Poincaré, Poincaré Recurrence, and the H-Theorem: A Continued Reassessment of Boltzmannian Statistical Mechanics

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Abstract: In (Weaver 2021), I showed that Boltzmann's H-theorem does not face a significant threat from the reversibility paradox. I argue that my defense of the H-theorem against that paradox can be used yet again for the purposes of resolving the recurrence paradox without having to endorse heavy-duty statistical assumptions outside of the hypothesis of molecular chaos. As in (Weaver 2021), lessons from the history and foundations of physics reveal precisely how such resolution is achieved.

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1 Introduction

Ludwig Boltzmann's (1844-1906) H-theorem entails that closed monatomic gas systems remain in thermodynamic equilibrium or else always increase in entropy until they reach thermodynamic equilibrium. The H-theorem is therefore "a demonstration of the second law of thermodynamics"[1] in a limited domain. In the early history of statistical mechanics, two important objections to Boltzmann's attempt to explain the truth of the second law of thermodynamics by appeal to the H-theorem were proffered. The first was called the **reversibility objection**. It says that the dynamical laws that govern the punctiform constituents of gases are time-reversal invariant. The performance of a time-reversal operation on dynamical laws of motion can yield a solution that describes an evolution that entails decreasing entropy over minus-time. Such a rewound evolution contradicts the H-theorem and thereby creates the **reversibility paradox**. The second objection used Henri Poincaré's (1854-1912) recurrence theorem resulting in the creation of the so-called **recurrence paradox**.[2] Poincaré's reasoning (it is thought) entails that for conservative classical systems confined to some finite spatial region and that start in some initial states, over time those systems will evolve and end up returning to their initial states (or arbitrarily close to their initial states) infinitely many times.[3] Eventually, the recurrence theorem was appropriated by both Poincaré and Ernst Zermelo (1871-1953) in attempts to show that appropriate non-equilibrium gas systems do not inevitably evolve toward equilibrium and stay there permanently. Rather, they will inevitably head back to their initial (lower entropy) states. As a result, some became convinced that the recurrence theorem posed a problem for both the H-theorem and non-statistical expressions of the second law of thermodynamics.

Hendrik A. Lorentz (1853-1928) and others pointed out that proof of the H-theorem rests upon an assumption, *viz*., what became known as the hypothesis of molecular chaos (**HMC**).[4] Roughly put, the **HMC** states that with respect to constituents of gas systems such as those to which the H-theorem was thought to be applicable, pre-collision velocities of those constituents are uncorrelated while post-collision velocities become correlated *because* of collisions.[5] I have recently argued (in Weaver 2021) that this hypothesis should be understood as an interpretive time-asymmetric one about causation in the dynamics of collisions. He, I believe, convincingly shows that such an interpretive maneuver resolves the reversibility paradox (*qq.v.*, **sect. 2** and **sect. 3**). Discussions of the recurrence theorem and recurrence paradox authored by those working on foundations of statistical mechanics rarely mention that Poincaré's original intention behind the articulation and proof of the recurrence theorem was to demonstrate the stability of the orbits of planets. I believe that if one takes on board the causal interpretation of the **HMC** (in Weaver 2021) after appreciating the role Poincaré's recurrence theorem plays in Poincaré's work on celestial mechanics, the solution of the recurrence paradox all but reveals itself. Surprisingly, the resulting resolution does not require the endorsement of any statistical or probabilistic considerations other than the **HMC**.

[1] (Gressman and Strain 2011, 2351).

[2] For the best scientific biographies of Poincaré, see (Gray 2013) and (Verhulst 2012).

[3] See **sect. 4.3.3** for a precise statement of the recurrence theorem as it was supplied in its original context.

[4] See (Darrigol 2018, 323). On Lorentz and the development of kinetic theory, see (Kox 1982, 1990).

[5] If you ardently insist on rejecting this characterization of the **HMC**, I'd like to point out that the remark in the main text is historical in nature and that a defense of something close to this statement is beyond the scope of the current paper. Importantly, Boltzmann did ascribe to the **HMC** as it is (roughly) characterized here. See (Boltzmann 1964, 42). See also (ibid., 58-59); (Boltzmann 1895); (Cercignani 1998, 259); and (Kuhn 1978, 64).

Let's begin with a precise statement of the H-theorem and some motivation for ushering it back into a place of prominence within Boltzmannian statistical mechanics.

2 The H-Theorem

2.1 A Historically Sensitive Statement

In 1872, and then again in 1875, Boltzmann attempted to prove what has become known as the H-theorem (then called the minimum theorem).[6] At the time, Boltzmann was concerned with showing that the equilibrium velocity distribution function $f(\mathbf{v})$ for a classical monatomic gas system (*e.g.*, the noble gases helium (**He**), neon (**Ne**), argon (**Ar**), or krypton (**Kr**)) is the Maxwell distribution introduced by James Clerk Maxwell (1831-1879) in (1860a, 1860b, 1867):

(Eq) 1:

$$f(\mathbf{v}) = n(\frac{m}{2\pi kT})^{3/2}\, e^{(-\frac{m(\mathbf{v}-\bar{\mathbf{v}})^2}{2kT})}$$

where e is Euler's number, $f(\mathbf{v}) = f(v_x, v_y, v_z)$, n is the number density of the gas system, m is inertial mass, T gives the absolute temperature of the gas, and k is an experimentally determined (later called the Boltzmann) constant.[7] For monatomic gases, functions such as $f(\mathbf{r}, \mathbf{v}, t)d^3\mathbf{r}d^{\mathbf{3}}\mathbf{v}$ provide one with the probability (at time t) that a constituent of the gas system is in the space volume element around (centered on) $\mathbf{r}$ and velocity volume element around (centered on) $\mathbf{v}$ in a higher-dimensional geometric space.

The Maxwell distribution is asymptotically Gaussian. It can be shown to satisfy the following relation (where velocities $\mathbf{u}_1$ and $\mathbf{u}_2$ are final (post-collision) velocities, and $\mathbf{v}_1$ and $\mathbf{v}_2$ are initial (pre-collision) velocities):

(Eq) 2:

$$f(\mathbf{v}_1)f(\mathbf{v}_2) = f(\mathbf{u}_1)f(\mathbf{u}_2)$$

By way of the experimentation of Nobel laureate Otto Stern (1888-1969), (Eq. 1) was shown to be the approximately correct distribution function for constituents of appropriate rarefied

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[6] (Boltzmann 1872); (Boltzmann 1875); *cf*. (Boltzmann 1964, 49-55). See also (Darrigol 2018); (Segrè 1984, 278-279); (Spohn 2001); (Uffink 2007, 2017); and (Weaver 2021) for more on these papers. On the important contributions of Maxwell, see (Garber et. al. 1995); (Robson et. al. 2017) and the primary and secondary literature cited in (Weaver 2021, **sect. 2**).

[7] When the classical system is more complicated featuring polyatomic gas molecules as with ammonium ion ($\mathbf{NH_4^+}$), nitrite ($\mathbf{NO_2^-}$), or chlorite ($\mathbf{ClO_2^-}$), the distribution function should be the Maxwell-Boltzmann distribution stated here as a function of energy:

$$f(E) = \frac{1}{Ae^{E/kT}}$$

where A is the normalization constant.

gas systems in thermodynamic equilibrium.[8] To establish the uniqueness of the Maxwell equilibrium distribution, Boltzmann specified the H-functional (initially the E-functional) as follows (in modern notation):

(Eq) 3:

$$H \equiv \int f\, log\, f\, d\mathbf{v}$$

The distribution function was said to satisfy what has become known as the Boltzmann equation (first introduced in 1872 although expressed here in modern notation using a bilinear (in form) quadratic Boltzmann collision operator Q):

(Eq) 4a:

$$\frac{df(t)}{dt} + v \cdot \nabla_r f(t) = Q\big(f(t), f(t)\big)$$

where $v \in \mathbb{R}^d$, $\Omega \subset \mathbb{R}^d$ giving the spatial domain such that d is greater than or equal to 2, and $r \in \Omega$. Or if you prefer to forsake the collision operator and allow for an external influence (in 3D):

(Eq) 4b:

$$\frac{\partial f}{\partial t} + \mathbf{a} \cdot \boldsymbol{\nabla}_{\mathbf{v}} f + \mathbf{v} \cdot \boldsymbol{\nabla}_{\mathbf{r}} f = \int d\mathbf{v}_2 \int \{f(\mathbf{u}_1) f(\mathbf{u}_2) - f(\mathbf{v}_1) f(\mathbf{v}_2)\}\, d\Omega v \sigma(v, \theta)$$

taking the partial derivatives on the subscripts of the gradients, (again) allowing for the influence of an external conservative force acting on our system resulting in the existence of a potential connected with $\mathbf{a}$ by $\mathbf{a} = -\nabla_{\mathbf{r}} \frac{U}{m}$ (for the individual classical particle with inertial mass m). v here gives the pre-collision magnitude of the relative velocity of the particles involved in the binary collision. $d\Omega$ gives the differential solid angle element that includes the post-collision relative velocity of the colliding particles, and $\sigma(v, \theta)$ is the differential collision cross section relevant to those binary collisions that yield a scattering angle θ relative to an impact parameter.

One additional expression closer to the work of Boltzmann can be useful:

(Eq) 4c:

$$\frac{\partial f}{\partial t} = \int d\mathbf{v}_2 \int \{f(\mathbf{u}_1) f(\mathbf{u}_2) - f(\mathbf{v}_1) f(\mathbf{v}_2)\} |\mathbf{v}_1 - \mathbf{v}_2|\; d\Omega \sigma(\Omega)$$

[8] See (Stern 1946, 9-10 although the method presented on page 9 is described as “not very accurate”, the second method on page 10 includes no such qualification). The history is told by (Holton and Brush 2006, 322-326; “In addition to confirming the shape of the velocity-distribution curve predicted by Maxwell, Stern’s experiment also showed…” ibid., 326. At ibid., 324. n. 5, these authors describe the Maxwell distribution as “now well-proved”).

if $d\Omega\sigma(\Omega)$ is the differential collision cross section "for a collision in which the relative velocity" after the collision is "in the solid angle $d\Omega$ at Ω compared to the relative velocity before."[9]

None of these versions of the Boltzmann equation are time-reversal invariant. This fact is often associated with the further fact that for collisions amongst the particle constituents of the gas, the **HMC** holds (Villani 2006, 784-785). You see this in Boltzmann's efforts to show that (Eq. 5) below holds (Boltzmann 1964, 42). He tried as best he could to prove that *if* (a) the distribution function satisfies the Boltzmann equation and (b) the Boltzmann equation is omnitemporally true or applicable to the system under evaluation, *then*:

(Inequality) 5:

$$\frac{dH}{dt} \leq 0, for\ any\ time\ t$$

Of course, the result he was after just is the H-theorem (Huang 1987, 74). As a desired bonus, all of this entails that the full time-derivative of the H-functional vanishes if, and only if, the distribution function is Maxwellian.

2.2 The Disappearance of the H-Theorem

Modern philosophers, physicists, and mathematicians have taken up an approach to modern statistical mechanics that makes much of Boltzmann's combinatorial outlook first promulgated in (Boltzmann 1877). This modern Boltzmannian statistical mechanics (**MBSM**) forsakes the H-theorem and seeks to save the phenomena and solve many of the most important puzzles of statistical mechanics with some complex combination of the following theses (where (a), (b), (d-ii), and (e) are *essential* components of **MBSM**): (a) the combinatorial statement of the Boltzmann entropy:

(Eq) 6:

$$S_B(X) = k \log vol\, \Gamma(X)$$

which asserts that the Boltzmann entropy of macrostate X (or $S_B(X)$) is equal to Boltzmann's constant multiplied by the natural logarithm of the volume of the phase space region for macrostate X[10]; (b) the dynamical laws (plus various auxiliary principles such as Liouville's theorem, *inter alia*); (c) the past hypothesis, (d-i) the statistical postulate, (d-ii) the standard Lebesgue—Liouville

[9] (Klein 1970, 101). For more on the Boltzmann equation, see (Kremer 2010); (Villani 2002, 2008, 2006); and I add (Segrè 1984, 278-279) for beginners.

[10] Or more technically, the $vol\, \Gamma(X)$ term gives the volume of the phase space region representing macrostate X, but it is determined by the Lebesgue—Liouville measure projection onto the energy hypersurface of the phase space (assuming that total energy remains constant over time so that you can work with the $6N - 1$ energy surface of the phase space (or a thin shell around that surface) and not the full $6N$-dimensional phase space).

I resist calling (Eq. 6) or its close cousin $S = k \log W$, "Boltzmann's principle" or "Boltzmann's law" because Boltzmann did not state this principle. Max Planck (1858-1947) did (Duncan and Janssen 2019, 49; 94); (Kragh 1999, 61). So far as I'm aware, Albert Einstein (1879-1955) was one of the earliest scholars to call the stated entropy formula the "Boltzmann principle" in 1905 (Einstein 1989, 86-103).

measure of statistical mechanics[11], and (e) the probabilistic version of the second law of thermodynamics.[12]

Part (b) includes things like Hamilton's equations of motion along with whatever else may be needed to facilitate the use of those equations (*e.g.*, symplectic geometry and the necessary higher-dimensional space(s)). Part (c) says that the universe began in an exceedingly low entropy macrostate. Part (d-i) says that there exists a law of nature that gives a uniform probability distribution over the microstates that realize the initial low entropy macrostate referenced in part (c). For part (d-ii), see note 11, but the gist is that the standard measure enables one to make sense of larger and smaller volumes in coarse-grained regions of the 6*N*-dimensional phase space used to model the choice system(s) despite the fact that each region features infinitely many points indicative of possible states of the physical system being modeled. Part (e) just says that the most likely evolution of macroscopic systems is one that heads toward thermodynamic equilibrium or else they will remain in thermodynamic equilibrium.

I have detected within this movement a major influence by a prominent, although ultimately inaccurate story told by the eminent historian of physics Martin Klein (Mechanical 1973, 63; Development 1973) (*inter alios*), a story about the place of the H-theorem in the development of Boltzmann's thought.[13] This standard story says, roughly, that Boltzmann abandoned the H-theorem and so also a statistical mechanics weighed down by it in light of the reversibility objection discussed in **sect. 3** and/or the recurrence objection discussed in **sect. 5** below.

Perhaps it is unsurprising then to find that the H-theorem plays no essential role in the work of *much* contemporary statistical mechanics in general and **MBSM** in particular. Three recent (otherwise very good) textbooks on statistical mechanics say nothing about the H-theorem, *viz*., (Laurendeau 2005), (Peliti 2003), and (Sethna 2021). Frigg and Werndl (2019) present Boltzmannian statistical mechanics and never once mention the H-theorem. In addition, Frigg and Werndl's "Entropy: A Guide to the Perplexed" states that the "H-*Theorem*…is generally regarded as problematic" (2011, 123 emphasis in the original) citing (Uffink 2007, 962-974) which at (ibid., 967-968) emphasizes Boltzmann's abandonment of the H-theorem in light of Loschmidt's reversibility objection.[14] This is an element of the erroneous standard story.

The Boltzmannians themselves show almost no interest in the theorem. In (Albert 2000), the H-theorem is mentioned only once (and there it is erroneously said that Boltzmann proved the theorem "rigorously" (ibid., 55)). The eminent physicist Joel L. Lebowitz (1999) authored an important review of statistical mechanics and never mentions the H-theorem. The same can be said about the (also eminent) mathematician Sheldon Goldstein (2001) in his discussion of

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[11] On this feature, see (Callender 2011, 88).

[12] Among some of the most notable practitioners or defenders of **MBSM** we may include: (Albert 2000, 2015); (Callender 2011); (Carroll 2010); (Chakraborti et. al. 2021); (Fermi 1956); (Goldstein and Lebowitz 2004); (Goldstein et. al. 2017a); (Goldstein et. al. 2017b); (Goldstein et. al. 2019); (Goldstein et. al. 2020); (Lebowitz 1993a, 1993b, 1999, 2021); (Loewer 2012, 2020); (Penrose 2004, 686-712), and a host of others.

[13] The story's inaccuracy is proven so by (Badino 2011), (Darrigol 2018, 2021), (Kuhn 1978), (von Plato 1994), and (Weaver 2021), with (Darrigol 2021) even correcting some of the standard translations of the relevant primary literature. These five authors do not always see eye-to-eye on where precisely the standard story goes wrong.

That the story is standard is supported at (Weaver 2021, sect. 1).

[14] The other source referenced is (Emch and Liu 2002 92-105), which, in my humble opinion, hardly counts as a serious historical investigation despite its insightful and brilliant non-historical remarks about the Boltzmann equation (the title of the section).

"Boltzmann's Approach to Statistical Mechanics".[15] Callender (2011, 86-87) discusses the H-theorem but cites as his choice authority on the background history (Brown, Myrvold and Uffink 2009) whom at (ibid., 185, 187) buy into that part of the standard story that emphasizes the abandonment of the H-theorem due to Loschmidt's reversibility objection. You can also see in Callender (2011, 86-87) itself an indication that Boltzmann's H-theorem was rightly seen by his contemporaries to be problematic because the mechanics on which it leans is "quasi-periodic and time-reversal invariant" (ibid., 87). It is reasonable to believe that the two features Callender references are implicitly connected to the recurrence and reversibility objections respectively.

Again and again, work on **MBSM** highlights the insights of (Boltzmann 1877) over against the actual target of the reversibility and recurrence objections, *viz*., the H-theorem.[16] I believe it is therefore a reasonable conclusion that a large contingent of physicists, mathematicians, and philosophers adopt a view of the H-theorem that has been summarized by Carroll (2010, 172): The H-theorem is but "an amusing relic of intellectual history" (ibid.).[17]

2.3 The Lasting Importance of the H-Theorem

2.3.1 The Illustrious History

The decision to overlook or ignore the H-theorem, the decision to regard it as merely "an amusing relic of intellectual history" is a mistake. My (continued) reassessment of **MBSM** puts the H-theorem front and center, using it as the chief means whereby a mechanical explanation of the second law is obtained. Pushing for such prime placement of the H-theorem puts my project in clear continuity with Boltzmann's most mature thought in the *Lectures on Gas Theory* while also aligning itself with some of the most important contributors to contemporary statistical mechanics (both its mathematics and physics), for there are those like me who agree with Richard C. Tolman's (1881-1948) remark that "[t]he derivation of this [H-]theorem and the appreciation of its significance may be regarded as among the greatest achievements of physical science" (Tolman 1979, 134). The reason why it is such an achievement is easy to see. For recall that "[t]he H-theorem shows that because [of]…collisions the quantity H decreases monotonically with increasing time" (Ehrenfest and Ehrenfest 1990, 14). However, "[t]he monotonic decrease of $H(t)$

[15] One might think that (Goldstein 2001, 44) implicitly references the H-theorem, but it doesn't. There, Goldstein seems to be under the false impression that Boltzmann's final or most mature view of statistical mechanics (notice the wording: "Boltzmann did not (finally) claim") is the one given in his 1877 paper, the memoir in which Boltzmann communicates his probabilistic approach. And so, Goldstein relates both the reversibility and recurrence objections to Boltzmann's combinatorial arguments or viewpoint first stated in 1877. Boltzmann's most mature thought was actually communicated in his *Lectures on Gas Theory* (1964) (with vol. 1 appearing in 1896, and vol. 2 appearing in 1898). These lectures hold a very high view of the H-theorem and hardly interact with (Boltzmann 1877).

Let me add here that Goldstein's work on **MBSM** is of the very highest quality and deserves serious study and praise..

[16] As North's discussion of Boltzmannian statistical mechanics asserts (2011, 319 emphasis mine) "Boltzmann's *key insights* were developed in response to the so-called reversibility objections (of Loschmidt and Zermelo)." North then cites (Brush 1975), which I believe really should be either (Brush 1976a) or (Brush 1976b). I am unsure which source was intended. Be that as it may, Brush is a proponent of key aspects of the standard story as articulated by Klein before him. See, for example, (Brush 1974, 52-53, 56).

[17] I am not claiming these thinkers have no arguments for their understanding. For example, Maudlin (1995, 146-147) rejects the incorporation of the H-theorem into a modern statistical mechanics on the grounds that it requires a modification of "the underlying dynamics by adding some 'rerandomization' posit", but (Maudlin continues) such a "surreptitious" modification is without justification. Maudlin is wrong here. See the discussion of the empirical evidence for the **HMC** at (Weaver 2021 sect. 8.2.2).

demonstrated by Boltzmann…implies that the entropy…state increase[s] with time" (O. Penrose 1970, 200). The H-theorem therefore proves the second law even if in a limited domain. But more can be said. The H-theorem's consequences aren't just profound, and its history isn't merely illustrious. The theorem enjoys indirect empirical support, it has been set atop mathematically rigorous foundations in several respects, and it has been rigorously shown to have analogs in both non-relativistic and relativistic quantum mechanics.

2.3.2 The Empirical and Mathematically Rigorous Success

The Boltzmann equation is commonly used with profound success in a great many domains of physics (*e.g*., to study neutron transport, plasma systems, and transport coefficients for various thermodynamic processes etc.). It was derived (given an asymmetry of incoming and outgoing configurations) from Hamilton's equations of motion in the Boltzmann-Grad limit by Oscar Lanford III (1940-2013) in (Lanford 1975, although only an outline of the proof appears there).

According to Carlo Cercignani (1939-2010) (1998, 96), the first truly rigorous proof of the H-theorem for the classical monatomic case was provided by Torsten Carleman (1892-1949) in (Carleman 1933; 1957). However, (according to Weinberg 2021) Josiah Willard Gibbs (1839-1903) proved a generalized H-theorem in his *Elementary Principles in Statistical Mechanics* (Gibbs 1960). Gibbs' proof shows that the H-functional will decrease to a minimum and remain there. Gibbs' argumentation receives an updated rigorous formulation in (Weinberg 2021, 35-37). Additional modern proofs of the H-theorem for the monatomic gas cases can be found in (Cercignani 1998, 273-276) and (Tolman 1979, 136-142).[18] Both (Cercignani 1998) and (Darrigol 2018) proved H-theorems for classical polyatomic gas types.

2.3.3 The Quantum Analogs

The quantum Boltzmann equation was formulated by (Nordheim 1928) and (Uehling and Uhlenbeck 1933). It is therefore not surprising then to see an H-theorem in a full non-relativistic quantum regime along with a proof that includes a quantum analog of the **HMC**.[19] But perhaps ***it is*** surprising to see an analog of the H-theorem in *relativistic* quantum mechanics that includes (as in non-relativistic quantum mechanics) an analog of the **HMC**. In quantum field theoretic statistical mechanics, the equilibration entailed by the analogous H-theorem there follows from features of the continuous wave function in keeping with a metaphysics of QFT that privileges fields over particles. It assumes the system is closed and *truly* out of equilibrium (not in touch with a heat bath), and the Boltzmann equation needed for the H-theorem in that context is rigorously *derived* in (Snoke, Liu, and Girvin 2012 which includes a proof of the quantum field theoretic analog of the H-theorem).

2.3.4 The Key Asymmetric Assumption

The temptation to sweep the H-theorem under the rug really does seem to come from the lasting conviction that it was somehow shown to be suspect by the reversibility and recurrence

[18] On deriving stationarity and uniqueness from Boltzmann's H-theorem, see (Cercignani 1988, 143).

[19] See (Tolman 1979, chapter 12 entitled "The Quantum Mechanical H-Theorem" with the proof appearing on pages 455-477 followed by an application to interacting systems at 477-480). See also the reasoning in (Nordheim 1928, 690-695).

objections (of which there are quantum analogs). The key to seeing why these objections do not work resides in the needed time-asymmetric assumption that is the **HMC**.[20] What precisely is the **HMC**? My rough statement in **sect. 1** is, I believe, pretty much correct. However, there is some debate about the precise form of the assumption in the statistical mechanics literature. Some (mostly philosophers following (Ehrenfest & Ehrenfest 1990)) argue that it is the Stoßzahlansatz, an ansatz relating the number (or the probability) of seeing a pair of molecules with velocities $\mathbf{v}_1$ and $\mathbf{v}_2$ (around $d^3\mathbf{v}_1$ and $d^3\mathbf{v}_2$ respectively) to the product of finding a molecule in that same pair with $\mathbf{v}_1$ around $d^3\mathbf{v}_1$, and the other molecule in that pair with $\mathbf{v}_2$ around $d^3\mathbf{v}_2$ (see e.g., Callender 2011, 85); (Uffink 2017 etc.). This Stoßzahlansatz is often thought to just be what some have called the factorization condition:

(Eq) 7:

$$f^{(2)}(\mathbf{v}_1, \mathbf{v}_2) = f(\mathbf{v}_1)f(\mathbf{v}_2)$$

given that $f^{(2)}$ is the distribution function for two (a pair of) molecules, atoms, or particles in the system.[21] But Fields Medal winner (for work on the Boltzmann equation) Cédric Villani (2002) has convincingly shown how (Eq. 7) (or related factorization expressions) does/do not fully capture the content of the **HMC**. To accurately represent the **HMC**, the equation must be sufficiently generalized, and it is unclear how to proceed. Indeed, it appears that the **HMC** has no mathematical representation at all. As Villani remarked, "the physical derivation of the Boltzmann equation is based on the propagation of one-sided chaos, but no one knows how this property should be expressed mathematically…"[22] Herbert Spohn concluded similarly, "the decrease of [the] H-function is linked to instants of molecular chaos. These properties remain a guess."[23] That the **HMC** eludes rigorous mathematical representation constitutes a problem. I have (in Weaver 2021) called it the **No Mathematics Problem** (**NMP**).

There is some agreement and continuity from Lorentz and Boltzmann all the way down through the decades to Spohn (1991) and Villani (Villani 2006, 785), that the early or original characterizations were right. The **HMC** says that two incoming particles have velocities that are uncorrelated, but subsequent to collision, the velocities of those two particles become correlated.[24] This asymmetry propagates for all future time. As Lanford pointed out in (1975, 77), proofs of the Boltzmann equation and H-theorem "need this assumption at all positive times, not just for t = 0." The **HMC** is therefore not merely an initial condition, for the asymmetry propagates for future times.

All of the disagreement and contention about how to put the **HMC** aside, everyone agrees that it is:

[20] As I have already pointed out, this was first recognized by Lorentz (Lorentz 1887). It was then recognized by George Bryan (1864-1928) and Samuel Burbury (1831-1911). See (Bryan 1895); (Burbury 1894, 1895). Strictly speaking, the condition or assumption at work in the minds of others such as Burbury was not identical to the **HMC** as I've articulated it. See (Dias 1994) for more on Condition A, *i.e.*, what was perceived to be the necessary assumption in the work of Burbury. Dias argues that Burbury's Condition A is related and indebted to Maxwell's Proposition II in (Maxwell 1860a, b).

[21] See (Callender 2011, 85); *cf.* (Uffink 2007, 1036).

[22] (Villani 2002, 99).

[23] (Spohn 1991, 76). I cannot launch a full defense of this point here. I ask the reader to see the reasoning in (Villani 2002) for a more rigorous defense.

[24] I will now speak as if the constituents of monatomic gases are particles, and use the terms 'particles' and 'molecules' interchangeably.

(i) …about collisions, the driving force of entropic increase
(ii) …not merely an initial condition (the one-sided chaos propagates)
(iii)…not part of the classical dynamical equations of motion (it is sometimes called an "extra" mechanical assumption)
(iv)…and like the Boltzmann equation, the **HMC** is not time-symmetric. The direction of chaos propagation is toward the future and not toward the past.

We should now ask: *why* is the **HMC** temporally asymmetric? What explains its temporal arrow? This is the **Chaos Asymmetry Problem** (**CAP**).[25]

Let's now turn to the provision of some motivation for the resolution of the reversibility paradox (which also resolves the **NMP** and **CAP**) in (Weaver 2021) to motivate the causal interpretation of the **HMC**.

3 The Reversibility Paradox

In (Weaver 2021), I argued that the resolution of the **CAP** and **NMP** resides in the resolution of yet another problem, *viz*., **the reversibility paradox** already introduced. First proposed (*to Boltzmann*) by Boltzmann's colleague, Johann Josef Loschmidt (1821-1895), William Thomson (or Lord Kelvin; 1824-1907), and then later by Edward P. Culverwell (1855-1931), the reversibility worry capitalizes on the time-reversal invariance of the microdynamics of statistical mechanical systems.[26] Again, it says that if all of the velocities of the molecules of a (*e.g*., monatomic) gas system are reversed under the performance of the time-reversal operation (remembering that this involves flipping the sign of t and also reversing or flipping all signs of all odd forms of t), H will increase over minus-time and as a result, the gas system will evolve away from the Maxwell distribution instead of toward it. Such a result very plainly contradicts the H-theorem which in this case entails that for monatomic gas systems, H monotonically decreases over time until it hits the Maxwell distribution (I'm assuming that the Boltzmann equation holds for such systems and that for them f satisfies the Boltzmann equation for all times of their evolutions).

My resolution of the reversibility paradox capitalized on what I insisted was a metaphysical and interpretive hypothesis about the nature of the acting forces in collisions between molecules, the very collisions referenced by the **HMC**. My choice interpretive hypothesis affirmed (**Causal Collisions**):

> Within the collisions that are quantified over by the…**HMC**…and that produce entropic increase thereby making true the Boltzmann equation…and H-theorem…are instances of an obtaining fundamental causal relation that is formally and *temporally* asymmetric. Particular instances of this fundamental relation in evolutions of thermodynamic

[25] On the Lanford theorem and the assumed factorization condition, plus attempted resolutions of the **NMP** and **CAP** in that program, see (Uffink and Valente 2010) and (Weaver 2021, Appendix 2).

[26] See (Loschmidt 1876); (Thomson 1874); (Culverwell 1894). There has been some important recent work on Boltzmann's reply to Loschmidt at (Darrigol 2021). I argue that Boltzmann probably read (Thomson 1874) in (Weaver 2021).

> systems necessitate one-sided chaos and produce the velocity correlations referenced by the **HMC**.[27]

Monatomic gas systems march on toward equilibrium by virtue of causal interactions *between their punctiform constituents.* The correct explanation for the equilibration of relevant gas systems is a *restricted* causal explanation. The question: "Do *all* microphysical causal interactions contribute to the entropic increase of the relevant gas system, even collisions between particles and system boundaries?", is an important one. I originally left it unanswered. I now add that the empirical successes of statistical mechanics epistemically justify the thesis that *at least* the collisions between particle constituents of gas systems contribute to entropic increase and that contribution is significant enough to facilitate epistemically justified approximations of thermodynamic properties and changes thereof.

The causal explanatory potency of the H-theorem on its assumed **HMC** is what makes Boltzmann's H-theorem an attempted mechanistic explanation of entropic increase and an attempted mechanistic explanation of the truth of the second law of thermodynamics for gas systems. The fact that the collisions involve a temporally asymmetric fundamental causal relation explains why merely reversing the velocities (under time-reversal) of statistical mechanical evolutions does not result in a reversed evolution of the system (the actual evolution "rewound"). The **HMC** was itself always understood as a time-asymmetric assumption, and so my (Weaver 2021) response to the reversibility paradox reveals why, under the performance of a time-reversal invariance operation, that operation, appropriate solutions to the time-reversal invariant equations of motion, and the **HMC** do not entail a true description of an evolution featuring a reversed propagating (toward our past) one-sided chaos. It also explains why the Boltzmann equation is *not* time-reversal invariant (see the proof of this in Uffink and Valente 2010). The collisions that equation references are collisions involving fundamental temporally asymmetric causation. This is an interpretive maneuver with real empirical consequence. Of course, one would be well within one's epistemic rights if one were to imagine a reversed evolution with a flipped chaos propagation, but that scenario is set up in an artificial manner. It is put in by hand. This resolves the **CAP**. To repeat for clarity: Why is the **HMC** temporally asymmetric? It is asymmetric because the collisions it references involve an obtaining fundamental temporally asymmetric causal relation.

The systems imagined by Boltzmann (and for that matter Maxwell) were idealized systems with elastic collisions. The mechanical interactions are therefore governed by a time-reversal invariant collision theory. Why did I (in Weaver 2021) claim that one cannot secure an evolution of an appropriate gas system in which H increases (and so entropy decreases) over minus-time by way of time-reversal? I insisted (and continue to insist) that one take the **HMC** and the collisions it references seriously. The worlds or idealized systems imagined by Maxwell and Boltzmann feature temporally reversed evolutions (an idealized world/system rewound) and therefore *do not* feature systems that evolve in a way that can be partly described by the **HMC**. This is because the types of collisions I insert into the **HMC** are not idealized but are instead *real-world collisions*. The particles (approximated by point-masses) really do slam into each other. Maxwell and Boltzmann both modeled around such collisions using a conceptual strategy Mark Wilson has called **physics avoidance**.[28] Binary collisions of point-masses yield blow-ups or singularities (in

[27] (Weaver 2021, p. 8) emphasis mine.

[28] (Wilson 2017). See (Darrigol 2018, 139); (Weaver 2021 sect. 7.2.1; I cite primary source literature to support this point.)

the mathematics). Therefore, neither the collision theory of Maxwell and Boltzmann, nor modern collision theory describe the intimate details of such collisions.[29] Instead, certain collision parameters are used to capture the post-collision velocities of colliding subsystems, but what transpires during the Δts when the subsystems interact is left without an explicit direct modeling. That is why the involved causation in **Causal Collisions** is not represented by the relevant mathematical models. And here, I leaned on the early pioneering work of Gottfried Wilhelm Leibniz when he maintained that during the relevant Δts, molecules or particles are joined by efficient causation (Leibniz 1989); (Leibniz 1998); (Weaver 2021, sect. 7.2.1). Thus, I am interpreting the **HMC** as a hypothesis expressly about real-world collisions that cannot be handled by the mathematics because that mathematics yields blow-ups. This constitutes a resolution of the **NMP**. I have explained why the **HMC** hides from modeling.

I hope that what I've here summarized (*in improved fashion*) motivates my earlier (from Weaver 2021) approach to the reversibility paradox. Further evaluation of (Weaver 2021) is beyond the scope of this project. Instead, and as promised, I will argue that the H-theorem, **HMC,** and **Causal Collisions** can be used to solve another problem that Boltzmann's H-theorem project encountered, *viz*., the **recurrence paradox** *as articulated by Poincaré*.[30] As in (Weaver 2021), sensitivity to certain historical developments surrounding the early articulations of that paradox will be instructive for seeing how much work **Causal Collisions** can do. It is to that historical discussion that I now turn.

4 Poincaré and the Three-Body Problem[31]

4.1 Setting the Scene: The Essay Competition of 1889

Novice mathematician, Oscar Fredrik or Oscar II (1829-1907) was king of both Norway and Sweden. Oscar II celebrated his 60th birthday on January 21st, 1889. To mark the occasion, he, and Swedish mathematician Gösta Mittag-Leffler (1846-1927) established an essay competition. They connected the competition to the academic journal *Acta Mathematica*, a journal which Oscar II financially supported.[32] The competition prize was 2,500 Swedish crowns (or kronor) (for comparison, around this time, Mittag-Leffler's annual salary was 7,000 Swedish crowns[33] or kronor).

In June of 1884, Mittag-Leffler sent a letter to the brilliant mathematician, Sofya Vasilyevna Kovalevskaya (1850-1891).[34] In it, Mittag-Leffler reported on a recommendation from Oscar II and Carl Johan Malmsten (1814-1886) regarding the constitution of the review committee for the future 1889 essay competition. The list recommended:

- …a Belgian or French mathematician such as Charles Hermite (1822-1901)

[29] *Q.v.*, my brief discussion of regularization techniques and Karl Sundman's result in **sect. 5**.

[30] I do not discuss in detail the objections and arguments of Zermelo. However, much of what I say in response to Poincaré can be used in a proper response to Zermelo as well.

[31] The following history (throughout **sect. 4**) is told in great detail by (Barrow-Green 1994, 1997), (Darrigol 2018, 388-390), (Domar 1982), (Gray 2013, 253-299), (Rågstedt 2022), (Rowe 1998), and (von Plato 1994, 89-93). My retelling shall be heavily reliant upon this excellent historical work. I am especially indebted and reliant upon the work of Barrow-Green and Gray cited throughout this section.

[32] (Domar 1982, 6).

[33] (Domar 1982, 6).

[34] On Kovalevskaya, see (Audin 2011); (Cooke 1984); (Koblitz 1983); (Koblitz 2013, 107-136); (Kovalevskaya 1978); and (Stillman 1978).

- …an American or English mathematician such as Arthur Cayley (1821-1895) or James Joseph Sylvester (1814-1897)
- …an Austrian or German mathematician such as Karl Weierstrass (1815-1897)
- …the editor of *Acta Mathematica*
- …an Italian or Russian mathematician such as Kovalevskaya, or Pafnuty Lvovich Chebyshev (1821-1894), or Francesco Brioschi (1824-1897)[35]

Kovalevskaya thought it practically impossible to recruit as recommended (Barrow-Green 1994, 109; 1997, 55). In the end, there were four topics with but three judges for discerning a winner, *viz*., Hermite[36], Mittag-Leffler, and Weierstrass.

Mittag-Leffler and company appeared to have deliberately crafted their list of topics to pique the interest of Poincaré. As Gray has written, "one can hardly imagine a set of questions better contrived to attract Poincaré: all four questions could have been tackled by him."[37] Indeed, one of the four topics (the fourth) directly referenced Poincare's new function-type (*i.e*., *fonctions fuchsiennes* or Fuchsian functions).[38]

Among the four proposed problems or questions that could be addressed in the interest of participating in the competition (although one could also address a topic of one's choice), only one resided in the domain of celestial mechanics. That one problem was the *n*-body problem (see (1) below).[39] Here is a summary (dependent upon Barrow-Green 1997, 51-70 and Gray 2013, 267-268) of the first question/issue (probably) recommended by Weierstrass[40]:

(1) Suppose there's an *n*-particle system whose citizen particles *never interact by way of contact collisions* and whose citizen particles are all under the sway of Newtonian gravitation.[41] Is there a way to demonstrate the stability of the planetary orbits by looking to a method (reportedly communicated by Johann Peter Gustav Lejeune Dirichlet (1805-1859) to an anonymous mathematician who was probably Leopold Kronecker (1823-1891)) of integrating the differential equations of motion governing the aforesaid particle system assumed to approximate some planetary system?

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[35] Summarized from the reproduction found in (Barrow-Green, 1994, 109; 1997, 53, 227-228).

[36] On Hermite's work, see (Goldstein 2007); (Goldstein 2011); (Hermite 1905-1917) and (Hermite and Stieltjes 1905).

[37] (Gray 2013, 268).

[38] See (Barrow-Green 1997, 230-231). According to (Gray 2013, 268), Hermite posed the fourth question and admitted to doing so for the purposes of attracting Poincaré.

Fuchsian functions are a distinguished class of automorphic functions that are defined on a disk and that are invariant under transformations belonging to particular discrete groups.

[39] It was probably Weierstrass who recommended the *n*-body problem (Barrow-Green 1994, 110 and n. 9; 1997, 59); (Gray 2013, 268). Weierstrass had an interest in that problem himself (Mittag-Leffler 1912). For more on the work of Weierstrass, see (Bottazzini 2003); (Dugac 1973); (Gray 2008, 68-71, 129-133); (Gray 2015, 195-216); (Hawkins 1977); (Boniface 2007); and (Lützen 2003, 184-187).

[40] See n. 39. On the importance of the source of the question, *q.v*., **sect. 4.3.2** below.

[41] The wording in the English version of the original announcement at this point is as follows:

> "A system being given of a number whatever of particles attracting one another mutually according to Newton's law, it is proposed, on the assumption that **there never takes place an impact of two particles** to expand the coordinates of each particle in a series proceeding according to some known functions of time and converging uniformly for any space of time" (As quoted in Barrow-Green 1997, 229) emphasis mine. See **Appendix 1**.

4.2 Poincaré's Submission

Poincaré submitted an entry to the competition. Submissions were supposed to be anonymized. Poincaré did not follow directions. Everyone knew which submission was his.[42] Mittag-Leffler and Weierstrass took a month and decided, with Hermite agreeing, that Poincaré won the competition. But what did Poincaré say? Poincaré exegesis was/is a difficult task. Mittag-Leffler corresponded with Poincaré so that clarification of his submission might be acquired. This was an indication of further violation of the rules (Barrow-Green 1997; Gray 2013; Nabonnand 1999).

In the end, Poincaré would add 93 pages to the original submission to help explicate his many new results and ideas.[43] Researchers have been unable to acquire the originally communicated memoir. However, (quoting Barrow-Green) "correspondence at the Institute Mittag-Leffler suggests that, excluding the *Notes* [*i.e*., the material Poincaré produced to help clarify his submission], it assumed a very similar form to the first printed version"[44] that is (Poincaré 1889).

That some of Poincaré's results were new was challenged by astronomer Johan August Hugo Gyldén (1841-1896), a member of *Acta Mathematica*'s editorial board. But matters were (perhaps) worse. The original submission (Poincare 1889) contained an important error which was discovered by Poincaré in light of some questions from an assistant editor with *Acta Mathematica*, *viz*. Lars Edvard Phragmén (1863-1937) who was later promoted to full editor and helped to a position in Stockholm in light of his admirable role in the ordeal under discussion.[45] Poincaré confessed his mistake and its severity (which was quite significant) in a letter to Mittag-Leffler dated December 1st, 1889 (Gray 2013, 278). Mittag-Leffler subsequently asked Poincaré to rework the essay with corrections despite the fact that he (*i.e*., Mittag-Leffler) had already begun to share the essay with others (*e.g*.., Kovalevskaya, Gyldén, and Sophus Lie (1842-1899) *inter alios*).[46] Poincaré did just that. The published version (Poincaré 1890 [2017]) was the result, and (to quote

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[42] Barrow-Green (1994, 113) stated,

> "When Poincaré's entry arrived it was clear that his reading of the regulations had been somewhat perfunctory. As required he had inscribed his memoir with an epigraph, but instead of enclosing a sealed envelope containing his name, he had written and signed a covering letter, and had also sent a personal note to Mittag-Leffler. However, since he had already told Mittag-Leffler and Hermite of his intention to enter, and he knew that they would recognize his entry by its content—it was an explicit development of his earlier work on differential equations—as well as by his handwriting, it clearly was not a deliberate attempt to flout the procedures."

In addition, Barrow-Green (1997, 61) cites evidence that Poincaré made known to Mittag-Leffler his intention to submit an essay for the competition. Barrow-Green (1994, 113) states that all three judges knew that Poincaré would submit an essay.

[43] (Barrow-Greene 1994, 115; 1997, 65).

[44] (Barrow-Green 1997, 72) emphasis in the original.

[45] (Barrow-Green 1997, 69). The error in Poincaré's memoir pertained to Poincaré's remarks about asymptotic surfaces. See (Barrow-Green 1997, 67-69); (Gray 2013, 277-280). Poincaré incorporated high praise of Phragmén in the Author's Preface to (Poincaré 1890 [2017], xix-xx). Barrow-Green (1994, 118) reports that the error was committed at a place in the memoir that was distinct from that place about which Phragmén had inquired.

[46] (Barrow-Green 1994, 118; 1997, 67).

Gray) "[i]t is in many places unchanged from the one that won the prize. In others it includes the material first submitted as one of the notes, and in others, where the original was in error, it is completely new."[47]

4.3 The Restricted Three-Body Problem[48]

Poincaré's (Poincaré 1889) was not published. As already noted, that earlier draft was the object of significant revisions and additions. The published version of his now famous memoir featured the title, "Sur le problème des trois corps et les équations de la dynamique", or "The Three-Body Problem and the Equations of Dynamics". It appeared in 1890.[49] It helped to catapult Poincaré into the authorship of his three-volume *magnum opus*, *viz*., *Les Méthodes Nouvelles de la Mécanique Céleste* or *The New Methods of Celestial Mechanics* (Poincaré 1892, 1893, 1899).

Poincaré (1890) addressed a specific instance of the *n*-body problem called (by English scholars, according to Poincaré) the **two degrees problem**. Here, one looks at a system of three bodies:

Body #1 (primary): A celestial body with very large mass M
Body #2 (primary): A celestial body with very small mass $m << M$
Body #3 (the planetoid): A celestial body with infinitesimal mass m_0

The larger masses orbit their center of gravity in separate circles on the same plane, whilst the third orbits on that same plane. Poincaré set out to find the motion of the planetoid. In so doing, he opted to try and find a solution to what is now called **the restricted three-body problem**.[50] Admittedly, Poincaré failed to resolve the problem. Poincaré promised to demonstrate the stability of the planetoid's orbit "in the sense that" he claimed to be able to "give precise bounds on the maximum distance the planetoid escaped from the other two…".[51] While not as difficult as the more general three-body problem or the *n*-body problem, the two degrees (or restricted three-body) problem helps theorists approximate the behavior of complex systems like the Earth, Moon, and Sun.[52]

How did Poincaré tackle the restricted three-body problem?

4.3.1 The Mathematical Modeling

Poincaré's choice modeling technique adopted Hamilton's equations of motion. Unfortunately, following Poincaré's precise reasoning is overly difficult. Poincare's notational style and mathematical modeling is quite opaque to the modern reader. For example, Poincaré did

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[47] (Gray 2013, 280). For a taste of how (Poincaré 1889) differed from (Poincaré 1890), see the list of theorems appearing in the former but not in the latter at (Barrow-Green 1997, 247-248), and compare the tables of contents reproduced and discussed at (Barrow-Green 1997, 239-245, *cf.* 72-73).

[48] On the history of this problem, see (Barrow-Green 1997, 14-28).

[49] I will work with the 2017 translation of Bruce D. Popp, cited as (Poincaré 2017). See also (Poincaré 2003). The essay itself was originally published in volume 13 of *Acta Mathematica*. Important additional thoughts of Poincaré were published in (Poincaré "Mechanism", 1893).

[50] On more modern solutions to some three-body problems in classical celestial mechanics, see (Šuvakov and Dmitrašinović 2013).

[51] (Gray 2013, 271).

[52] There was an intense amount of interest in these types of issues at the time of the publication of Poincaré's memoir. See (Whittaker 1988, 339), (Grant 1966), and (Gray 2013, 253).

not use notation that distinguishes between partial and full derivatives. One is forced to infer that derivatives with respect to time are full while the others are partial. In addition, he uses 'F' to pick out the Hamiltonian and the notation of the calculus of variations is dated. I will therefore help the modern reader come to grips with Poincaré's work on the problem by modeling the *unrestricted* three-body problem with Hamilton's equations of motion. I will then connect important elements of that modeling to Poincaré's choice way of trying to come as close as he could to a solution of the restricted three-body problem (*i.e.*, by proving a type of stability of the planetary orbits).[53]

Start by stipulating that body #1 is b_1, body #2 is b_2, and that body #3 is b_3. One can further stipulate that these bodies have gravitational masses, m_1, m_2, and m_3 respectively. Specify that $i = 1, \dots, 3$, let the j^{th} generalized position coordinate of the i^{th} body b_i be q_{ij}, and let the j^{th} generalized velocity component of the i^{th} body b_i be the time derivative of q_{ij}. Using the Gaussian gravitational constant k (as in Kepler's third law; see Kopeikin et. al. 2011, 819), set k^2 equal to unity and model with Hamiltonian equipment by first specifying a gravitational potential energy U_g. We are allowed to do this because it is a further assumption that our system is holonomic and conservative.

(Eq) 8:

$$U_g = -\frac{m_2 m_3}{r_{23}} - \frac{m_3 m_1}{r_{31}} - \frac{m_1 m_2}{r_{12}}$$

(Eq. 8) will help us model with Hamilton's well-known canonical equations of motion that yield 18 first-order differential equations.

(Eq) 9 (set):

$$\dot{p}_{ij} = -\frac{\partial H}{\partial q_{ij}}, \qquad \dot{q}_{ij} = \frac{\partial H}{\partial p_{ij}}$$

where p_{ij} is generalized or conjugate momentum,

(Eq) 10:

$$p_{ij} = m_i \dot{q}_{ij}$$

The Hamiltonian or total mechanical energy for the 3-body system is now the well-known expression:

(Eq) 11:

$$H = \sum_{i,j=1}^{3} \frac{p_{ij}^2}{2m_i} + U_g$$

[53] Much of what's said below is standard and need not be cited. But for good measure, I note that I lean in part on the following sources in this section (Barrow-Green 2008a, 726-728), (Meyer and Offin 2017, 61-102), (Siegel and Moser 1995, 33-42 whose discussion involves regularization, and (Winter 1941)).

These equations are far more convenient than the Newtonian variety, but they are numerous in amount. To simplify further, one should look for a special algebraic constant that shows off a mathematical dependence between the involved variables (Barrow-Green 2008a). The imagined special mathematical object will remain the same in all solutions to the 18 first-order differential equations in the Hamiltonian formulation. Of course, the object-type I have in mind is the invariant integral introduced at (Poincaré 2017, 37-76) but already known to Leonhard Euler (1707-1783) and Joseph-Louis Lagrange (1736-1813) in a similar context (Barrow-Green 2008a).

For the three-body problem, there are ***but*** 10 invariant algebraic integrals. One represents the conservation of total energy. Three give the conservation of angular momentum. Six others are used to represent the trajectory of the center of mass by connecting three of the six invariant integrals to relevant momentum variables, leaving the remaining three others for relevant position variables.

Let's slow down and repeat just a little bit for proper digestion. Let's also connect what we've said about invariant integrals in the modeling to Poincaré's way of attacking the restricted three-body problem

Poincaré attempted to address the restricted three-body problem by modeling it with a system of canonical differential equations (Hamilton's equations) whose solution—presupposed to exist—gives the periodic orbit of body #3 (the planetoid). This orbit begins at point $\wp$ that rests on an imaginary arc the points of which constitute nearby alternative initial positions for body #3's periodic orbit (hence Poincaré's discussion of nearby solutions and the like in Poincaré 2017). As can be discerned from the preceding discussion, crucial to modeling the arc's movement is the specification of invariant (or (on the arc) constant in time) integrals. From the existence of invariant integrals, Poincaré could show that over the course of its evolution, body #3 (the planetoid) will be confined to a spatial region that is bounded (Gray 2013, 272). The recurrence theorem was then used to show that such confinement entails Poisson stability about which Poincaré stated:

> In the following, we will frequently need to be concerned with the question of stability. There will be *stability*, if the three quantities x_1, x_2, and x_3 remain less than certain bounds when the time *t* varies from $-\infty$ to $+\infty$; or in other words, if the trajectory of the point *P* remains entirely in a bounded region of space…[54] …For there to be stability, after sufficiently long time the point *P* has to return if not to its initial position then at least to a position as close to this initial position as desired. This latter meaning is how Poisson understood stability.[55]

Hence, the recurrence theorem was used to demonstrate that body #3 (the planetoid) would return to its initial position, or arbitrarily close to its initial position.

[54] (Poincaré 2017, 5).
[55] (Poincaré 2017, 58).

4.3.2 No Collisions

According to Poincaré (2017), a point $\wp$ would, over time, sweep out a curve defining the trajectory of the point representing the planetoid. $\wp$ must be tracked by a coordinate system (x, y, z), or following Poincaré's convention (x_1, x_2, x_3), differentiated with respect to time. As we've seen, one must look to a collection or system of differential equations not unlike those below to model accordingly[56]:

(Eq) 12 (set):

$$\frac{dx_1}{dt} = X_1, \qquad \frac{dx_2}{dt} = X_2, \qquad \frac{dx_3}{dt} = X_3$$

which is a specific instance of the more general set of equations:

(Eq) 13 (set):

$$\frac{dx_1}{dt} = X_1, \qquad \frac{dx_2}{dt} = X_2 \, , \dots , \frac{dx_n}{dt} = X_n$$

X_1, X_2 and X_3 are assumed to be uniform analytic functionals that are respective functions of x_1, x_2, and x_3. It is perhaps more efficient to specify the relevant collection of equations as follows $(i = 1, \dots, n)$:

(Eq) 14:

$$\frac{dx_i}{dt} = X_i$$

where X_i now hides: X_1, X_2 and X_3…etc.[57] In such a case, the functions and functionals are generalized and the motion of $\wp$ travels in a 6*N* dimensional phase space. The trajectory of the point gives its evolution, and that evolution is determined by the system of differential equations. If $n = 3$, then we are back to modeling a physical system in a 3*D* space, and (Eq. 12) gives the system's velocity.

In either the generalized or non-generalized cases, to model appropriately, Poincaré says one will need canonical dynamical differential equations of motion. This is how Poincaré's way of doing things connects with our modern Hamiltonian modeling. The canonical differential equations are (again) Hamilton's equations.

Because our background theory is a classical (celestial) mechanical one, the uniformity of functional sets like (Eq. 12 (set)) ensures that every point features but one trajectory extending through it.[58] Poincaré was aware of two exceptions to this rule of classical mechanics. He knew that "if one of" the functionals (*i.e.*, X_1, X_2, X_3 etc.) "becomes infinite or if all three are zero", there would be "an exception" to the rule. The "points where these exceptions occur are called *singular*

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[56] The equations of **sect. 4.3.2** are taken from Poincaré 2017.

[57] See (Poincaré 2017, 43).

[58] (Brush 1976b, 630).

points."[59] The infinities mentioned here have to do with the well-known problem of singularities in classical dynamics, a problem which (to remind the reader) my (Weaver 2021) project capitalizes on. Again, in classical mechanics, when two subsystems collide, they coincide at a single point in space, and thus two trajectories pass through one and the same point resulting in unmanageable infinities indicative of singularities. Why is this important? Poincaré knew that anything close to a resolution of the restricted three-body problem would require that one use the power series technique of integrating a system of differential equations. This meant that the system of differential equations must feature functionals that can be expanded in increasing powers of the coordinate variables plus the powers of a parameter μ. But there can be no expansion of this kind when the functionals are not analytic. They can fail to be analytic when the coordinate variable values blow-up as in the case of collision singularities.[60] And so, "[c]ollisions result in singular points in Newton's law of gravitation preventing convergence of series expansions. The problems considered must therefore be collisionless".[61] Rendering the restricted three-body problem collisionless constrains the nature of the defining system of differential equations used to recover motions.

One reason for disclosing the intimate historical details I articulated in **sects. 4.1** and **4.2** was to ensure the presentation of two facts.

(**a**) Poincaré's choice essay question (*i.e.*, the first question about the *n*-body problem) was recommended for the essay competition by Weierstrass.

(**b**) Both Mittag-Leffler and Weierstrass served as judges in the essay competition.

It would be surprising if Poincaré did not believe these facts upon authoring, submitting, and revising his essay. Consider that while Weierstrass is only mentioned twice in (Poincaré 2017), it was well-known at the time that Weierstrass had an interest in the *n-body* problem (Mittag-Leffler 1912). Mittag-Leffler kept Poincaré apprised of Weierstrass's work in analysis[62] and would have had an interest in defending and promulgating Weierstrass's research programs because he was one of Weierstrass's many brilliant students.[63] In addition, Mittag-Leffler had a very good professional relationship with Poincaré[64] and we know that Weierstrass would have had an interest in securing Poincaré's response to the *n*-body problem because he studied Poincaré's work closely interacting with him on the *n*-body problem before the essay competition (Bottazzini 2014; Nabonnand 1999). What is more, Weierstrass (quoting Barrow-Green) "designed his questions [including question #1] to appeal particularly to Poincaré."[65] Poincaré corresponded with Mittag-Leffler about his intentions to submit an entry to the essay competition (see the correspondence

[59] The quotations in this and the preceding sentence in the main text come from (Poincaré 2017, 5). But see (ibid., 11-12) where Poincaré there communicates that collisions yield singular points.

[60] It's true that Poincaré did not seem to have in mind non-collision singularities, but Paul Painlevé (1863-1933) showed that such singularities do not obtain in the context of the three-body problem (Barrow-Green 1997, 78, 175-197); (Painlevé 1895/2015).

[61] (Popp 2017, xii).

[62] See the correspondence cited in (Nabonnand 1999, 60).

[63] Mittag-Leffler defended Weierstrass's accomplishments and reputation, promoting his work outside of the classrooms in which so much of Weierstrass's brilliance was put on display. Mittag-Leffler studied with Weierstrass after his doctoral work.

[64] Mittag-Leffler looked to Poincaré to help him establish the reputation of the *Acta Mathematica* which he founded in 1882. Poincaré obliged. He published five papers in each of the first five volumes of the journal.

[65] (Barrow-Green 1997, 62). This point was made about all judges in **sect. 4.1**.

quoted at Nabonnand 1999, 61) and recall that in violation of the essay competition rules, Mittag-Leffler and Poincaré corresponded during the evaluation of the essay competition submissions so that clarification of Poincaré's submission could be acquired (*q.v.*, **sect. 4.2**). Lastly, recall that alongside Weierstrass and Mittag-Leffler sat a third judge of the competition, *viz*., Hermite (*q.v.*, **sect. 4.1**). Hermite was Poincaré's doctoral advisor. It is therefore likely that Poincaré probably knew facts (**a**) and (**b**) upon authoring, submitting, and revising his essay.

What is the significance of the fact that Poincaré probably knew both (**a**) and (**b**)? Weierstrass was responsible for a turn to rigor in the history and development of modern analysis. Weierstrass's emphasis of rigor mainly consisted of the imposition of a methodological constraint, *viz*., to explicate and solve problems in terms of analytic functions.[66] For Weierstrass, "Das letzte Ziel bildet immer die Darstellung einer Funktion"[67] or "The final goal is always the representation of a function". By "die Darstellung", Weierstrass undoubtedly meant "analytic representation" (Lützen 2003, 188). Thus, when Weierstrass crafted his statement of the *n*-body problem as question #1 of the essay competition, he did so with the intent of soliciting a rigorous solution to that problem. Poincaré probably knew that a rigorous solution was required *because* he knew facts (**a**) and (**b**). Indeed, my observation here is supported by the already referenced interaction between Weierstrass and Poincaré on the *n*-body problem, interaction (again) that dates prior to the essay competition. Weierstrass communicated worries about Poincaré's (1882) *Sur l'intégration des équations différentielles par les séries* (On the Integration of Differential Equations by Series) in which Poincaré had argued that differential equations have solutions whose contents are represented by series that converge with respect to any value of the new variable. Weierstrass challenged the idea by appeal to a three-body problem that involves collisions. Poincaré responded by noting that the new variable would become singular (because of the collisions). He then stated that "the formulas do not give anything" subsequent to collisions, "that is the best they have to do."[68] Non-coincidently then, "Weierstrass…specifically excluded collisions in the competition question" on the *n*-body problem.[69]

I now invite the reader to draw the following conclusion. The system-types to which Poincaré's famous recurrence theorem applies are system-types that do without collisions. The preclusion of collisions helped ensure a singularity-free treatment of orbital stability in the context of the restricted three-body problem. This was all in the name of rigor. The motivation stemmed from perceiving the type of modeling that the competition judges desired.[70] But there's a problem. Precluding collisions makes perfect sense in the context of proving the Poisson stability of planetary orbits. It does not make sense in the context of a general kinetic theory of gases for even dilute monatomic gases have constituent corpuscles that collide a plurality of times every second. If one desires to follow the evolution of a gas system more closely, one should not be completely happy with the "rigorous solutions" because they incorporate modeling walk-arounds and

[66] See (Gray 2008, 69-70) who argues that the picture of Weierstrass as the "arch rigorist" and "the man who put the edifice in place…", while popular among historians, is not without need of cropping or qualification. For example, Weierstrass did not like Cauchy's integral theorem and sought to push integrals and integration out of his theory of analysis. Still, Gray concludes that "[i]f Weierstrass was not some impossible paragon of rigor, he was nonetheless its most powerful advocate" (ibid., 71).

[67] (Weierstrass [1886] 1988, 176).

[68] As quoted and translated by (Nabonnand 1999, 60) who is my secondary source and on whom I lean for my readings of this exchange.

[69] (Barrow-Green 1997, 78); and (**Appendix 1**).

[70] This point should not be over emphasized. Poincaré's essay still contained gaps in reasoning. This was Poincaré's typical style. That style drew criticism from both Mittag-Leffler and Weierstrass.

finessing assumptions to avoid singularities.[71] The cost is treating the system as if it does not involve real-world collisions between gas constituents.

4.3.3 The Needed Recurrence Theorem

Poincaré's memoir makes use of many theorems that I will not review here because they have received careful attention in (Barrow-Green 1997, 77-131).[72] Chief among the many theorems is of course the recurrence theorem found at (Poincaré 2017, 58-68 where this page range includes the presentation of a corollary). The theorem is crucial to his efforts because (repeating a little bit) it establishes that there are infinitely many Poisson stable evolutions of the planetoid.

Poincaré's proof of the recurrence theorem did not use Henri Lebesgue's measure theory (1875-1941) and neither did the proof found within Zermelo's often discussed (later) work.[73] Lebesgue's research on measure theory was not published until after the turn of the century (Lebesgue 1902). One doesn't therefore see a modern rigorous proof of the recurrence theorem that makes use of measure theory until the work of Constantin Carathéodory (1873-1950) in (Carathéodory 1919; 1956, 296-300). There is some question among scholars in the literature about whether Poincaré's proof is nonetheless sufficiently rigorous even if it doesn't use measure theory. Brush (1976b, 631), Clifford Truesdell (according to evidence cited by Barrow-Green 1997, 86), and Wintner (1947) all maintain(ed) that Poincaré's proof was in essence correct and sufficiently rigorous. I take no stand on this matter but note here that Poincaré's reasoning at least provides sufficient epistemic justification for believing the consequent of the theorem based on its mechanical assumptions/presuppositions and antecedent.

But what is the theorem precisely? Poincaré asked that one look to a system that is a point with coordinates x_1, x_2, x_3 so that $n = 3$. He then assumed that this point remains in a finite boundary or area with a finite volume described by an invariant integral which he wrote as:

$$\int dx_1 dx_2 dx_3$$

---

[71] The use of potentials such as the Lennard-Jones potential (**LJ-P**) to help model collisions is yet another way in which physicists practice walk-arounds. The **LJ-P** says:

$$V(r) = 4\epsilon[\left(\frac{\sigma}{r}\right)^{12} - \left(\frac{\sigma}{r}\right)^{6}]$$

where σ gives the distance at which the potential energy of the constituent-to-constituent interaction becomes zero (*i.e.*, the distance parameter), ϵ gives the dispersion energy, and r gives one the distance between the particles. It should be obvious from this equation that the **LJ-P** approaches infinity as *r* approaches 0. It is therefore common to invoke a Van der Waals barrier to prohibit such blow-ups so as to ensure that the two gas particles can be modeled using that potential. Indeed, there is a minimum distance r_m less than which the **LJ-P** ceases to make sense. Thus, choosing to work with the **LJ-P** just amounts to (*inter alia*) making sure your choice gas constituents do not actually make contact. I thank Siddharth Muthu Krishnan here for challenging me to say something about potentials like the **LJ-P**. (These facts are well-known and in no need of citation-support. But for good measure, see (Losey and Sadus 2019).)

[72] My discussion of the recurrence theorem leans on (Albert 2000, 73-81); (Barrow-Green 1997, 86-88); (Brush 1976b, 630-640); (Darrigol 2018, 388-403); (Gray 2013, 272-273); (Poincaré 2017); (von Plato 1994, 28).

[73] On Boltzmann, Zermelo and the recurrence theorem, see (Boltzmann 1896); (Boltzmann Zermelo's Paper 1897); (Boltzmann Poincaré 1897); (Kuhn 1978, 26-29, 270); (Brush 1976a, 238-240); (Brush 1976b, 627-640); (Darrigol 2018, 388-403); (Uffink 2013); (von Plato 1994, 89-93); (Zermelo Theorem of Dynamics 1896); (Zermelo Reply to Boltzmann 1896). On Zermelo, see (Ebbinghaus 2007).

Quite naturally then, the finite region to which our point is restricted features a volume that is invariant over time. Poincaré then adds, “consider an arbitrary region r_0, however, small this region, there will be trajectories which will pass through it infinitely many times.”[74]

I have included Poincaré’s statement of the recurrence theorem for historical comprehensiveness. The best characterization reads as follows:

> THEOREM I (recurrence theorem): Suppose that the coordinates $x_1, x_2,\ x_3$ of a point P in space remain finite, and that the invariant integral $\iiint dx_1 dx_2 dx_3$ exists; then for any region r_0 in space, however small, there will be trajectories which traverse it infinitely often. That is to say, in some future time the system will return arbitrarily close to its initial situation and will do so infinitely often.[75]

As others have noted, this theorem strictly implies that for systems of the kind with which Poincaré was concerned, there are infinitely many solutions of the relevant system of differential equations describing evolutions exhibiting Poisson stability.[76] But it is easy to see that this theorem will not work if the curves or trajectories traveled in phase space encounter singularities. That such singularities play havoc with solution curves in Hamiltonian mechanics is well-known (Devaney 1982, 535). Indeed, already in the early 1880s, Poincaré had recognized that “if” a solution curve “never meets a singular point it can be followed forever.”[77] Thus, if the curve can’t “be followed forever”, then the curve “meets a singular point”.

I cannot improve upon the statements of the theorem’s proof that appear in (Albert 2000, 73-81), (Barrow-Green 1997, 86-88), (Darrigol 2018, 388-403), (Gray 2013, 272-273), and (Poincaré 2017), so I leave the proof unexpressed. *Everyone* accepts the theorem as such.

5 Beyond Celestial Mechanics to Statistical Mechanics

After the *Acta Mathematica* competition, Poincaré discussed the implications of his theorem for the kinetic theory of gases in the context of evaluating attempted mechanical explanations of the second law of thermodynamics (Poincaré Mechanism 1893; Poincaré 1966). While he did not cite (Boltzmann 1872) or (Boltzmann 1875), Poincaré’s (1893; 1966) reasoning had a direct bearing on the real-world applicability of the *H*-theorem, an attempted mechanistic explanation of the second law at least in a restricted domain. He wrote:

> The kinetic theory of gases is up to now the most serious attempt to reconcile mechanism and experience, but it is still faced with the difficulty that a mechanical system cannot tend toward a permanent final state but must always return eventually to a state very close to its initial state [recurrence]. This difficulty is overcome only if one is willing to assume that the universe does not tend irreversibly to a final state, as seems to be

[74] (Poincaré 2017, 58).

[75] (Barrow-Green 1997, 86). Remember that for Poincaré, the target system abides by modeling that includes canonical equations of motion and a Hamiltonian function that can be represented as an invariant integral. Total mechanical energy is therefore conserved in the target system.

[76] See ibid.

[77] Quoting Gray’s point at (2013, 258).

indicated by experience, but will eventually regenerate itself and reverse the second law of thermodynamics.[78]

The argument against the *H*-theorem from Poincaré's recurrence theorem should now be clear.

SOA = There is a (forever) closed conservative classical monatomic gas system SYS that is forever confined to a finite region of space.

The Argument from Recurrence

(1) SOA and SYS starts its evolution in a low entropic state at time t_1.
(2) The recurrence theorem and its assumptions are true (*i.e*., they are applicable to SYS).
(3) If (1), then (if the recurrence theorem and its assumptions hold (*i.e*., they are applicable to SYS), then SYS will at some future time t (where $t >> t_1$) evolve back to its initial low entropy state (or arbitrarily close to that initial low entropy state)).
(4) If the *H*-theorem and its presuppositions are true (*i.e*., they are applicable to SYS), then it is not the case that SYS will at some future time t (where $t >> t_1$) evolve back to its initial low entropy state (or arbitrarily close to that initial low entropy state).[79]
(5) Therefore, it is not the case that (the *H*-theorem and its presuppositions are true (*i.e*., they are applicable to SYS)).

The **argument from recurrence** is not sound. SYS is a monatomic gas system that is confined to a finite region and that increases in entropy. It does this by virtue of collisions between its constituent particles. If there are collisions between the constituents of the gas, then the recurrence theorem's assumptions fail to apply to SYS's evolution. **Sect. 4.3.2** and **sect. 4.3.3** demonstrated that the recurrence theorem requires a "no collision" or "no singularity" assumption. That assumption is at odds with the general mechanism of entropic increase if that mechanism is properly understood. In other words, I can resolve the recurrence paradox by arguing that premise (2) should be rejected.

The "no collision" assumption is incompatible with the **HMC** as I have understood it (*i.e*., as it is interpreted by **Causal Collisions**). The **HMC** is an interpretive hypothesis *about the nature of the general mechanism of entropic increase*. It is therefore no surprise that the recurrence theorem is seemingly problematic for proponents of the *H*-theorem's real applicability to the actual world. The **HMC** is an assumption or presupposition of the *H*-theorem. The apparent problem goes away in a manner favorable to proponents of the *H*-theorem once one realizes that one acquires the necessary velocity changes during the process of equilibration via the asymmetric *real* (and not to be walked-around) causal collisions the **HMC** (as interpreted through the lens of **Causal Collisions**) references.

There's more to say. Recall that the mathematical modeling of collisions in both old and modern kinetic theory or statistical mechanics walk-around the collisions (an instance of Wilson's "physics avoidance"). *That is why* that modeling—which is part of traditional Boltzmannian classical statistical mechanics or **MBSM**—renders that mechanics susceptible to the **argument from recurrence**. The threat that is the argument from recurrence goes away once one stops taking the convenient modeling walk-arounds so seriously. Let me elaborate.

[78] (Poincaré 1966, 203).
[79] An example of a presupposition of the *H*-theorem would be the **HMC**.

The **HMC** is an empirically well-justified interpretive hypothesis about the engine of entropic increase, *viz.*, collisions (Baxter and Olafsen 2007). Systems that abide by the **HMC** and the antecedent of the *H*-theorem approximate real-world systems much better than the idealized systems targeted by the mathematical models of modern collision theory. That collision theory *was* part of the statistical mechanics of Maxwell and Boltzmann (Weaver 2021, 45-49), and (again) *is* part of **MBSM**.[80] These varieties of statistical mechanics each face the challenges of resolving both the reversibility paradox and the recurrence paradox. I argued in (Weaver 2021) that to resolve the former paradox, one must appropriate the **HMC** as it is interpreted by **Causal Collisions**. Fortuitously, adding the **HMC** (with **Causal Collisions**) to one's statistical mechanics renders premise (2) of the argument from recurrence false and thereby provides a resolution to the recurrence paradox as well. It's simply not true that the constituents of gas systems do not actually involve real causal collisions.

The above said, my choice modern kinetic theory or statistical mechanics, does not do away with the idealized models of **MBSM**. The reassessed brand of Boltzmannian statistical mechanics—the version I adopt (call it **RBSM**)—takes on board all the mathematical formalism and modeling of **MBSM**. That is a benefit. **MBSM** has an impressive empirical track record, and I'd like **RBSM** to save all the phenomena that **MBSM** can. Yet, my understanding of the precise attitude one should have toward the idealized walk-around models of **MBSM** is indebted to both Bas C. van Fraassen's work on constructive empiricism[81] and the natural philosophy of Leibniz.[82] Talk of impact/collision parameters, azimuthal angles, and "collisions" without contact that enable recovery of post-collision trajectories or velocities that do not (*i.e*., the talk does not) explicitly represent (in the mathematical modeling) contacts that transpire during the crucial Δts should be interpreted literally (it is meaningful), and so too should talk of helpful potentials used to approximate the evolutions of systems that likewise avoid $r = 0$ cases. The relevant talk, however, should not be believed. There are no robust ontological implications of that portion of the modeling. That portion of the modeling departs from the real world. When two gas particles hit one another, they don't approach and then fade away without true contact. One's attitude about such modeling-from-a-distance should be one of **acceptance** (*i.e.*, believe that **MBSM**'s collision theory is empirically adequate), nothing more. If you want insight into what actually happens, you should add to your classical Hamiltonian mechanics an interpretation of classical collision theory, *viz*., the **HMC** as interpreted through the lens of **Causal Collisions**.

If you add the **HMC** (with **Causal Collisions**) to your classical statistical mechanics, your classical mechanics will become temporally asymmetric. This is because the **HMC** is a temporally asymmetric interpretive postulate. Does this mean I'm committing blasphemy? Am I suggesting that Hamiltonian mechanics is not time-reversal invariant? As in (Weaver 2021), I answer with an emphatic "No!". Time-reversal invariance is a feature of the partially interpreted mathematics of Hamiltonian mechanics. It is a mathematical property of the equations of motion amidst, *inter alia*, a specification of H as the Hamiltonian set equal (for conservative systems) to the sum of kinetic and potential energy represented by T and U respectively. Such identifications of functions constitute the partial interpretation of the theory as the theory came into the world (Ruetsche 2011). Partial interpretations are useful in pedagogical contexts. They allow students and experts alike to

[80] Neglecting collisions and interactions in **MBSM** is a common practice. See (Goldstein and Lebowitz 2004, 58 "one can neglect…the existence of interactions between the particles, although of course they still play a role in the dynamics now described by a succession of collisions…."). See also (Goldstein et. al. 2019, 28).

[81] See (van Fraassen 1980).

[82] See (Leibniz 1989, 124); (Wilson 2017, 116).

grasp enough of some successful theory or computational machinery to describe systems and make predictions. But if you're sufficiently realist, and you believe our best physical theories sometimes inform us about what the world is like, then you'll want more than a partial interpretation. You'll want to discern a physical theory's scientific ontology. You'll want to know what, according to the best interpretation of that theory, it is committed to, and what makes its laws approximately true. According to a Boltzmannian statistical mechanics that holds on to the **HMC**-laden H-theorem and **Causal Collisions** (*i.e.*, according to **RBSM**), what helps make true the second law and the *H*-theorem are temporally asymmetric causal collisions. Thermodynamic irreversibility (in appropriate contexts) is a consequence of temporally directed obtaining causal relations.

5.1 Sundman and Wang

Wasn't the three-body problem solved? Hasn't the problem of singularities been resolved for binary and ternary collisions? Did we not learn from Karl Sundman's (1873-1949) tremendous 1910 paper that the singularities in binary collisions can be surgically removed through a process of regularization?[83] And wasn't his result generalized (although not directly) to systems of n-bodies (where $n > 3$) by the fantastic work of Qui-Dong Wang in 1991?[84]

It is believed that Sundman solved the three-body problem by way of discovering the appropriate converging power series solution. To find that series, Sundman had to figure out how to handle singularities due to binary collisions. This is because (again, and as is well-known) collision-wrought singularities shrink the convergence radius of power series solutions. What Sundman did was take the system of equations that give you the motions of the system and, when dealing with binary collisions, morph them into a distinct system that represents binary collisions as something one can handle, *viz*., standard points. The intricate mathematical details are complicated, but the moral is that one performs the translation to ensure that one can obtain an analysis of the evolution of the system even after the collision by changing the equations of motion (altering the relevant independent variable). There are delicate questions about when one makes the relevant mathematical maneuvers because no one has been able to predict when one will encounter a binary collision given a set of initial data.[85] Of course, you *do* get a type of analytic continuation after binary collisions in Sundman. Sure. But the resulting elastic bounce and post-collision "motion" is something that is qualifiedly strange and unphysical. In their well-regarded discussion of collisions and the three-body problem, C.L. Siegel (1896-1981) and Jürgen K. Moser (1928-1999) asserted that the continuation provided by regularization "has no physical significance."[86] What's worse is that even after the regularization there are (in some contexts) new singularities to worry about which may not be collision singularities and which cannot be avoided by regularization. As Wang judges,

> Although with regularization one can define 'motion after a binary collision', the regularized system can admit other singular solutions for which the concept 'motion

[83] On Sundman and his work, see (Barrow-Green 2010, 180-198); (Siegel and Moser 1995, 19-90); See also (Saari 1990); (Saari 2005, 137-206) and the primary literature cited therein. On regularization, see (Marchal 1982). My exposition leans on this literature.

[84] (Wang 1991). *Cf.* (Babadzanjanz 1979) and (Babadzanjanz 1993).

[85] (Saari 1990, 115). Saari speaks very positively of Sundman's results.

[86] (Siegel and Moser 1995, 45). Elsewhere, Siegel stated that the "analytic continuation" provided by Sundman's regularization technique "has no physical meaning". (Siegel 1941, 432); *cf.* the similar point in (Barrow-Green 2010, 181-182 also citing Siegel 1941).

> after stop time' does not make sense. (See for example, Mather-McGehee's paper [4], where regularized collisions accumulate at a limit point.)[87]

Wang's important study of the $n > 3$ cases skirted around singularities and admitted to being unable to directly generalize Sundman's analytical technique.[88] In the context that concerns Wang's study, there are non-collision singularities to worry about too (Wang 1991, 76). I therefore find no successful rebuttal in the regularization literature and that without resorting to complaints about convergence times.

6 Conclusion

In (Weaver 2021), I showed that Boltzmann's H-theorem does not face a significant threat from the reversibility paradox. I have shown that my earlier defense of the H-theorem against that paradox can be used yet again for the purposes of resolving the recurrence paradox without having to endorse heavy-duty statistical assumptions outside of the **HMC**. As in (Weaver 2021), lessons from the history and foundations of physics revealed precisely how such resolution is achieved.

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[87] (Wang 1991, 74).

[88] "Because we know almost nothing about the complex singular point in the τ plane, it seems hopeless to try to improve the convergence of such a series" (Wang 1991, 87). Florin Diacu stated:

> "Quidong (Don) Wang, published a beautiful paper…in which he provided a convergent power series solution of the *n*-body problem. He omitted only the case of solutions leading to singularities—collisions in particular." (Diacu 1996, 69).

Appendix 1:

I include here an image of part of the *Acta Mathematica* announcement for the essay competition discussed in **sect. 4.1**. It is provided by Barrow-Green (1997, 229-230), but I take the image from the English translation produced in the July 30th, 1885 issue of *Nature* page 303. The announcement was forwarded to *Nature* by Mittag-Leffler:

> 1. A system being given of a number whatever of particles attracting one another mutually according to Newton's law, it is proposed, on the assumption that there never takes place an impact of two particles, to expand the coordinates of each particle in a series proceeding according to some known functions of time and converging uniformly for any space of time.
>
> It seems that this problem, the solution of which will considerably enlarge our knowledge with regard to the system of the universe, might be solved by means of the analytical resources at our present disposition; this may at least be fairly supposed, because shortly before his death Lejeune-Dirichlet communicated to a friend of his, a mathematician, that he had discovered a method of integrating the differential equations of mechanics, and that he had succeeded, by applying this method, to demonstrate the stability of our planetary system in an absolutely strict manner. Unfortunately we know nothing about this method except that the starting-point for its discovery seems to have been the theory of infinitely small oscillations.[1] It may, however, be supposed almost with certainty that this method was not based on long and complicated calculations, but on the development of a simple fundamental idea, which one may reasonably hope to find again by means of earnest and persevering study.
>
> However, in case no one should succeed in solving the proposed problem within the period of the competition, the prize might be awarded to a work in which some other problem of mechanics is treated in the indicated manner and completely solved.

Abbreviations:

BWAn	Boltzmann, Ludwig. *Wissenschaftliche Abhandlungen von Ludwig Boltzmann*, edited by Fritz Hasenöhrl (Leipzig: Barth, 1909), vol. *n*.
Œuvres n	Poincaré, Henri. *Œuvres de Henri Poincaré*, vol. *n*. (Paris: Gauthier-Villars, 1952).
SPMn	Maxwell, James Clerk. *The Scientific Papers of James Clerk Maxwell*, edited by W.D. Niven. (Cambridge: Cambridge University Press, 1890), vol. *n*.
ZCW1	Zermelo, Ernst. *Collected Works: Volume I – Set Theory, Miscellanea*, edited by Heinz-Dieter Ebbinghaus and Akihiro Kanamori. (Berlin: Springer-Verlag, 2010).
ZCW2	Zermelo, Ernst. *Collected Works: Volume II – Calculus of Variations, Applied Mathematics, and Physics*, edited by Heinz-Dieter Ebbinghaus and Akihiro Kanamori. (Berlin: Springer-Verlag, 2013).

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