which is based on the famous spin echo experiment performed by Hahn ([1950]). Spins in a magnetic field are aligned by a strong pulse. The magnetic field causes precession of the spins, and because of small inhomogeneities in the field, the spins precess with different frequencies and are oriented “randomly” after some time. Hence, they are in a state of coarse-grained equilibrium. Now, a second pulse is applied, which reverses the direction of precession. In consequence, the spins return, after some time, to their initial aligned state.

Since the spins were isolated after the second pulse, we appear to have a case of an isolated system spontaneously evolving away from equilibrium, which could be interpreted as a violation of the second law. The explanation for why this happens and why this is not a violation of the second law, Ridderbos and Redhead ([1998], p. 1251) argue, is that the system was not actually in fine-grained equilibrium. It was in only in coarse-grained equilibrium. But since the typical argument for coarse-graining is that we cannot distinguish between fine-grained and coarse-grained equilibrium, and since we here have found a way in which we can, coarse-graining is not a reasonable way of explaining the approach to equilibrium. In particular, if we think of “equilibrium” as “coarse-grained equilibrium”, the second part of the spin-echo experiment would constitute a violation of the second law of thermodynamics (which, of course, it is not).

I will get back to the justification of coarse-graining in section 6. What is interesting here is that Ridderbos and Redhead defend a certain characterization of equilibrium as “fine-grained equilibrium”. They contrast their position with that of Sklar ([1995], p. 253), who argues that the first part of the spin-echo experiment constitutes “normal” thermodynamic behaviour. Ridderbos and Redhead ([1998], pp. 1254-1257), on the other hand, argue that thermodynamic behaviour would be an approach to “true” equilibrium, including the loss of correlations. On this perspective, an isolated gas that spreads out in a container would not exhibit an approach to equilibrium, since its distribution would not turn into the uniform equilibrium distribution.

*I explain it here in a simplified form, following Ridderbos and Redhead ([1998], pp. 1242-1243).
This, they conclude, requires external interventions.

But we do not necessarily need to use this definition of equilibrium. Maybe, we are perfectly happy with the weaker characterization of “equilibrium” as a state in which the values of certain macroscopic quantities are approximately constant (Callender [2001], p. 547). For example, we macroscopically observe that hot coffee cools down. When we, starting from a microscopic theory, derive a prediction for how the average temperature of the coffee evolves in time, we get, after some approximations, just that: The temperature of the coffee will approach room temperature.

These two definitions thus lead to very different meanings of Q4 (“Why do systems approach equilibrium?”). If we use fine-grained equilibrium as a definition, combined with the very strict reading proposed by Ridderbos and Redhead, then the explanation for why gas expands in an isolated container, or why an isolated cup of coffee cools down, would not be a part of Q4. Using a definition based on constant macroscopic quantities, on the other hand, we would seek for an explanation of precisely this behaviour.

We have, up to now, identified two general types of definitions. One characterizes “equilibrium” as the state with maximal (statistical) entropy, the other one by properties the system has on the thermodynamic level. Both definitions, however, require some additional clarification:

For the definitions in terms of “entropy”, we obviously need to specify what entropy is. This is a question to which a remarkable variety of answers can be given (see Frigg and Werndl [2011] for an overview). First, we need to specify whether we use fine-grained or coarse-grained entropy, which will then lead to a fine-grained or coarse-grained definition of “equilibrium”. Ridderbos [2002], pp. 76-77) uses the following argument against the coarse-graining procedure: We typically say that coarse-graining is allowed because the coarse-grained distribution gives the correct values for macroscopic observables. But for entropy, coarse-graining does not give the correct macroscopic value (since we employ it to get an entropy that can increase). Hence, coarse-graining does not succeed in giving thermodynamics a microscopic foundation.

This argument is based on a certain idea of what “the correct value for the entropy” is. If entropy is defined as a measure for the volume of the fine-grained distribution, then the coarse-
grained distribution gives the wrong entropy. But this is just a definition. We could also say that by “entropy”, we mean a measure for the volume of the coarse-grained distribution, and then the macroscopic values obtained via coarse-graining would - of course - be completely right. Thus, Ridderbos’ argument against coarse-graining - in our framework: her answer to Q3 - presupposes a certain answer to Q2, namely that entropy should be defined in terms of the fine-grained distribution.

Moreover, we need to specify whether we use a Gibbsian or a Boltzmannian definition of entropy. Most of the literature is focused on the Gibbsian picture, where entropy is a property of an ensemble. (This is also the viewpoint that was presented in section[2]) In the Boltzmannian approach, on the other hand, entropy is a measure for the number of microstates by which a certain macrostate can be realized, and thus a property of an individual system. Callender ([1999]) has suggested that the Boltzmann entropy might be relevant for reducing thermodynamics to statistical mechanics. Finally, it needs to be mentioned that the question what is meant by “entropy” for a nonequilibrium system is not clear and a subject of current research in physics. For close-to-equilibrium systems, local formulations of the first law have been used to calculate entropy production (Wittkowski et al. [2013]). For active matter (see section[6], defining and calculating the entropy is even more difficult (Nardini et al. [2017]).

Macroscopic definitions also come in various forms. One can characterize equilibrium as a state in which the system remains forever (Brown and Uffink [2001], p. 528) or as a state in which the values of macroscopic quantities are constant (Callender [2001], p. 547). These definitions are not equivalent, since it is, at least logically, possible that a system’s macroscopic quantities are constant only for a certain time, or that a system remains forever in a state of permanent oscillation. This also makes a difference for whether or not we have to add the attribute “irreversible” to “approach to equilibrium”. If we define “equilibrium” as a state which the system remains in forever, then thermodynamic irreversibility and approach to equilibrium essentially mean the same thing. If, however, our definition of equilibrium is set up in such a way that “being in equilibrium” does not entail “being in equilibrium forever”, then we need to discuss both why systems approach equilibrium and why (or whether) they never leave this state. In this case, the fourth question has two parts. Moreover, a definition of “equilibrium” in
terms of “constant values of the values of macroscopic observables” needs to clarify the status of “nonequilibrium steady states” (see, e.g., Lax ([1960]) or Knoch and Speck ([2015])), where a system (for example one that is constantly driven by external influences) is in a steady state without being in equilibrium (as defined in statistical mechanics).

Finally, the relation of microscopic and macroscopic definitions is a subtle problem. For example, if we define “equilibrium” as “the state with maximal fine-grained entropy”, then the minus first law is, due to Liouville’s theorem stating that the fine-grained entropy is constant, simply wrong. Brown and Uffink ([2001], p. 530) therefore distinguish in their discussion two different notions of equilibrium: In thermodynamics, the concept of equilibrium is introduced through the minus first law. Here, it is a state that the system spontaneously enters and then remains in. Therefore, equilibrium is by its nature a time-asymmetric notion. In statistical mechanics, on the other hand, equilibrium is the macrostate with the largest entropy or phase-space volume, i.e., the state that can be realized with the largest number of microscopic configurations. This definition is time-symmetric, since deviations are, in principle, possible in both directions of time.

Callender ([2001]), in his discussion of the relations between thermodynamics and statistical mechanics, argues that thermodynamics is taken “too seriously” if we demand that statistical mechanics strictly reproduces its statements. What we ought to explain is not a monotonous approach to equilibrium, which is forbidden by the underlying laws, but the observation that systems approach a quasi-stationary state and remain there for a very long time. Thus, equilibrium is not the strictly stationary distribution of equilibrium statistical mechanics, but a state in which certain macroscopic properties of a system are constant for a very long time. Thus, in the project of reduction, we should be happy if we can show statistical mechanics to approximately recover thermodynamics.

As an example for some of the problems mentioned in this section, I use a popular method of nonequilibrium statistical mechanics known as dynamical density functional theory (DDFT). Derived by Marini Bettolo Marconi and Tarazona ([1999]) (and many others), it is now considered a paradigmatic example of an irreversible dynamical theory describing the approach to equilibrium. More precisely: It describes the irreversible time-evolution of the one-body
density of a fluid towards the state in which the free energy, written as a functional of the one-body density, reaches its minimum. Thus, according to DDFT, the one-body density evolves towards the distribution it would have in thermodynamic equilibrium. However, since the fine-grained distribution does not only depend on the one-body density, DDFT does therefore, strictly speaking, describe an approach to coarse-grained equilibrium. This has physical consequences: If we start with a state that has the same one-body distribution as the equilibrium state, but a different correlation function, then DDFT will not predict that it relaxes. Problems of this form are relevant, e.g., for the glass transition (Heinrichs et al. [2004], p. 1124).

To summarize this section: There are two different ways of defining equilibrium in statistical mechanics through a maximum of entropy, namely by using the fine-grained or the coarse-grained entropy for the definition. These have different implications for what “approach to equilibrium” means and can lead to (apparent) conflicts in how it should be explained. Moreover, it is possible that there has to be a second definition in thermodynamics (which may or may not coincide with the one used in statistical mechanics).

6 Third Problem: The Justification of Coarse-Graining

The third problem is the question whether and how we can justify coarse-graining. It is an important point here that “Can we use coarse-graining to explain the approach to equilibrium?” and “How can we use coarse-graining to explain the approach to equilibrium?” are two very different questions. While the second one requires, of course, that the first one has been answered with “yes”, it has a different content.

A first point we need to make here is that, to not get things mixed up, it is important to distinguish “coarse-graining”, which is a mathematical technique, from the position of “the coarse-grainers”, which is the aims of criticism from interventionists. As mentioned above, I mean by “coarse-graining” any mathematical procedure in which the exact microscopic den-

\[ \text{In grand-canonical fine-grained equilibrium, one can prove that the one-body density does uniquely determine the fine-grained density. This is exploited in equilibrium density functional theory (Evans [1979], p. 148). However, there is an additional dependence on the initial condition for out-of-equilibrium systems (Chan and Finken [2005], pp. 1-2).} \]
sity is replaced by an averaged one. If, in the philosophical literature, someone criticises “the coarse-grainers”, the criticism is usually aimed at people who use a coarse-grained definition of equilibrium, typically combined with an epistemic justification based on finite measurement resolutions (Ridderbos and Redhead [1998]). This criticism, however, is not directed at the procedure of coarse-graining as such (at least it should not be), it is directed at a particular justification of coarse-graining or at a particular definition of “equilibrium”. The method of coarse-graining, in particular in the form of projection operators, can also be used to formally describe the influence of external interventions, and thus form a basis for interventionism (Robertson [2018], p. 10). Since “the position of the coarse-grainers” is just one possible justification of the mathematical procedure of coarse-graining, and since this mathematical procedure is used very successfully throughout physics, one should be very careful with rejecting it altogether, and if one does, this requires a very good justification.

To see why Q3 is a problem on its own, it is helpful to compare the way coarse-graining is used and discussed in physics and in philosophy. Philosophers are interested in coarse-graining because of the role it plays in the explanation of the approach to equilibrium. Physicists, on the other hand, also use it in situations where the approach to equilibrium is of no interest or not even expected. This can be illustrated using the example of active matter. Active particles are particles which use energy in order to create directed motion. A typical example would be a swimming bacterium. Systems consisting of active particles are permanently out of equilibrium, i.e., they do (as long as there is enough energy available) never reach or approach a state of thermodynamic equilibrium.

In the description of active particles in statistical mechanics, coarse-graining methods are frequently used to derive a macroscopic field theory based on a microscopic theory of the individual active particles (see, e.g., Bickmann and Wittkowski [2019]) for such a derivation). The theories derived in this way correctly describe the dynamics of active matter systems, which is why it is plausible to assume that the methods employed in such derivations are somehow justified. Nevertheless, for a philosopher who discusses the justification of coarse-graining exclusively in relation to the question whether coarse-graining (correctly) predicts the approach to equilibrium, this part of physics is difficult to understand.
Moreover, coarse-graining is also applied to systems of passive (i.e., not active) Brownian particles (Marini Bettolo Marconi and Tarazona [1999]). These particles are immersed in a fluid and move around governed by random fluctuations and friction resulting from the fluid. This is incorporated in the Langevin equations, which are the equations of motion for these particles. Due to the friction term, the Langevin equation is already asymmetric in time (Luczak [2016], p. 405), such that coarse-graining is not required here to provide irreversible dynamics. Nevertheless, it is required in order to get a practically useful description of those systems, since one cannot solve the Langevin equations for all particles.

Understanding that coarse-graining as a method is not always connected to the approach to equilibrium (without denying, of course, that it does also play a role there) allows us to get a clearer understanding of both arguments against and justifications of coarse-graining. We start with the former: A typical argument against coarse-graining is that the asymmetry arising from coarse-graining is merely a consequence of our ignorance of microscopic details and so is illusory and/or anthropocentric (Robertson [2018], pp. 14-19). Various things need to be clarified regarding this objection. First, there is no such thing as an asymmetry resulting from coarse-graining, since coarse-graining can be done both forwards and backwards in time. Coarse-graining is used in the explanation for the approach to equilibrium - this is an answer to Q4 - but not in the explanation for the temporal asymmetry - an answer to Q5, which is a different point. This will be discussed in more detail in sections 7 and 8. A better way of analyzing this objection is that the approach to equilibrium predicted by coarse-graining is illusory. Whether this is a reasonable position is a matter of debate, corresponding to the discussion of Q2 and Q4. What is important here is that the illusory-objection moves from the premise “Coarse-graining does not actually describe the approach to equilibrium” to the conclusion “Coarse-graining is a procedure that should not be used”, which is only possible if we assume that a successful explanation of the approach to equilibrium is a necessary condition for coarse-graining to be

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Physically, the Langevin equations can themselves be derived by coarse-graining the exact reversible dynamics of solvent and colloidal particles. However, theoretical studies of Brownian particles usually take the Langevin equations as fundamental and then coarse-grain them for practical purposes.
justified. However, if coarse-graining is also used in situations which have nothing to do with an approach to equilibrium, but only with the correct prediction of macroscopic quantities, then equilibrium cannot be the central criterion for whether it is justified.

This can also be made clear in the context of spins: The relaxation of spins towards an equilibrium magnetization can be described using the Bloch equations, which can, as shown by Kivelson and Ogan ([1974], pp. 105-109) be derived using a popular coarse-graining method known as the Mori-Zwanzig formalism (Mori [1965]). In this derivation, an approach to equilibrium is correctly predicted based on coarse-graining. Now let us assume that the spins are subject to a time-dependent external magnetic field, which means that they will not approach equilibrium. For this case, a more general equation has also been derived using the Mori-Zwanzig formalism by te Vrugt and Wittkowski ([2019a]). If the prediction of equilibration is the criterion for the justification of coarse-graining, then the derivation for spins without time-dependent magnetic fields by Kivelson and Ogan ([1974]) would be justified, whereas the derivation by te Vrugt and Wittkowski ([2019a]) with magnetic fields would not be. Given that both derivations give empirically adequate results, and given that the results with time-dependent fields contain the results without time-dependent fields as a limiting case, this is not a very plausible position. Positions that reject coarse-graining because it delivers a coarse-grained rather than a fine-grained equilibrium essentially do not distinguish between Q3 and Q4.

We now turn to justifications of coarse-graining. Robertson ([2018], p. 15) has suggested to split the project of justifying coarse-graining into two further sub-problems: We need to justify both why we coarse-grain at all and why we coarse-grain in a particular way. The answers to these questions might be linked, but do not have to be identical. For the purpose of this article, which is to bring structure into the overall debate, these can both be subsumed under Q3. It should, however, be mentioned here that they might have to be distinguished when one develops a more detailed answer.

The justification of coarse-graining can then proceed in various ways. One option is to justify the replacement of the fine-grained by a coarse-grained density by our ignorance about microscopic details. Robertson ([2018], pp. 19-22) justifies coarse-graining by its ability to
reveal autonomous dynamics on a higher level of description. Notably, both justifications do not require a connection between coarse-graining and equilibrium and are thus applicable more generally. We can also be indifferent about microscopic details of an active system, and active systems also show autonomous dynamics on higher levels of description.

Again, we use as an example the DDFT method introduced in section 5. It can be derived by coarse-graining the reversible microscopic dynamics, for which the irreversible DDFT equation is an approximation (Español and Löwen [2009]). However, a DDFT has also been derived by Dean ([1996]) as an exact theory for a system of Brownian particles, where the underlying dynamics is itself irreversible. Moreover, DDFT methods have also been developed for active matter (Wittkowski and Löwen [2011]) or disease spreading (te Vrugt et al. [2020]), both of which are fields not primarily concerned with the approach to equilibrium. Hence, coarse-graining is not solely responsible for the irreversibility of DDFT.

Finally, we need to clarify how Q3 would have to be answered by philosophers who do not consider coarse-graining a valid method of reasoning. This depends on what precisely they oppose. If they only reject a certain justification - such as “the position of the coarse-grainers” - they need to find a different one which does not have the properties they consider to be undesirable (such as an appeal to human observers). A much stronger position would be to reject the mathematical procedure of coarse-graining altogether. In this case, one would have to come up with a very good argument for why coarse-graining is not justified, given that it is successfully applied by thousands of physicists for deriving theories that show excellent agreement with experiment. Moreover, one should have a good explanation for why this method works if it is not justified. In any case, it is not possible to answer Q3 by simply answering “Can coarse-graining be justified?” with “No”.

7 Fourth Problem: The (Irreversible) Approach to Equilibrium

While the first three problems have been setting the stage, we now dive more deeply into what is usually thought of as “the problem of irreversibility”: We wish to explain why systems that are initially out of equilibrium tend to move towards an equilibrium state.

In physics, derivations of irreversible equations from the reversible microscopic physics
have a long tradition, starting from Boltzmann’s H-theorem discussed in section 3. “H-theorem” is, in fact, now a standard name for a proof that a certain theory leads to a monotonous behaviour of (for example) the entropy (see Anero and Español (2007) or Español and Löwen (2009) for examples). Here is a typical idea of how this can work (North [2011], pp. 323-326): As discussed above, the entropy is, in statistical mechanics, introduced as a measure for the volume in phase space. Equilibrium is the macrostate with the largest phase space volume. In fact, the phase space volume of the equilibrium state is overwhelmingly larger than that of other states. Hence, if we move into some direction in phase space, it is far more likely that we are going towards equilibrium than away from it (the latter result would correspond to extremely special initial conditions). The approach to equilibrium is therefore explained by statistical considerations. This argument goes back to Boltzmann, and notably, it is a probabilistic argument (Brown et al. [2009], pp. 185-187).

Today, a popular framework for the description of irreversible processes is stochastic dynamics (Uffink [2006], pp. 1037-1062). Here, the laws governing the behaviour of the constituents of a system are assumed not to be deterministic, but stochastic. In particular, a specific type of stochastic processes known as Markov processes is employed. A process is Markovian if it has no memory. This means that (the probability of) what happens in the future does not depend on what has happened in the past. For example, the probability for getting “heads” in a fair coin toss is always 0.5, no matter how often the coin landed on “heads” in previous coin tosses. The Markov processes employed in stochastic dynamics have the attractive feature that they tend towards an equilibrium distribution after a while, so if we describe a system in statistical mechanics by Markovian dynamical equations, then chances are good that we will obtain a description of the irreversible approach to equilibrium. (Mathematically, the description in terms of Markov processes corresponds to using so-called “Master equations”, which can be obtained as an approximation of the actual microscopic dynamics within the Mori-Zwanzig formalism (Zeh [1989], pp. 48-57).)

The question is, of course, why we should be justified to model a system using stochastic

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9I assume here, for the sake of the argument, that these are acceptable definitions of “entropy” and “equilibrium”.
Markov processes given that the actual dynamics is governed by Hamilton’s equations, which are deterministic. Three main viewpoints can be found in the literature (Uffink [2006], pp. 1037-1062). The first one is an appeal to coarse-graining: We ignore microscopic details of the system that we cannot measure anyway and focus on macrostates. Then, we can find transition probabilities for the change from one macrostate to another one and thus obtain a stochastic process. Another option is interventionism: The system we describe is not actually closed, but subject to external influences. Since these are not known, our description of the system is stochastic. Finally, some authors have advocated being agnostic about the origin of stochasticity and simply treating stochastic dynamics as fundamental.

Since a description of the system in terms of Markovian stochastic dynamics would predict an irreversible approach to equilibrium, the various approaches correspond to various ways of answering Q4. In analyzing them, one again needs to carefully distinguish between the various questions. Superficially, it seems as if coarse-graining and interventionism are two different ways of answering Q4. However, as discussed in section 5 it is more precise to see them as different ways of asking Q4 by answering Q2 in a different way. This then, of course, also affects what the answer to Q4 looks like. The two approaches can be analyzed as

- Think of “equilibrium” as “coarse-grained equilibrium” (answer to Q2) and then explain the approach to equilibrium by the spreading of the coarse-grained density on phase space (answer to Q4).

- Think of “equilibrium” as “fine-grained equilibrium” (answer to Q2) and then explain the approach to equilibrium by the influence of the environment (answer to Q4).

Importantly, these approaches are not in contradiction: Recall that on the interventionist’s definition, the facts that gas in an isolated container spreads out, that a hot cup of coffee in an isolated room acquires room temperature, or that the spins go out of phase in the first part of the spin echo experiment do not constitute instances of an approach to equilibrium. Nevertheless, they do not deny that these things happen. Now let us have a look at how these effects are explained in the physics literature, where coarse-graining is employed. An illustration used by Donev et al. ([2014], p. 14) is a system consisting of many oscillators that, after some
time, will go out of phase. Every individual oscillator follows a time-reversible law, yet the average amplitude will decay due to dephasing. This, remarkably, is exactly the explanation that Ridderbos and Redhead ([1998], p. 1242) use for what happens in the first part of the spin-echo experiment - the spins oscillate and go out of phase.

Thus, there is no difference between the physical mechanisms by which interventionists explain the spread out of an isolated gas and the mechanisms by which a physicist using coarse-graining would explain the same effect. The major difference is that for an interventionist, “approach to equilibrium” means something different, such that different mechanisms (namely external interventions) are required to explain it. Therefore, interventionists can still explain the approach to a state of coarse-grained equilibrium based on phase-space considerations, they will just not classify this as an “approach to equilibrium”.

As in sections 5 and 6, we can consider the example of DDFT. When deriving it from the reversible laws of classical mechanics, as done by Español and Löwen ([2009]) using the Mori-Zwanzig formalism, one has to make a so-called Markovian approximation, which here is the assumption that the rate of change of the one-body density does not depend on the state of the system at previous times. This assumption allows to derive a H-theorem.

An argument against the idea of stochastic dynamics is that it cannot explain the thermodynamic asymmetry since the notion of a Markov process is time-symmetric (Uffink [2006], pp. 1049-1053). Similarly, Callender ([2016]), in his review of the debate around irreversibility, discusses interventionism as a possible solution to the problem of the direction of time alongside the past hypothesis and mentions as a criticism that the environment is itself governed by time-symmetric laws. Again, these objections are a result of a lack of distinction between the various questions: Interventionism, if it is used as a justification of the idea of stochastic dynamics, is a possible answer to Q4. Nevertheless, it does indeed not by itself provide a mechanism for the breaking of the temporal symmetry of classical mechanics. For this reason, it is inaccurate to present the past hypothesis and interventionism as alternative answers to the same problem, since the past hypothesis, unlike interventionism, is mostly aiming at Q5 (Shenker [2000], pp. 9-10).
8 Fifth Problem: The Arrow of Time

We have now reached the final problem, which is concerned with the *arrow of time*. Let us assume we have found some explanation for why, if we go forwards in time, the systems are observed to approach equilibrium, i.e., that we have answered question four. We now need to find out why, despite the symmetry of the fundamental laws of physics, this answer cannot be applied to the past.

The distinction between Q4 and Q5 allows us to understand why, as mentioned in section 3, philosophers concerned with the origin of irreversibility are discussing it based on considerations about the big bang and cosmology, while physicists doing nonequilibrium statistical mechanics are successful without ever spending a thought on these issues (Penrose [1994]). The methods employed in statistical physics - coarse-graining and Markovian approximations - allow for a solution of Q4. This is sufficient if one takes the temporal asymmetry as given, which is why the early universe plays no role in the everyday life of a statistical physicist. Philosophers (and physicists) who are interested in the source of this asymmetry, on the other hand, need to address Q5, which requires a different type of answer.

Recall how the standard solution discussed in section 2 works: We start from a reversible theory describing the dynamics of the density and then perform a coarse-graining, in which we replace the fine-grained by the coarse-grained density, allowing to define a coarse-grained entropy. The dynamics of the coarse-grained density then leads to an increase of the coarse-grained entropy. Unfortunately, the original microdynamics we started with was time-reversal symmetric, i.e., it does not know a difference between past and future. Hence, we could have simply decided to perform the coarse-graining in the other direction of time, which would have been mathematically just as well justified as the original derivation. This “backwards coarse-graining” would then lead to a theory in which the entropy decreases in the future and increases in the past, in wild contradiction to every observation (Wallace [2011], p. 15).

Albert ([2000], pp. 115-119) has explained this problem as a problem of inference: Q4 is, as discussed in section 7, frequently solved by arguing that an evolution towards equilibrium is more likely on statistical grounds. Albert approaches this from an epistemological perspective: In principle, he argues, we have two ways of obtaining information. *Prediction* means
that we, from the known laws of physics and the present state of the world as an initial condition, infer what will happen in the future. Doing the same for the past is called retrodiction. The alternative is to use records, from which we obtain most of our knowledge about the past. The question is now, Albert argues, why we should have sources of information other than prediction or retrodiction given that we only have direct reliable empirical information about the present state of the world. Unfortunately, if we use classical mechanics combined with statistical considerations about phase space to retrodict what happened in the past based on our knowledge of the present, we will conclude - by the line of argument employed in section 7 - that the past had a higher entropy than the present. Hence, most of our records would most likely be wrong. Even worse, given that we only believe in the laws of mechanics because of experiments that we have records of, we could infer from classical mechanics that classical mechanics is, presumably, also wrong. Thus, statistical mechanics has brought us into a position of scepticism.

To avoid this, he introduces an additional postulate, the past hypothesis: The entropy of the initial state of the universe was very low. If we then use, as a basis for our inference, not only the laws of mechanics + statistics of phase space, but in addition also the past hypothesis, then we can believe that our records of the past are, in general true. Thus, we have avoided Albert’s problem of scepticism. Moreover, what is important for us here, we can recover the laws of thermodynamics. We can also solve the problem posed by Uffink: When solving our stochastic equations of motion, we need to take into account a boundary condition. If this boundary condition is provided by a low-entropy initial state of the universe, as demanded by the past hypothesis, we have an explanation for why we can only get an increase of entropy through stochastic dynamics in the future, since only this is in agreement with the boundary condition.

Wallace ([2011]) has suggested an alternative formulation, the simple past hypothesis: He defends the position that coarse-graining is justified if the density is forwards compatible, i.e., if it does not affect the predictions for the macroscopic variables. This is satisfied if the density is “simple” [11]. Unfortunately, a simple distribution is also backwards compatible, i.e., we can

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[7] See Brown ([2017]) for a comparison of Albert’s and Wallace’s formulation.
[11] See Wallace ([2011]) for a more mathematical discussion.
perform a coarse-graining in the other direction of time, predicting an (unphysical) increase of entropy in the past. To avoid this, and therefore to answer Q5, we assume that the the initial state of the universe was simple. In this case, we cannot perform a problematic backwards coarse-graining, since the initial state of the universe has no past.

The past hypothesis is, however, not without criticism. There are concerns about whether it is a well-defined statement in general relativity (Earman [2006]). Moreover, it has been questioned whether an explanation of the entropy increase of the universe as a whole also gives a basis for an explanation of the observed entropy increase of its subsystems (Winsberg [2004]).

An alternative position is to interpret Q5 as an indication of the borders of statistical mechanics. Leeds ([2003]) has argued that we should see statistical mechanics as an instrument that allows us to make successful predictions, but that does not make statements about the past. See North ([2011]) for an overview over positions of this type. (We could ask here whether this is actually a solution to the problem, given that it is still an interesting question why statistical mechanics is restricted to statements about the future. And, after all, the temporal asymmetry seems to be a property of the world around us rather than just a property of our theories of them. If statistical mechanics does not give us this asymmetry, something else should.)

Once again, DDFT provides an interesting example. As mentioned in section[7] it is possible in the Mori-Zwanzig formalism to derive the irreversible DDFT equation from the reversible laws of classical mechanics, a theory where the entropy always increases. However, this only answers Q4 - depending on the choice of initial conditions, it is also possible in the Mori-Zwanzig formalism to derive a theory in which entropy always decreases (Zeh [1989], p. 56). Notably, a “backwards coarse-grained theory” is sometimes actually employed in statistical physics (Lutsko [2019]): In the theory of nucleation, where particles in a fluid aggregate to a nucleus that then grows leading to solidification, one is interested in calculating the “most likely path” along which nucleation occurs. For nucleation, the system has to overcome an energy barrier. The most likely path can be obtained by starting at the top of the energy barrier, evolving the system forwards in time by the (coarse-grained and irreversible) DDFT equation towards the final state and backwards in time (!) towards a the initial state which had a lower free energy. Lutsko ([2011], p. 2), who introduced this procedure, emphasizes that it is only
a mathematical trick. Nevertheless, it is an interesting observation that this is mathematically possible and occasionally useful, and it is an interesting question why this is only a mathematical trick.

9 Conclusion

In summary, I have shown that “the problem of irreversibility” consists of five different problems - irreversibility in thermodynamics, definitions of “entropy” and “equilibrium”, justification of coarse-graining, approach to equilibrium and arrow of time - which need to be distinguished to allow for a careful analysis of the problem. Not distinguishing them can lead to problems. For example, most derivations of H-theorems in physics answer only Q4, while leaving unanswered Q5. Moreover, interventionists and “coarse-grainers” employ different definitions of “equilibrium”, which has the consequence that the Q4 has different meanings for them. The resulting answers then are, of course, different, but not necessarily in conflict. And finally, coarse-graining is typically discussed in philosophy exclusively as a method to explain the approach to equilibrium, which does not reflect its much broader use in physics. For a satisfactory solution of “the problem”, one therefore needs to answer each question separately, with a terminology that is chosen carefully and in contact with modern research in nonequilibrium statistical mechanics.
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