Turing-Unsimulability, and instantaneous and sustained Cosmic-Censorship-falsity, for Einstein-Vacuum General Relativity

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Abstract:

(I) We construct instantaneous counterexamples to Penrose's "cosmic censorship conjecture" (CCC) in Einstein's vacuum field equations (EVFEs) in general relativity (GR).

(II) We also construct ones that persist for a positive timespan (e.g. 1 million years). More precisely, II demonstrates *either* (1) the existence of a solution of EVFEs – note, *no* matter is involved – for a million years, throughout which there are any desired arbitrary number (including infinity as a "number") of "naked" point-singularities, or (2) Einstein solutions suddenly stop existing, or (3) solutions of Einstein that ought to be well described by Newton-law dynamics, are not, or (4) "stability" of Newton law solutions does not work the way everybody thought based both on many experiments and KAM/Nekhoroshev mathematical theory.

Consequently, *if* Penrose's CCC is physically valid, *then* the reason is *not* Einstein gravity alone – some other physics must play a crucial role. The construction for II shows as corollaries that GR can have **everywhere non-analytic** metrical solutions, maximally-refuting an unfortunately-widely-believed myth; and also indicates that naked singularities arise from *generic* initial data – at least with some people's notions of the word "generic" (but possibly not yours).

(III) We sketch a proof of the "Turing unsimulability" of EVFEs. More precisely, either (1) the metric of spacetime time-evolves during a finite timespan (e.g. 1 year) in a manner which no Turing machine can simulate to within arbitrary user-specified accuracy bound in any finite timespan, or (cases 2, 3 basically same as in II), or (4) "chaos lifetime" in Newtonian 3-body scenarios behaves very differently than everybody had thought based on extensive experiments. It probably should be possible to get rid of case (4) via a different, chaos-avoiding, proof technique based on more-explicitly defined motions with perturbation bounds devised with computer aid – I sketch how but do not actually do this. The argument also suggests that unsimulability happens with *generic* initial data, at least with some people's notions of the word "generic" (but possibly not yours). All these scenarios I, II, III involve finite and bounded total mass-energy.

Crucial to I-III is the fact that the EVFEs permit storing an infinitude of information in a compact finite-volume region using finite mass-energy; and furthermore (for III) an infinitude that's dynamically *relevant*, i.e. changing any single bit of that information will yield an easily-observable macroscopic consequence within a fixed timespan. That mathematical fact probably is unphysical, in which case the EVFEs are not the laws of gravity in our universe, but rather only an approximation to truer (e.g. "quantum gravity") laws.

I believe case 1 is the truth in both theorems II and III; cases 2-4 were added to handle my inability to prove case 1 fully rigorously. (Theorem I, however, is fully rigorous and does not need extra cases.) Key obstacles to rigor: Humankind presently is usually unable to prove eternal existence and uniqueness of solutions to the Einstein equations; and cannot prove or disprove (for any particular N≥3) that a positive-measure set of Newton N-body solutions can exhibit "eternal chaos." And although there has been progress on problems resembling "proving stability of the solar system" for Newton N-body problems (at least in a Nekhoroshev long-time-survival sense), that progress has not yet been good enough to handle N=∞.

But regardless of which cases happen, I contend theorems I, II, III signify the *failure* of the EVFEs as an algorithmic theory of gravitational physics. Some lessons are drawn from that, e.g. everybody trying to combine standard model with GR wjhile keeping the latter nonquantum, is misguided. Also includes (a) an introduction reviewing previous works in my "computational complexity status of physics" aka "Church's thesis meets physical law X" research programme; (b) a long survey of useful facts about Newtonian N-body problems, in some respects the best currently available, and highlighting the important open question of whether a positive-measure set of "eternal chaos" N-body solutions exist.

Table of contents

The main new results are in §4 and 5.

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1. Introduction – Computational complexity status of physics

A multi-decade research programme of mine has been "Church's thesis meets physical law X."

"Church's thesis" aka the "Church-Turing thesis," is the hypothesis, foundational in computer science, that no physical system can exceed the computing power of a "Turing machine." I.e. the Turing machine is the ultimate computer: what it can compute, is "computable," and what it cannot, is "uncomputable"; and no physical system can solve Turing-undecidable problems in finite time. Furthermore, "*extended* Church's theses" hypothesize, more strongly, that Turing machines are optimal up to "polynomial slowdown" factors, versus any rival "computing system" permitted by the laws of physics in our universe.

Church's thesis should not be merely a hypothesis. It must be investigated. Alonzo Church and Alan Turing did the first such investigations. Turing 1936 gave arguments based on a set of naive assumed axioms about physics (he seemed ignorant of quantum mechanics, and as we shall see, he must not have thought deeply about general relativity either) that Turing machines were the most powerful possible kind of computer. Turing, Church and others proved "universality" and "simulation" theorems showing that various other kinds of computing machines, e.g. "cellular automata," Von Neumann RAM model, were not more than a polynomial factor stronger than a Turing machine.

One can try, for any given set X of mathematically defined putative "laws of physics," to prove or disprove Church's thesis. A disproof would be: exhibiting a physical system operating under laws X, which solves Turing-undecidable problems in 1 hour (or some other, possibly problem-dependent, always-finite timespan). A proof would be: design simulation algorithms, which run on a Turing machine, and provably simulate any physical system operating under laws X, to arbitrary-user-specifiable accuracy, with finite "slowdown factor" that depends on "resource bounds." For some law-sets X, I was able to succeed.

I. With Newton's laws of motion and gravity for a finite set of point-masses (<100 masses should suffice) in the Euclidean plane, I proved in 1993 using an argument structured around J.Gerver's 1991 construction of an N-body "non-collision singularity," that there exists a set of starting configurations with bounded total mass, #particles N, and total energy, such that in the next hour, they will time-evolve according to an infinite set of topologically-distinct never-colliding trajectories. Indeed, an *uncountably* infinite set. Meanwhile, a Turing machine, in finite time, can only describe, or simulate, a *finite* set of trajectory-topologies. If the initial data (masses, positions, velocities) were encoded as real numbers using suitable "redundant binary" number-encoding systems on a finite set of infinitely-long input tapes for a Turing machine, one tape per real number, then: one could design Turing machine simulation algorithms which read all the input tapes up to position N, then simulate the motions of the particles during the next hour of their time, then output a description of what happened and/or where they ended up and/or the answer to some yes/no question about the trajectories – e.g. did they ever enter a particular circle ("yes") or never enter a twice-as-large concentric circle ("no") or "don't know" because did not have enough accuracy. (We also can make "promise problem" scenarios where I promise the truth really is either "yes" or "no"; if I lied, then Turing machine is permitted to deliver a false answer.) If "don't know" the Turing machine could go back, read more input bits (say up to position 2N) and try again. It still might not have enough accuracy, in which case it could try again (say now reading to position 4N) and so on. In my proof's nasty starting configurations, that never ends and can never end: the Turing machine must keep asking for more input forever, and never can attain enough accuracy. No matter how many bits of input it read, there still are uncountably-infinitely many topologically-distinct trajectory-types compatible with that partial-input, some of which do enter the inner circle, others of which never enter the outer circle. Suitable input bit-strings that cause this pathology to happen are producable by a second Turing machine which can supply more (as many as wanted) on demand, forever, and that second Turing machine's input could be only finitely long. In that sense, **with Newton's laws of gravity/motion for N point masses, Church's thesis is *false*.**

However, that proof shamelessly exploited certain features of Newton's laws for point masses, that are physically unrealistic. That is: Newton's laws are not the true physical laws in our universe. They are only an approximation to the true laws. Truer laws would involve, e.g. the fact from relativity that all particle speeds are bounded by the speed *c* of light. (In my proof, all *initial* speeds were slow, but particles reached unboundedly high speeds during the next hour, far exceeding *c*.) Another fact is that "point" masses M have a non-zero "Schwarzschild radius" $2GM/c^2$. A third reality Newton ignored is quantum mechanical "Heisenberg uncertainty principles."

In the same paper, I also showed that if Newton's laws were replaced by certain other laws bearing a greater resemblance to physical reality (e.g. knowing about *c* as a speed limit) then I could produce simulation algorithms. Thus, the Church-thesis investigation, alone, could have suggested to us the inadequacy of Newton's laws and thus suggested the need for relativity and/or quantum mechanics. That would have been an alternate history rather different from the actual history of science, e.g. perhaps involving A.Church and A.Turing, instead of A.Einstein and W.Heisenberg. I bring up that alternate history not merely because it is an enjoyable fantasy. It suggests something important: Church thesis investigations have the potential to usefully *guide* theoretical physicists in their quest to find the True Laws of Nature.

Aside about possible extensions to my 1993 proof. My original proof had been based on Gerver 1991's noncollision singularity. Xue 2020 and Gerver-Huang-Xue 2022 devised noncollision singularity proofs involving only N=4 bodies, which Painleve in 1897 had shown to be the minimum possible N. Therefore, I presume that my proof could be redone (after sufficient effort) to show the unsimulability of Newtonian 4-body problems. An open *question* I had raised in my 1993 paper was whether the unsimulable problems, or noncollision singularities, necessarily corresponded to *zero-measure* subsets of initial conditions. I can now tell you that when N=4 the answer is "yes" due to results by D.G.Saari and A.Knauf indicating 4-body noncollision singularities have measure zero (and for any N, *collision* singularities in $\mathbb{R}^d$ with d≥2 *also* arise only from zero-measure sets of initial conditions, e.g. Fleischer & Knauf 2019); and the known fact that any singularity necessarily involves some interparticle separation reaching zero, while for motions without singularities there exists δ>0 such that every interparticle separation stays ≥δ. Whenever such a δ>0 exists for an N-particle motion over a specified timespan, that motion (a) is simulable and (b) the maximum such δ is a "computable real number." But for N≥5 my question currently remains open.

Aside about 1-dimensional billiards with an infinite number of billiard balls. I recently learned about works by Atkinson & coauthors (see references for Atkinson's papers 2007-2014) on the Newtonian mechanics of point-mass "billiard balls" on the 1-dimensional real line. If you do not like points, then Atkinson's masses can instead be made to be non-overlapping closed line-segments with positive widths (e.g. proportional to their masses). If you do not like Newtonian billiard balls, Atkinson also considers Einsteinian special relativistic ones. Masses always are positive reals. Atkinson supposes that the only way his masses interact is via binary collisions, and supposes all his masses are "hard" and their collisions "elastic" i.e. each collision preserves both energy and momentum. There are no other interbody forces, for example zero gravity.

Interestingly, Atkinson et al were able to design scenarios involving a **countably-infinite** number of billiard balls in which, after a finite amount of time, energy ceased to be conserved; and also he could cause their motion to be **nondeterministic**, and also he could cause **unboundably-large velocities** to occur within finite time in the Newtonian case (subluminal velocities arbitarily near lightspeed in the Einsteinian case) even in situations where all initial |velocities| were bounded below an arbitrarily small positive constant. This is despite the fact that in any scenario with only a finite number of billiard balls performing a finite number of always-binary collisions, energy and momentum are always conserved and the motion always is deterministic and the velocities always finite.

> **Example 1** (J.P.Laraudogoitia 1996; description quoted from Atkinson & Peijnenburg 2014 with slight editing): "An infinite number of identical point masses (balls) are placed at the Zeno points 100, 100/2, 100/4, 100/8, ... on the real line. All the balls are at rest except the first, at 100, which moves with constant speed towards the second, at 100/2. After elastic collision the first ball comes to rest, passing all its kinetic energy on to the second ball, which soon collides with the third, which acquires all the energy, and so on ad infinitum. However, after the finite time that it would have taken the first ball to reach the point 0, had the other balls not been in its way, every ball will have moved briefly, but then been brought to rest. After all motion has subsided, the energy and momentum of the balls have *disappeared* without trace!
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> And since classical mechanics is time-reversal invariant, a video recording of the above scenario of collisions, run backwards, should also depict a possible mechanical evolution that is consistent with the Newtonian equations. Such an evolution begins with an infinite number of identical balls at rest. At a certain nondeterministic moment motion arises spontaneously, out of the origin, and balls move to the right, successively passing on the motion to their rightmost neighbor until the last ball carries off all the energy and momentum."
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> [A different, also interesting, version involves all balls at rest except for one additional rightward-moving ball started at x=**–**100. What happens?]
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> **Example 2** (D.Atkinson): For n=1,2,3,... place point masses $m_n=2/([n+1][n+2])$ at the points $x_n=100/n$ on the real line [note $\sum_{n\ge1} m_n=1$]. All the balls are at rest except the first, at 100, which moves with some constant subluminal speed towards the second, at 100/2. In this case under Newtonian mechanics momentum is conserved, but after finite time an infinite number of collisions occur during which half the initial energy is lost and unboundedly high ball speeds are reached. Under special relativistic mechanics with either those m_n or instead $m_n=2^{-n}$, after a finite time an infinite number of collisions occur during which a positive fraction (depending on the initial speed and which m_n formula is used; Atkinson calculates some numbers) of both energy and momentum are lost; ball speeds approach lightspeed arbitrarily closely although they all remain subluminal. (And again we also may consider the "time-reversed video," which can involve a nondeterministic amount of energy introduced ex nihilo.)

I now want to point out that this situation can be **rescued**. (Atkinson himself had already thought of this rescue idea in embryonic form; I think my version of it works in full generality.) My rescue is to demand that such 1D billiard problems initially have

i. finite summed mass,
ii. finite summed kinetic energies,
iii. finite summed |momenta|,
iv. finite infemal and superemal ball-locations and finite infemal and supremal ball-velocities,
v. $|x_j-x_k|>|m_j+m_k|$ if j≠k (initial separations exceed summed gravitational radii)
vi. all that remains true even if we make our initial positions x and momenta p have "intervals of **uncertainty**" whose positive widths obey Heisenberg-style "uncertainty inequalities" $\Delta x\Delta p\ge1$ and we demand finite total energy, finite ∑|momenta|, and ball non-overlap for *all* configurations allowed by those uncertainty intervals,
vii. no matter which configurations (allowed by those uncertainty intervals) we choose, we still have the same ordering of all masses along the real line, and the same time-ordering of all binary collisions (at least within some specified time-interval Ω).

Call configurations disobeying this demand-set "ill posed." I believe: *well-posed* 1D billiard problems conserve energy and momentum, keep all |velocities| bounded, and remain deterministic throughout the time-interval Ω. Indeed, I believe i-vii prevent any infinite set of masses from even *existing* within any finite-width real position-interval if during Ω every ball "communicates" (perhaps indirectly and perhaps only one-way) with every other. And certainly all Atkinson's paradoxical examples are ruled out.

Parts (vi) and (vii) of this rescue are crucial (in the sense that Atkinson's paradoxes still work under demands i-v) and provide an interesting toy illustration of the ability of "quantum mechanics" to "save" nonquantum mechanics from foundational mathematical problems.

II. I next proved in 1999 that the quantum mechanical N-body problem, involving N point masses interacting via a certain allowable class of pair potentials (including the classic Coulomb inverse-square force law) evolving under Schrödinger's nonrelativistic equation, *was* Turing-simulable, i.e. Church's thesis was *true*. In this second paper, I had to contend with the fact that quantum mechanics is interpreted probabilistically. I therefore granted the Turing machine access to a random-bit generator. The Turing machine would simulate the N-body system then answer a question (most simply, a yes/no question) about it corresponding to the result of some physical "measurement," thus producing a random output (most simply, a single bit). This could be done repeatedly by re-running the Turing machine arbitrarily many times (the run-number 1,2,3,... being an additional Turing machine input), thus producing a set of arbitrarily many independent samples from the probability distribution of output-answers. Meanwhile, a real physical system could be run in a lab, measured, and that whole experiment repeated arbitrarily many times, thus also producing a set of arbitrarily many independent samples from its probability distribution of output-answers. My theorem was: Turing machine simulation algorithms exist, which cause these two probability distributions to be statistically indistinguishable; i.e. the hypothesis that the simulations were exact cannot be disproven with high statistical confidence, no matter how many re-runs are performed. (The simulations actually are *not* exact, but the Turing machine tries harder to get more accuracy on run R+1 than it tried on run R, and in such a way that it succeeds in achieving statistical indistinguishability and hence apparent/effective true exactness, for all R, including R→∞.) This proof employed "rigorous quantum mechanics."

So, rather stunningly, **the quantum N-body problem with N point-masses is actually computationally *easier* than the Newtonian N-body problem; the Church thesis with those laws of physics is true.** (This also was a stunning success in some alternate history of science...)

III. In my third paper in this programme (2001), I tackled the "Navier-Stokes equations of hydrodynamics" as the investigated "physical law." Here, I encountered the problem (which I was not able to overcome) that humanity currently is just too stupid to be able to rigorously prove almost anything about the Navier-Stokes equations. We will also encounter the analogous problem in the present paper: humanity currently is too stupid to be able to rigorously prove almost anything about Einstein's field equations!

To explain what I mean by "too stupid": for both Navier-Stokes and Einstein-vacuum-GR: nobody has ever been able to prove eternal existence and uniqueness of solutions to those equations, arising from time-evolving any respectably-large class of possible initial data. And if you cannot even prove existence and uniqueness of solutions, you pretty much cannot do anything.

> Indeed, as a philosophical matter: is it even legitimate to call Navier-Stokes and/or Einstein-vacuum-GR "laws of physics" while existence and uniqueness of their solutions is unknown? I would contend that the answer is "no" and at best these should be considered "tentative" laws of physics. And if existence and/or uniqueness were disproved, then that would show they were *not* acceptable as "laws of physics" – and the only way they could be accepted is if some extra ingredients (i.e. new laws) were added.
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> "Constructivists" would go further and argue that unsimulable laws of physics were not acceptable "laws of physics" even with some future nonconstructive existence/uniqueness proof. I do not take that stance. I instead would contend that they were "*unsimulable* laws of physics." And if the true laws of physics in our universe indeed are unsimulable, then that is a sad – but possible – reality.

There are many computers out there allegedly approximately computing Navier-Stokes solutions for e.g. ships and airplanes, and Einstein solutions for e.g. colliding or nearly-colliding black holes. They output alleged results, which various people then hope accurately reflect whatever those equations would truly do. However, *there is not one drop of rigor in any of that. Nobody currently knows how to do such simulations while producing any accuracy guarantee whatsoever.*

In my opinion it is a crucially important problem to devise algorithms to simulate physics with rigorous guarantees of accuracy, and that problem ought to be a central focus of mathematics. I've been saying that practically my whole life, but never saw much sign many people were paying much attention.

What *can* humanity rigorously say? If you can find *exact* Navier-Stokes or Einstein solutions, then they are rigorous. Unfortunately, usually you cannot. I think (as an order-of-magnitude rough estimate) that humanity so far has been able to produce roughly 100 exact Einstein and exact Navier-Stokes solutions, and that's all.

Also, for smooth-enough initial data *close enough* (in certain precise norm-senses) to a few especially nice known exact solutions, humanity has developed the power to prove eternal existence and uniqueness. Specifically, for Navier-Stokes, Leray 1934 proved we enjoy eternal existence (but not necessarily uniqueness) of a "weak solution." And with initial data *close enough* to "everything is stationary," Scheffer, Cafarelli, Kohn, Nirenberg proved (and Lin 1998 improved their proof) that Leray's "weak solution" becomes both regular (i.e. a genuine solution) and unique. For Einstein vacuum-GR, with initial data *close enough* to "Minkowski flat spacetime," Christodoulou & Klainerman, and later Klainerman & Nicolo 2003 in a more streamlined version, proved eternal existence and uniqueness. Work by Klainerman and others also allegedly (2022) has accomplished this for the Kerr rotating black hole metrics with sufficiently small angular-momentum parameters. This also was done for Einstein vacuum equations with repulsive cosmical constant Λ for the "de Sitter spacetime" by Friedrich 1986, and for the "Kerr-de Sitter black hole metrics with small angular momentum" by Hintz & Vasy 2018. Those were very difficult and long proofs (totalling over 1000 pages!) which very few people can understand or produce. And unfortunately, from the practical point of view, almost nobody cares. The reason almost nobody cares is: the "close enough" bounds those theorems need to assume as step 1, are just *too* close. Almost nobody *wants* to solve the Navier-Stokes equations with initial data that close to stationary, because those simply are not interesting and useful fluid flows. Similarly, almost nobody cares about solving Einstein vacuum-GR in those "close to flat" scenarios, because they simply are too boring.

In these boring scenarios, the nonlinear terms in the Navier-Stokes and Einstein equations become small, so we "nearly" have linear PDEs. That was key to enabling the existence/uniqueness proofs to work, but also ensures their almost total lack of practical interest.

So given this human-stupidity-problem, how can I accomplish the Church-investigation programme for Navier-Stokes or for Einstein vacuum-GR? My answer: "by **cheating**"! Specifically, in the case of Navier-Stokes, I proved a 3-case theorem statement; the other two cases handle scenarios where my proof of the main case might fail due to the nonrigor in my arguments for it. Basically, my Navier-Stokes theorem was as follows:

> I proposed a class of 3D hydrodynamics problems involving the flow of "idealized water" (a constant-density constant-viscosity zero-surface-tension never-evaporating-or-freezing fluid) partially filling a stationary compact rigid-walled container, during the next hour. The container walls and initial velocity and vacuum-water-boundary data are smooth everywhere *except* at a single point. The initial data has finite total energy and finite maximum speed and is finitely describable. We can ask a yes/no promise-problem question such as "during the next hour, will more than 2 liters of water enter a certain chamber, or less than 1 liter?" (I promise one of these two alternatives will happen; and if I lied, then the Turing machine fluid-simulator is allowed to output any answer.) Either:
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> 1. The fluid flow during the next hour will, by answering the yes/no question, solve Turing-undecidable questions. In other words, this class of Navier-Stokes hydrodynamics problems is Turing-*unsimulable*, so Church's thesis with those laws of physics is false.
> 2. Sometime during the next hour, the Navier-Stokes equations will fail to have a solution.
> 3. My container-shape involves a lot of pipe and joint "fluidic logic components"; and the behavior of the Navier-Stokes equations inside those components is (in case 1 above) postulated to correspond to certain experimentally-known behaviors all (or at least 99.99% of) the time. (I was able to incorporate "error correction" into the proof so that a small rate of random component failures, e.g. perhaps 0.01%, could be tolerated.) The present third case is: such too-large component-error-rates happen, and hence Navier-Stokes *equation* behavior *disagrees* radically and experimentally-obviously versus *experimental* behavior in a few heavily experimentally-investigated scenarios (where literally millions of experiments have been tried with zero failures).
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> Furthermore, I was able to extend the proof to make fluids solve "higher level undecidable" problems in 1 hour, by using somewhat fancier classes of container-shapes.

I believe the truth is case #1. But I contend that no matter which of those three cases happen, this theorem has demonstrated the **failure of Navier-Stokes hydrodynamics to be an algorithmically useful physical theory**.

My proof shamelessly exploited certain features of Navier-Stokes hydrodynamics, that are physically unrealistic. Namely, actual water is made of molecules, i.e. is *not* a continuum down to arbitrarily tiny length scales. And it does not truly have constant density and constant viscosity regardless of how hard you whack it and how hot it gets.

IV. A lot of research on **"quantum computers"** can be regarded as another branch of my same Church/physics research programme – but the vast majority of it was done by others, not me.

Algorithms for factoring N-digit integers into primes using a hypothetical "quantum computer."

Who	When	Bound on expected qubit ops	Bound on qubit count
Peter W. Shor	1994-1999	$O(N^2 \log N \log\log N)$	$O(N \log N \log\log N)$
Craig Gidney, then Gregory D. Kahanamoku-Meyer & Norman Y. Yao	2017-2024	$O_\varepsilon(N^{2+\varepsilon})$ for any fixed ε>0	O(N)
Oded Regev, Seyoon Ragavan, Vinod Vaikuntanathan	2024	$O(N^{1.5}\log N)$	O(NlogN)
Oded Regev, Seyoon Ragavan, Vinod Vaikuntanathan	2024	$O_\varepsilon(N^{1.5+\varepsilon})$ for any fixed ε>0	O(N)

P.W.Shor discovered in 1994 (paper in 1997) that a hypothetical "quantum computer" with O(NlogN) qubits could **factor arbitrary N-digit integers into primes** in an expected number of "qubit operations" bounded by $O(N^2\log N\log\log N)$ on a quantum computer with O(NlogNloglogN) qubits. In the 1999 version of Shor's paper and/or in later authors' recountings, the loglogN (and sometimes even logN) factors mysteriously vanished. Later alleged improvements are shown in the table. All of these quantum algorithms enjoy *polynomial* expected runtime.

Meanwhile, as of year 2025, the asymptotically fastest known integer-factoring algorithms on old-fashioned nonquantum kinds of computer are the GNFS methods, which *conjecturally* (and the conjecture is compatible with empirical experience) run in expected time

$$\exp(C N^{1/3} (\ln N)^{2/3}) \quad \text{where} \quad C=9^{-1/3}(\ln 10)^{1/3}4+o(1)<2.54.$$

This conjectural runtime, asymptotically when $N\to\infty$ is much slower than any polynomial(N). It is widely **conjectured** that no expected-polynomial-time integer-factoring algorithm exists on Turing machines with access to random bit generators, i.e. **integer factoring is superpolynomially hard**. But proving that conjecture would require proving P≠NP (or its randomized version). Such complexity-class separation claims as P≠NP and P≠PSPACE, while believed by 99% of computer scientists, have remained open since they were first formulated in the early 1970s, despite a million-dollar prize offered by the Clay Research Institute. Only much cruder complexity-class separations have been provable, such as (one of the crudest, and the earliest, by A.Turing in 1936) "Turing undecidability."

Under this factoring-is-superpolynomially-hard conjecture: if the laws of physics permit building quantum computers with arbitrarily many qubits, then Shor et al **disproved the *extended* Church thesis**. However, in Smith 2003 I raised the **objection** that the laws of physics in our universe might *not* allow quantum computers to have more than a constant number of qubits. If so, then quantum computers could not asymptotically outperform old-style computers by more than a constant factor – making the extended Church thesis safe from them. As I explained there, this question depends on the **nature of "decoherence."**

Even if my objection is correct (which I suspect it is!) the "constant number of qubits" upper-bound might well be large-enough (for example, 10^{40}) so that, for practical purposes, nobody cares! If so, that would call into question the whole claim of the "foundational importance" of Church's thesis, and indeed the "importance" in computer science of asymptotic "big O" runtime claims about algorithms generally.

Meanwhile, despite 30 years of research funded by billions of dollars, nobody has ever managed to factor any integer$\ge 2^{35}$ on a quantum computer (Willsch et al 2024) – a feat my PC can accomplish in well under a millisecond using an 11-line algorithm from 1975.

V. That brings us to the present paper, about Einstein vacuum GR. (I also plan a future book **VI** about quantum field theory, but cannot discuss that here.) I am going to state an alleged theorem which incorporates features like those in my prior Newton-N-body theorem *and* in my prior Navier-Stokes theorem. Specifically, it uses its own "cheat" of having a 3-case theorem statement with the other two cases intended to handle scenarios where my proof of the main case might fail due to the nonrigor in my arguments for it. And it re-uses some (certainly nowhere near all, but some) ideas from my prior Newtonian N-body paper. Any serious reader of the present paper will need to read my prior Newtonian N-body paper (and Gerver 1991) first to assimilate those ideas. And I will provide a sketch (which admittedly will *be* sketchy!) of a proof. The sketchiness bothers me very little because I am familiar with the ideas from my prior Newton N-body and Navier-Stokes proofs (which were *not* done sketchily) which I will reuse here; and with various phenomena commonly agreed to happen in general relativity... so I know it works. However, it might bother somebody who lacks that familiarity.

> **MAIN "PHYSICIST'S THEOREM."** I propose a class of asymptotically-Minkowski-flat, topologically the same as flat space, Einstein vacuum-metric initial data, containing finite total mass-energy, which nowhere distorts time by more than a positive constant factor versus the t-coordinate in flat (t;x,y,z) spacetime, and containing *no* matter, electromagnetic fields (nor any other kind of field, e.g. gluons or Higgs), or black holes. This initial data is bilaterally symmetric about a mirror-plane, is finitely describable and is C^∞-smooth except at a single point – that point is a "naked singularity." We can ask simple yes/no promise-problem questions about the next week of time-evolution according to Einstein's classical GR equations. For example: during the next week, will at least one black hole with mass>10 solar masses enter a ball centered at your current location with radius 1000 km, or will *no* holes with masses>1 kg enter the 2000 km radius concentric ball? (I promise one of those two alternatives will happen, and if I lied then the Turing machine conducting the simulation is allowed to output any answer. Incidentally, observe that the answer to this question will have life or death importance for you.) My time evolution will first create black holes, which are intended to (and in "case 1" below will) move around, staying within the plane of reflection-symmetry, in a manner highly accurately described by Newton's laws. Then *either*:
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> 1. The metric evolution during the next week will, by answering the yes/no question, solve Turing-undecidable questions. Furthermore, this metric evolution will involve an infinite set (indeed, uncountably infinite) of possible trajectory-topologies for a set of black holes that get created (and I can promise that they never collide), but a Turing machine can only describe a finite set in any finite amount of runtime. In other words, this class of (3+1)-dimensional Einstein vacuum-GR problems is Turing-*unsimulable*, so Church's thesis with those laws of physics is false.
> 2. Sometime during the next week, the Einstein vacuum GR equations will fail to have a solution.
> 3. Newtonianism does not happen, i.e. the Einstein GR equation black-hole motion & behavior will unexpectedly disagree radically with Newton law predictions, and/or those Newton law predictions will unexpectedly disagree radically with highly-believed conjectures (Lecar's law) about how Newtonian N-body solutions behave.
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> Furthermore, I suspect one should be able to extend the proof to make Einstein vacuum GR solve "higher level undecidable" problems in finite time, by using somewhat fancier classes of initial metric now involving fancier naked singularities *not* just located at a single point (although I won't describe that, or even define the notion of "higher levels" of undecidability, here... the ideas needed are similar to my Navier-Stokes/Church paper).

I believe the truth is case #1, and in particular I believe that my particular class of naked singularities will *not* prevent unique eternal existence of an Einstein solution. But I contend that no matter which of those three cases happen, this theorem has demonstrated the **failure of Einstein vacuum GR to be an algorithmically useful physical theory in every situation.**

A critic could attack that by claiming, or hoping, that initial data like mine cannot physically happen, and in particular, "naked singularities" cannot happen. Roger Penrose in 1969 in fact famously proposed a somewhat vague "cosmic censorship hypothesis" that naked singularities are not physically achieveable, and singularities in GR can only arise safely "cloaked" inside event horizons where they cannot be seen by, or affect, external observers. This hypothesis can be and has been verified in various specific cases. The reason I call this hypothesis "vague" is that counterexamples have been devised, or allegedly devised, to precise versions of the censorship conjecture (e.g. Christodoulou 1994/1999; Harada, Iguchi, Nakao 2002; Joshi & Malafarina 2015)...

> "The cosmic censorship conjecture is the biggest open question in nonquantum general relativity theory. The idea is that naked singularities should not evolve from regular initial conditions. But a sharp formulation of the conjecture – not to mention a proof – remains elusive."
> – Vincent Moncrief & Douglas M. Eardley: The global existence problem and cosmic censorship in general relativity, General Relativity and Gravitation 13,9 (1981) 887-892.
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> Actually, I contend that my theorem's class of initial data provides ***another* such cosmic censorship counterexample**, because it seems pretty obvious that this class of data and that kind of naked singularity *would* be physically achievable with the right laws of physics involving exact Einstein GR. Unlike all the prior counterexamples I am aware of, mine is rather like an "essential singularity" in complex analysis, as opposed to all the prior ones, which resemble much more prosaic, e.g. algebraic, singularity types in complex analysis. If we consider (which is easier to think about) the 1D real line, the real-valued function $X^2\sin(1/X)$ has an essential singularity at X=0 with a certain spiritual resemblance to the initial data in my Einstein

proof, but is analytic for all complex X≠0. In contrast, functions such as $X^{-1/3}$ or log|X| are more spiritually like other people's prior C.C. counterexamples. I'll explain a C.C. counterexample of my kind in a special warm-up section. I can design versions of it which "persist" for long timespans, e.g. a million years, while everybody else's lasted only one instant; and it works in pure vacuum, while everybody else's needed some sort of "matter" postulated to obey some collection of properties and hence were not CC counterexamples in *pure* general relativity.

Historically, community response to those C.C. counterexamples has basically been "your initial data was not physically-realistic enough, so if I add more 'physical-realism' assumption-demands to the conjecture statement, then your counterexample showing creation of a naked singularity will be excluded." This process was iterated, with more counterexamples allegedly found, then more uglier and uglier assumption-demands added to exclude them... making the C.C. conjecture a moving target! Given that history, it certainly is no longer clear, at least to me, exactly what we even want to conjecture. Anyhow, at least from the computer science perspective, my C.C. counterexample really seems to kill CCC really really dead.

I believe that the real lesson of my Theorem is: it **demonstrates the inadequacy of Einstein GR**, even in asymptotically-flat vacuum, as a physical theory, and demonstrates the necessity for a better theory of gravity, probably some kind of "quantum gravity." Recently certain theoretical physicists (whom I will leave nameless except to say they're probably idiots) to the accompaniment of worldwide hype, have been proposing the idea that gravity is *not* quantum and yet somehow happily coexists with a quantum theory of the other forces (e.g. the "standard model"). To them I say: "You lose!!"

More precisely: *Any* physical theory, which in the absence of matter reduces to classical Einstein gravity, is (1) nonalgorithmic and (2) includes persistent naked singularities. Therefore many people, in particular constructivists, would (or should) regard it as an unacceptable physics theory – at least if a rival algorithmic theory is devisable.

My proof will shamelessly exploit certain mathematical features of Einstein GR, that I believe/suspect are physically unrealistic. E.g, I suspect physical spacetime metrics *cannot* contain an infinite sequence of arbitrarily-tiny-mass black holes, say mass 2^{-k} for the k^{th} hole. Presumably, it is not physically possible for any black hole to have mass much below the "Planck mass" (about 21 micrograms), and the reason for that impossibility is "quantum gravity." Certainly, far-tinier mass black holes, e.g. only 1 nanogram, would be permitted by, and would eternally exist in, non-quantum Einstein GR alone, and could in principle be created using particles available in the standard model. The Schwarzschild metric is a fully rigorous exact solution whose mass parameter M can be made arbitrarily small.

One final remark before we begin explaining the proof. A stunning paper by Bredberg, Keeler, Lysov, and Strominger came out in 2011 making some mind-blowing claims that Navier-Stokes hydrodynamics actually arises as a limit case "hidden inside" Einstein's vacuum equations in 1 spatial dimension higher. (E.g. 2D Navier-Stokes hydrodynamics arises inside 3+1 dimensional Einstein vacuum GR.) There have been about ten follow-up papers, including Bredberg & Strominger 2012 and Zhang et al 2012. (I do not necessarly endorse any of those papers, because I have not examined them in enough detail; but at least some of them are probably correct.) Note that while water is *not* perfectly described by Navier-Stokes hydrodynamics (since water is made of molecules and not a true continuum), the Einstein vacuum metric is postulated by Einstein to truly be a continuum. Thus, ironically, Navier-Stokes describes this fake fluid better than it describes real fluids!

I ended up *not* using the Bredberg et al papers or their ideas here. But they did re-inspire me to re-examine the Einstein-Church problem after 20 years of inactivity on it – this time largely successfully.

2. Preliminaries: Long survey of useful facts about Einstein Gravity, Newton Laws, 3-body problem

This long section contains a potpourri of facts for later use, mainly about the Newtonian 3-body problem. In some respects this is the best available N-body problem survey. Hopefully the reader will enjoy a highlights tour of the 400-year historical development of celestial mechanics and gravity; and I guarantee that every reader will learn *something* they did not know. Celestial mechanics is one of the oldest (and therefore grandest?) branches of mathematics, but surprisingly simple questions about it remain open. *But*: you might find this section annoyingly loaded with facts perhaps seeming irrelevant to the final goal. Impatient readers who think they know everything could plunge directly into sections 4 and then 5, which are (a) shorter and (b) the core of our new results, but (c) assume familiarity with the background material in the present section! That strategy would be a gamble that the hyperlinks in those sections back into this section (and to the appendices) will get you out of trouble. Certainly, the general plan of attack (as opposed to the details) of the main ideas in the proofs of our main results ought to be comprehensible to any educated astronomer.

$G\approx 6.674\times 10^{-11}$ meter^{3}kg^{-1}sec^{-2} shall denote Newton's gravitational constant and c=299792458 meter/sec the speed of light.

2.1 Early History. Obviously, the earliest humans knew about the Sun, Moon, and many stars. The ancient Babylonians were already aware of the planets Mercury, Venus, Mars, Jupiter, and Saturn around 2000 BC. It appears (Ronan 1991) the first telescope (combined refractor/reflector) was invented and built by Leonard Digges probably sometime between 1540 and 1559 and may have been kept a military secret by Queen Elizabeth of England. Galileo Galilei (1564-1642) built and used one of the first telescopes, discovering Jupiter's 4 largest moons in 1610. Galileo also observed Saturn but failed to realize that it had rings (only recognizing "odd changes in shape"); and observed Neptune on 28 December 1612 and 28 January 1613, but failed to publicize that discovery as a "new planet" – causing it to be forgotten until Drake & Kowal 1980 unearthed this from Galileo's notes. Tycho Brahe (1546-1601), using advanced naked eye instruments he built himself (far superior to any up to his time), recorded planetary and lunar positions for over 20 years accurate to around 1 arc-minute (Wesley 1978). Johannes Kepler (1571-1630) studied Brahe's data and during 1604-1619 deduced his three laws of planetary motion along ellipses with one focus at the sun. Isaac Newton (1643-1727) invented his laws of motion, gravity, and the calculus, and was able to prove that a spherical shell of matter with uniform areal density would produce the same gravitational attraction (outside it) as if its mass were replaced by a single point at the sphere center; and therefore this also is true for any spherically-symmetric distribution of matter. After that it was not difficult to verify that Kepler's laws (with the right constants inserted) were a consequence of Newton's when applied to sun-planet 2-body problems. (See Littlewood 1953 for a way to redo Newton's sphere proof, and Feynman, Goodstein, Goodstein 1996 for a way to redo Newton's ellipse proof, both better than Newton but using only Newton's own sort of ultra-elementary methods.) Actually solving the 2-body problem ab initio is harder than merely verifying an alleged solution, but in 1710 J.Ermanno, a student of Johann Bernoulli, was able to use Leibnizian calculus to find the orbits for inverse square force law directly; along the way he was the first to obtain what later was misnamed the "Runge-Lenz vector." This all made it clear that Kepler's "third law" governing planet orbital periods had been stated incorrectly, although correct to within ±0.1% error in our solar system. Christiaan Huygens first realized Saturn had rings in 1655 (published 1659) and discovered its moon Titan. F.William Herschel used a large self-built telescope to discover the 7th planet Uranus in 1781. Uranus is bright enough to be visible with the naked eye, but had been missed. Herschel also was the first to discover binary and triple stars, producing catalogues of hundreds of them in 1782, 1784, and 1821, and realizing after 25 years of observation that they were *rotating* gravitationally-bound systems, explainable at least roughly by Newton's laws.

In 1887 came the **Bruns-Poincare-Julliard theorem:** For any fixed N≥3, the only "constants of the motion" for the Newtonian N-body problem that are *algebraic* functions of the time, momenta, and positions of the N bodies (each assumed to have positive mass), are algebraic functions of these classic 10=3+1+6 constants of the motion:

$\vec{L}$=Angular Momentum (3), E=Total Energy (1), and the position of the Center of Mass as a Linear Function of time (6).

Expressed in terms of the position $\vec{x}_j$ of the j^{th} mass m_j and its velocity $\vec{v}_j$, these are: $\vec{L}=\sum_j m_j\vec{x}_j\times\vec{v}_j$, $E=\sum_j m_j|\vec{v}_j|^2/2$, and the center of mass is $\sum_j m_j\vec{x}_j/\sum_j m_j$.

Newton's laws are: $m_j\, d^2\vec{x}_j/dt^2 = \sum_k \vec{F}_{jk}$ where $\vec{F}_{jj}=\vec{0}$ but if j≠k then $\vec{F}_{jk}=Gm_jm_k\,(\vec{x}_k-\vec{x}_j)\,|\vec{x}_k-\vec{x}_j|^{-3}$.

This theorem was proved by Ernst H. Bruns in 1887 (explained in English by ch.XIV of Whittaker 1937) with later correction of Bruns' most important error by Henri Poincare, further corrections by Wm.D.MacMillan in 1913, and further corrections and additions by E.Julliard-Tosel in 2000. All the classic 10 arise via Noether's theorem from the symmetries of the Lagrangian under rotation, time-translation, and space-translation respectively. For *planar* N-body problems the same theorem holds for each N≥2, except then there are only 6=1+1+4 classic constants of the motion since the other 4 are zero. (In the circular restricted case, there is exactly *one* additional constant of the motion, discovered by Jacobi in 1836.)

Xia 1994 and Morales-Ruiz & Simon 2009's theorems: The only "constants of the motion" for the Newtonian 3-body problem that are nontrivial *real-analytic* (which MR&S redid now with "*meromorphic*") functions of the time, momenta, and positions of the N bodies (each assumed to have positive mass), are the real-analytic / meromorphic functions of the classic 10 (or classic 6 in the planar case).

Unsolvability-by-formula Corollary: Unlike for the 2-body problem, *no solution exists* for generic 3-body problems, in either the plane or 3-space, that is expressible as a **formula** in terms of any finite pre-specified set of meromorphic functions. The planar circular restricted 3-body problem similarly is generically unsolvable.

Liouville's theorem (and measure). The time-evolution of *any* Hamiltonian system (and this includes both the Newtonian and Einstein-Infeld-Hoffmann N-body problems) *preserves* infinitesimal elements of "phase space volume." Here "phase space" is the space whose coordinates are position and momentum, and hence for the three-dimensional N-body problem phase space would be 6N-dimensional. The natural measure, therefore, for sets of initial conditions is "Liouville measure," i.e. phase-space volume.

Action-Angle variables. Hamiltonian and Lagrangian systems (in which the Hamiltonian and Lagrangian have no explicit dependence on time) generally can be re-expressed in terms of other "positional" and corresponding other "momentum" variables, in an infinite number of ways. Example: for a planar double pendulum, the "position" variables could be the two angles at the "shoulder" and "elbow"; or they could be the angles of orientation of the two rods with respect to the y-axis; or they could be the x-coordinates of the "elbow" and "hand" in the xy plane (although the latter would only uniquely specify the state of the system within a *subset* of the phase space near "ground state"). This leads to the question: what is the **"most natural"** choice for these variables?

"Action-angle variables" are an attempt to answer that question (Fejoz 2013, also discussed in ch.11 of Corben & Stehle's 1960 textbook). Their "position" coordinates are chosen in such a way that the Hamiltonian ("total energy") does *not depend* on either position or time! The "momentum" coordinates then get defined by Hamilton's equations of motion and automatically each are *constants of the motion*. In these variables, time-periodic Hamiltonian flows are simply geodetic motion, the geodesics being straight lines, on a "standard torus," i.e. a hyper-rectangle (cartesian product of finite real intervals, one for each position coordinate) with periodic boundary conditions. Historically such positional coordinates then were called "angles" and the corresponding momentum coordinates "actions" (although these name-choices might have been unwise!). We shall not discuss quantum mechanics, but let me just note that in the original formulation of quantum mechanics, the only allowed values for actions were *quantized* in units of Planck's action-constant $\hbar$.

Ergodicity? L.Boltzmann, inspired in part by Liouville's theorem, introduced his "ergodic hypothesis." Consider the constant-energy hypersurface within the phase-space of a Hamiltonian mechanical system. [If the system conserves Δ other quantities besides energy, e.g. angular momentum, then we instead could restrict attention to a (2N-1-Δ)-dimensional hypersurface keeping them all constant, for a Hamiltonian system with N degrees of motional freedom and hence 2N-dimensional phase space.] In principle this surface could be partitioned into a large number of small cells with equal measures. Boltzmann's "ergodic hypothesis" is the claim that during the system's time-evolution, its phase-space trajectory eventually visits all those cells, spending, in the t→∞ limit, the same amount of time in each one. P.&T.Ehrenfest then suggested the weaker "**quasi-ergodic** hypothesis," stating that the dynamical evolution of each initial condition (except for a zero-measure subset) covers the hypersurface *densely*. And ideally, we also would want some kind of "**rapid mixing**" claim saying that this does not take an unreasonably long time. "Integrable systems" cannot be ergodic, since the trajectories emanating from an arbitrarily-tiny neighborhood of an initial condition are confined on an arbitrarily-tiny-measure subset of the constant energy hypersurface. The Bruns-Poincare proof of nonexistence of conservation laws suggested that perhaps N-body problems are ergodic if N≥3.

Fermi's 1923 error and the downfall of (quasi)Ergodicity. Fermi 1923 generalized Poincare's work with a demonstration that in a Hamiltonian system obtained as a generic perturbation of an integrable system with N>2 motional degrees of freedom, there is no (2N-2)-dimensional surface embedded in the (2N-1)-dimensional constant-energy surface that is (i) analytic in the action-angle variables, and (ii) all the trajectories that emanate from its points always remain on the surface. (Actually on p.262 Fermi claims "from its points" can be weakened to "from just one of its points.")

Unfortunately that made Fermi believe he'd proven (a) non-integrable Hamiltonian systems cannot have "separating surfaces"; and hence (b) are generically quasi-ergodic; and (c) arbitrarily small perturbations of integrable Hamiltonians with N≥3 motional degrees of freedom generically should suffice to make them ergodic; and (d) all this was a great new foundational insight in statistical mechanics. *All that*, however, is **completely false garbage**, as each of four later developments made clear:

1. KAM theory proves a positive-measure set of trajectories in Newtonian N-body problems *fail* to be quasi-ergodic.
2. Fermi, Pasta, and Ulam in 1950 performed one of the first computerized numerical experiments on a model of vibrations of a 1-dimensional crystal with small nonlinear perturbations added. Fermi thought this was going to demonstrate and explore the progress of the development of ergodicity in what (without the nonlinear perturbations) would have been a pure-harmonic system. But it didn't. Instead, it experimentally refuted his prior 1923 theoretical work.
3. The generalizations by Saari, Marchal & Bozis, etc of Hill-surface theory also showed the falsity of a,b,c,d for the Newtonian 3-body problem, and proved it is *not* quasi-ergodic. Hill's "zero-velocity surfaces" in the restricted circular 3-body problem are *analytic* separating surfaces that *prevent* quasi-ergodicity; but *no* trajectory lies on them! Furthermore, if, say, Venus is prevented by a Hill-like surface from approaching the orbit of Neptune in a Sun-Venus-Neptune 3-body system, then presumably that still would be true upon replacing "Neptune" with some tight binary, and still true upon replacing its components with tighter binaries, and so on – in which case Fermi was wrong about N-bodies for *every* N≥3, not just N=3.
4. E.Belbruno, M.Gidea, and F.Topputo claim the "weak stability boundary" separating "orbits around the moon" from "not" has an extremely complicated "fuzzy" structure consisting "of a family of infinitely many Cantor sets... fractal in nature... [sharing] many properties of a Mandelbrot set." They claim the presence of a "Smale horseshoe" in the dynamics causes motion of test masses started near this boundary to be chaotic – their fates are very sensitive to initial conditions – i.e. virtually the opposite of "analytically smooth."

Fermi's 1923 papers exhibited no sign of any awareness of Hill's surfaces. If he had been aware of them, then I suspect Fermi would never have written these papers. In reality, Fermi's surface-nonexistence theorem is *logically irrelevant* to the quasi-ergodicity question. And while it could conceivably have some other use, in fact, as far as I can tell from literature searches, nobody has ever successfully used it for any purpose whatsoever. Although Fermi was one of our greatest physicists (despite dying at age 53), his 1923 effort must be regarded as a massive failure.

What if we try to weaken quasi-ergocity still further? I believe that the Newtonian N-body problem, for each N≥3, can *fail* to be quasi-ergodic even if we restrict attention to *chaotic* trajectories (thus dodging KAM theory) and even if we restrict attention to the region whose accessibility is not obstructed by either Hill-like surfaces or the 10 classical conservation laws (thus dodging Hill-surface-based attacks). And certainly (we shall see), "rapid mixing" strengthened-ergodicity claims fail for various solar-system-related 3-body problems for lifetimes exceeding the present age of the universe.

Existence, Analyticity, and Series Solutions. Acta Math 7 (1885/1886) announced an international competition (prize 2500 Swedish crowns) sponsored by **King Oscar II** of Sweden and Norway, for the solution of this problem (formulated by K.Weierstrass):

> "Given a system of arbitrarily many mass points that attract each other according to Newton's laws, under the assumption that no two ever collide, try to find a representation of the coordinates of each point as a series in a variable that is some known function of time such that for all of its values the series converges uniformly."

King Oscar's problem was solved in the case of N=3 bodies with generic initial data – specifically, data with total angular momentum≠0 – by **Karl Frithiof Sundman** in 1912 – missing King Oscar's submission deadline 1 June 1888 by 24 years. To learn more about Sundman's solution, see Yeomans 1966 and Siegel & Moser 1971. Sundman first realized (P.Painleve had proved it in 1897) that the only "singularities" that could occur in 3-body motion (here meaning: times when the potential |energy| reaches infinity) were either 2-body or 3-body collisions, and Sundman proved the latter impossible if the total angular momentum were nonzero (also proved in Volchan 2008 and the argument had been known to K.Weierstrass). Two-body collisions actually are "regularizable" by a change of variables called the "Kustaanheimo-Stiefel transform" (explained by Saha 2009 in a nice way using "quaternions"); 2-body collisions then arise as a straight-line limiting case of hyperbolic or parabolic orbits in which they become "elastic bounces." [In the original variables, 2-body collision singularities resemble an algebraic branch point of an otherwise-analytic function. To describe things via an imprecise analogy: $F(q)=\sqrt{q}$ is an analytic function of the complex variable q, except for the algebraic branch point at q=0. If we were to change variables from q to s with $q=s^2$, which is a 2:1 map, then F(q)=s no longer has any issue at s=0.] Sundman then could show that, after an appropriate change of variables, the trajectories of the bodies had to be an *analytic function* of a new time-coordinate τ [a certain explicitly definable function of the original time coordinate t which behaves proportionally to $(t-t_{sing})^{1/3}$ when t is close to a 2-body-collision time t_{sing}] and indeed had to be analytic *throughout* an infinite strip of form |im(τ)|<K/2 for a certain explicitly computable strip-width K>0. Sundman then used the conformal map z=tanh(πτ/K) between that strip and the unit circle |z|<1. Upon then expanding everything in a Maclaurin series in powers of z, the resulting series must converge for all |z|<1, and hence for all τ in the strip. Unfortunately, Sundman's convergent series converges much too slowly to be practically useful. The reason for the slowness is that Maclaurin series convergent within some disk in the complex z-plane, converge slowly for z near the *boundary* of that disk. When τ is large, which it usually will be, z will lie exponentially(τ) close to the disk-boundary, forcing us to employ a number of series-terms that grows exponentially with τ to achieve any fixed accuracy level. While it often is feasible to predict 3-body motions a million years into the future, it is not feasible via Sundman's series.

Qiudong Wang in 1991 was able, by combining Sundman's with his own ideas, to solve King Oscar's problem for N bodies for *any* finite N (and also for N=3 in the case of zero total angular momentum that Sundman had forbidden), only 103 years after King Oscar's submission deadline. Wang expressed the solution in terms of a series which converged for all times t with $0\le t<t_{sing}$ but no times $t>t_{sing}$ (provided the solution exists for all times $0\le t<t_{sing}$; Wang permits $t_{sing}=\infty$ and does not need to know the value of t_{sing} a priori). If you thought Sundman's series converged impractically slowly, Wang's series takes that to a whole new level of "slow" because Wang's z will be *doubly*-exponentially close to the disk-boundary, forcing us to employ a doubly-exponentially large number of series-terms to achieve any fixed accuracy level. Wang first cleverly devises a monotonically-increasing map between τ and t (defining his new time coordinate τ) with the property that t approaches t_{sing} from below when τ→+∞, even though we do not know a priori what t_{sing} is, nor even whether it is finite. Then instead of Sundman's conformal map z=tanh(πτ/K) between the strip |im(τ)|<K and disk |z|<1, Wang employs a different conformal map of form $z=\tanh\big(H\tau\exp(B'\tau^2)\big)$ between a subset of form $|im(\tau)|<A'\exp\big(-B're(\tau)^2\big)$ of a region of form $|im(\tau)|<A\exp\big(-B|re(\tau)|\big)$ and the disk |z|<1. Here A, A', B, B', and H are suitable constants.

My Improvements over Wang. Actually, it seems to me that Wang could have done better by using a larger subset, of form $|im(\tau)|<A\,\mathrm{sech}\big(B\,re(\tau)\big)$, and a map of form $z=\tanh\big(H\tau\,\mathrm{sech}(B\tau)\big)$. But even with this improvement the convergence of Wang's series still would be vastly slower than (the already-slow) Sundman. Also, I think Wang could have changed variables to stop 2-body collisions from being "singular"; if so he would have been able to get a series convergent for all times t with $0\le t<t_{3sing}$ where t_{3sing} is the earliest time t>0 at which there is a singularity which is *not* a single 2-body collision. (Definition: A "**collision**" occurs when 2 or more bodies approach arbitrarily close to some fixed point of space when t approaches some specific time from below. Noncollision singularities have sometimes been called **pseudo-collisions**.)

In any case, Sundman and Wang's results prove constructively that the N-body problem, for any finite N≥1, generically has a unique solution – which indeed happens to be *real-analytic* – throughout any singularity-free open time-interval. But there were simpler ways to prove **uniqueness, existence, and analyticity away from singularities**: the Cauchy-Kovalevskaya theorem about systems of differential equations proves that for any finite N.

But now I would like to go a bit further than anything I ever saw in the literature by considering what happens if **the number N of bodies** is not demanded to be finite, but also is allowed to be **countably *infinite*.**

EXISTENCE AND UNIQUENESS THEOREM FOR NEWTONIAN N-BODY MOTION: Let N be either finite or countably infinite. I claim an analogue of the Picard-Lindelöf ordinary-differential-equation-system theorem (which incidentally is misnamed – it was proven by Cauchy in 1844, then simplified by Lipschitz, before either Picard or Lindelöf got involved) can be shown, and then it proves existence and uniqueness, under the following assumptions:

1. The sum of the absolute values of the masses is finite: $\sum_j |m_j|<\infty$.
2. The "**total tide**" is finite, i.e. letting $\vec{x}_j$ denote the position of body j:

$$\sum_k \sum_{j<k} |m_j\, m_k|\, |\vec{x}_j - \vec{x}_k|^{-3} < \infty.$$

During any time interval containing the initial conditions in its interior and in which that total-tide sum remains finite: a unique solution-motion of the bodies will exist.

PROOF: This follows immediately from theorem 6 p.117 of the survey by Lobanov & Smolyanov 1994, which in turn came from Lobanov 1992. In their theorem statement, LCS stands for "locally convex space," a generalization of "Banach space," and TS for "Hausdorff topological space." [Also, it looked like it alternatively could be shown by improving both theorem 2 p.42 of Murray & Miller 1954 and its proof a bit, e.g. replace their L with L_j and their Ln by $\sum_j L_j$ throughout, but I did not check carefully. Murray & Miller also give the Cauchy-Kovalevskaya theorem in their §5.10. Also: Schlage-Puchta 2021 claims to give "optimal versions" of the Picard-Lindelöf theorem extending it to a wider class of functions than Lipschitz, in which the Lipschitz "constant" can be allowed to become arbitrarily large, indeed "logarithmically infinite."] **Q.E.D.**

2.2 Kepler & Newton's solution of Newtonian two-body problem. When bound, both bodies move periodically along **ellipse** trajectories, with each ellipse having one focus at the system's center of mass, the two ellipses' major axes are collinear, and the two ellipses are oppositely oriented and geometrically similar with diameter ratio equal to the mass-ratio (except the heavy body describes the smaller ellipse). In (r,θ) polar coordinates (with the center of mass fixed at the origin r=0) the ellipse is

$$r = [1-e^2]\,[1 + e\cos\theta]^{-1}\,D/2$$

where e and D are constants: e with $0\le e<1$ is the eccentricity of the ellipse [e=0 for circle; for ellipse with axes $d\le D$ we have $e^2=1-(d/D)^2$ and $d/D=(1-e^2)^{1/2}$], while D is its diameter, aka "major axis." *Un*bound orbits also are described by the same equation but with e=1 (parabola) or e>1 (hyperbola). For the *repulsive* Coulomb case of two same-sign electrical charges ("Rutherford scattering") we also get the hyperbola-trajectory solution, except that the *other* component of the double hyperbola is used (the one with center-of-mass focal point *outside* rather than inside it), corresponding to eccentricity e<-1. The motion conserves angular momentum (leading to Kepler's "second law: equal areas are swept in equal times"), so that $r^2 d\theta/dt$ **stays constant**. Solving this differential equation, we find that (for motions starting with θ=0 at time t=0; and with |t|<P/2 and |θ|<π)

$$2\pi t/P = [1-e^2]^{3/2} \int [1+e\cos\theta]^{-2} d\theta = 2 \arctan\big((1-e)^{1/2} (1+e)^{-1/2} \tan(\theta/2) \big) - (\sin\theta)\,(1+e\cos\theta)^{-1}\,(1-e^2)^{1/2}\,e$$

where P is the orbital time-period. The distance of closest approach to the center-of-mass is (1-e)D/2 while the furthest is (1+e)D/2. The formula [from Bate, Mueller, White 1971 p.33; J.H.Lambert in 1761 obtained considerably fancier versions of this formula, see p.91 of Whittaker 1937] for the **orbital period** for masses M and m with ellipse diameter D is

$$\text{Period} = 2\pi\, G^{-1/2}\,(M+m)^{-1/2}\,(D/2)^{3/2}.$$

Kepler had incorrectly thought (in modern language) that "M+m" was just the solar mass "M," and that the focus of planet-orbit ellipses was the center of the sun. Because the mass m of even the heaviest planet Jupiter (which outweighs all the others combined by a factor≈2.5) obeys m/M≈1/1048, those errors always are numerically small for our solar system; and the foci of the ellipses do lie near the sun, even though not at its center and usually a bit outside of it. For binary stars Kepler's error usually is large, but there was no way for Kepler to know that because it was infeasible to observe binary stars (indeed they were not even known to exist) until well after Kepler's death. In terms of the angular momentum $\vec{L}$ (relative to its center of mass) and total energy E (which for a bound system is *negative*) of the 2-body system, the eccentricity and ellipse-diameter of the planet-orbit are

$$e = \big[1 + 2E|\vec{L}|^2 G^{-2}(M+m)^{-2} m^{-3}\big]^{1/2}, \quad D = G\,(M+m)\,m\,/\,|E|.$$

Therefore the circle (with e=0) is the orbit with uniquely minimal total energy E for any given angular momentum L. This explains why our solar system's planets have near-circular orbits lying nearly in a single plane. Even the most-eccentric and most-inclined of the eight IAU planets (Mercury, e=0.206) has major/minor axis ratio $D/d=(1-e^2)^{-1/2}\approx1.022$ and inclination=7.0 degrees. The second-most eccentric (Mars, e=0.094) has D/d≈1.0044, while the second-most inclined is Venus at 3.4°. The reason: When they formed, collisions of particles produced heat which was radiated away, thus losing energy; but since the angular momentum carried off by the radiation was negligibly tiny, angular momentum was approximately conserved. The minimum anergy for a rotating cloud of gas and dust with given angular momentum arises when all the particles lie in a single plane (since otherwise gravitational energy could be decreased by flattening). Then during the later condensation of that flat cloud into planets, more heat energy was lost, forcing near-circular orbits.

The above formulas relating θ to t make sense, i.e. are real-valued, only in the bound cases (circle & ellipse) when 0≤e<1. When |e|≥1 there is no period P (or it is infinite) and those equations would involve imaginary quantities. To repair those complaints, they should be replaced by

$$2\pi t/H = 2\,\text{atanh}\big((e-1)^{1/2} (e+1)^{-1/2} \tan(\theta/2) \big) - (\sin\theta)\,(1+e\cos\theta)^{-1}\,(e^2-1)^{1/2}\,e$$

where H is a certain characteristic time, still given by the period formula even though not a period. Every ellipse (and hyperbola) shape is attainable. **Parabolas** are a special borderline case arising for a comet with exactly the minimum possible energy it needs to escape (i.e, total energy=0); its speed approaches zero at times t→±∞, thus allowing escape – but very slow escape, with distance ultimately growing proportionally to $t^{2/3}$. In contrast, for hyperbolic orbits the escape speed tends to a positive constant when t→±∞, and indeed the interbody distance is $At\pm O(\ln|t|)$ where A>0 is a constant (Ch.1 of Pollard 1966). Note that every **hyperbola** is asymptotic to two straight lines. Hence, when far separated, bodies behave as if they were non-interacting.

For hyperbolas D/2 equals the distance between the point of closest approach, and the crossing-point of the two asymptotic lines. The distance of closest approach is MinDist=(e-1)D/2. The "impact parameter," i.e. distance between a comet-hyperbola asymptotic line and the sun-center, is $b=[e^2-1]^{1/2}D/2$. That is, if gravity were magically "switched off" (G→0) then comets would travel along the hyperbola's asymptotic straight line rather than the hyperbola, and b would be the minimum sun-comet distance. The **deflection angle** (angle between the two asymptotic lines) is

$$\text{DeflectionAngle} = 2\,\text{arcsec}(e).$$

which when e→1+ tends to 0+, meaning a trajectory which "bounces back" nearly 180°; and when e→∞ tends to π-, meaning a comet-trajectory that is nearly a straight line almost unaffected by the gravity of the sun. Note: some people would prefer π-|α| over my "deflection angle" α. To avoid confusion, I will call π-|α| the "**scattering angle**." It is zero if there is no scattering. This formula also may be written

$$\text{ScatteringAngle} = \arctan(D/b)$$

where b is the "impact parameter," while the constant D is the interbody distance at "turnaround" for a hypothetical head-on collision (which depends on the kinetic energy E in the center of mass frame at infinite separation) where note D is positive in the case of Rutherford scattering of an alpha particle off an atomic nucleus, but the formula for D in the Newton case is D=-GmM/E, which is negative. (Ch.1. of Belyaev & Ross 2021.) Note ScatteringAngle=0 if |b|=∞, but ScatteringAngle=±π if b→0, corresponding to 180° "bounce back" from head-on collisions).

We also should note that **straight line** radial-infall (head on collision) or ejection trajectories also (considerably more trivially) solve Newton's 2-body equations.

A slick approach to the 2-body solution is as follows. Without loss of generality assume the center of mass is fixed at (0,0,0). The fixed total angular momentum vector $\vec{L}$ tells you the plane-of-motion (i.e. is orthogonal to it). The so-called Laplace-Runge-Lenz vector $\vec{A}$ is another constant of the motion: $\vec{A}=\vec{p}\times\vec{L}-(m+M)^{-1}m^2M^2G\vec{r}/|\vec{r}|$ where $\vec{p}=m\vec{v}$ is the linear momentum of one of the bodies and $\vec{r}$ its position (it does not matter which body since choosing the other yields the same value for $\vec{A}$). This lies within the plane of motion and points in the direction of the orbit-ellipse diameters in the direction from the furthest-orbit-point toward the center of mass; $e=(m+M)|\vec{A}|/(GMm)$ is the eccentricity of each ellipse.

2.3 Interactions between the planets perturb Kepler & Newton's laws of elliptical motion. Isaac Newton, after solving the 2-body problem, worried that the *cumulative* effect of inter-planet interactions might be enough to damage the solar system. Jupiter is 1048 times lighter than the sun, and 4-7 times further away from the Earth. So Jupiter's gravitational force on the Earth is always $(16\text{-}52)\times10^3$ times smaller than the Sun's. However, those little tugs keep happening. Newton wasn't sure whether, over timespans$\gtrsim10^7$ years, their cumulative effect could be devastating – and wondered whether God would occasionally need to intervene to correct matters.

To understand that, P-S.Laplace (1749-1827), J-L. Lagrange (1736-1813), S.D.Poisson (1781-1840), U.LeVerrier (1811-1877), and F.F.Tisserand (1845-1896) in France, and G.W.Hill (1838-1914) in the USA, developed various kinds of "perturbation theory" approximate methods. LeVerrier evaluated these for the planets to 7th perturbative order after about 5 years of symbolic manipulation. Using such methods, LeVerrier in France, and also independently John Couch Adams in England, in 1846 predicted the existence and location of a new 8th planet "Neptune" as a hypothetical cause of anomalies in the observed motions of Uranus. (Lai, Lam, Young 1990 explain a simpler and better way to redo the Adams/LeVerrier calculation. Littlewood 1953's Neptune essay explains a *really* simple way, albeit somewhat dependent on luck.) Adams then failed to convince English astronomers even to bother to look for it, while LeVerrier similarly failed to convince the French ones. Finally LeVerrier contacted the German astronomer Johann Galle, who found Neptune less than 1° away from its predicted location that night (then verified from its proper motion that it was a planet the next night). This was lucky in the sense that Galle would normally have been denied permission to use the telescope for such a foolish mission, but by an exceptional stroke of good luck for science, LeVerrier's date happened to coincide with the date of a birthday party for the observatory director. Therefore, Galle was granted access for one night. (Without that luck, it is anybody's guess how many more years would have been needed to find Neptune, which is too dim to be seen with the naked eye.) This was regarded as a great triumph for, and verification of, Newton's laws, which thus clearly superceded Kepler's. LeVerrier also noted that Mercury's perihelion was gradually precessing, approximately 9% faster than planet-perturbation theory predicts. This could have been due to an extra planet ("Vulcan"), nonsphericity of the sun, or matter orbiting the sun inside Mercury's orbit – but wasn't: Vulcan doesn't exist, and the other effects eventually were realized to be >5000 times too small to account for the observations. A much simpler way to estimate the other planets' long term perturbative-Newtonian effect on Mercury's perihelion, which works quite well, is to approximate those other planets not as moving points, but rather as having their masses "smeared out" all along their orbits (modeled as uniform lineal mass density along a sun-centered circle) in a steady-state model like Saturn's rings. Even better would be to use elliptical, not circular, "rings," with lineal mass-density proportional to the reciprocated Keplerian velocity of that planet at that point of its orbit. This "time-averaging" approximation was already suggested in 1818 by C.F.Gauss (1777-1855), who understood how to express the gravitational potential from such a mass-distribution in terms of elliptic integrals via his "AGM" process. (See also the commentaries pp.253-254 of vol.XI,2 and pp.169-170 of vol.X,2 of Gauss' *Werke*.) The fact that the Newtonian gravitational field and potential of a circular ring of mass (also electrostatic field & potential for a ring of charge, and magnetic field if a current flows round the ring) may be expressed in closed form using complete elliptic integrals was rediscovered by many others after Gauss, one of the first being G.A.A.Plana in 1820, and two of the latest being Datta 2007 and Ciftja, Babineaux, Hafeez 2009; it alternatively may be done using "toroidal functions" (Selvaggi, Salon, Chari 2007); and the effect of a sub*arc* of the whole circle then may be expressed in terms of *incomplete* elliptic integrals.

Eventually Mercury's anomalous precession was explained by A.Einstein's 1915 theory of general relativistic gravity, i.e. Newton's laws are slightly *wrong*. After K.Schwarzschild's 1916 solution of the Einstein field equations in the spherically symmetric case, exact formulas for the trajectory (which generically is a transcendental curve expressible using elliptic functions) of a test-particle around a massive spherical star became available. Indeed, Carter 1968 found enough constants of the motion to prove that the geodesics around *Kerr* (spinning) black holes were integrable.

P-S.Laplace invented perturbation theory (explored in the solar system context by him, J-L.Lagrange, and, at second order by S.D.Poisson), and LeVerrier invented "period-averaged" perturbation numerical methods. Their effect on Newton's question, generally speaking (at least at orders≤2 for moderate timespans) seemed to be reassuring – the "tugs" seem to cancel out over time. Specifically, if the planet/sun mass ratios are regarded as proportional to ε then with $\varepsilon\to0+$ we get the Kepler/Newton 2-body model, but as we increase ε to also "switch on" inter-planet interaction, we get perturbations which we may regard as approximated by truncations of series in ascending powers of ε. Lagrange and Laplace's explorations of *first*-order perturbation theory (only powers≤1) for the 3-body problem proved that the effect of these perturbations on the planets' Keplerian orbital parameters (assuming the two planets are not in "resonance") qualitatively resemble the function sin(t), which remains bounded, cannot feature any "secular growth," and averages to zero. "Secular" growth means "linear in time." (When Napoleon re-asked whether God might need to get involved, Laplace famously replied "I have no need of that hypothesis.") A better word than "stability" to describe that might be "confinement." The proof of the Laplace/Lagrange result became easy once the problem was rewritten in Delaunay's "action-angle variables"; indeed you do not even need to know what those are, merely their existence suffices.

This zeritude also holds with Poisson's *second*-order perturbation theory: except now the effect on the planets' Keplerian orbital parameters (again, with at most *two* planets plus one sun) can include terms qualitatively like the function *t*cos*t*, which does *not* remain bounded. But Poisson – more dubiously! – argued this still demonstrated "stability" in the sense that the parameter-perturbations return to values arbitrarily near zero an infinite number of times, and the "Cesaro mean" *time-averaged* value of the perturbation is zero, or anyhow remains bounded, for all timespans. But really, the truth is that perturbation theory at any fixed order is *not exactly valid*, but rather merely approximately valid during short-enough timespans. When the timespan becomes large or infinite, fixed order perturbation theory loses validity. The maximum allowable timespan generically is proportional to some negative **power** of ε where ε governs the perturbation size, and which power depends upon which order of perturbation theory you are using.

> However, where applicable, the subsequently-developed "KAM theory" can enable making statements valid even for infinite time intervals, e.g. showing that for N-body systems resembling the solar system, if the inter-planet attractions are treated perturbatively and are weak enough, and if the unperturbed system is sufficiently far from exhibiting "resonances," then for the majority of initial conditions there are metrics in which the perturbation's effect on orbits will remain bounded *eternally*. And "Nekhoroshev theory" says that even if your initial conditions are in the "minority" set for which we cannot guarantee eternal boundedness, they still stay small for a timespan now *not* proportional to a negative-power of the perturbation size ε, but rather much more-quickly-growing to ∞, e.g. like an *exponential* function of something proportional to a power. And Moser and Pöschel extended KAM theory to larger classes of systems than "Hamiltonian" (e.g. "reversible systems") and allowing only large-enough finite-order differentiability rather than (as in the original KAM theory) demanding analyticity.

Lagrange and LeVerrier than found out how to do perturbation theory at arbitrarily high order (if you do enough computational work). See Burns 1976 for an elementary exposition redoing some of that. However, Spiru C. Haretu (1851-1912) in his 1878 PhD thesis at Univ. of Paris 1878 (published by Annales de l'Observatoire de Paris in 1885) discovered more secular growth terms in third-order perturbation theory. This showed that perturbation theory at orders≥3 *fails* to deliver even Poisson's more-dubious kind of alleged solar-system stability. KAM theory can prove those kinds of confinement statements, but high-order perturbation theory cannot.

And (to make it even worse) I warn you that Laplace/Lagrange and Poisson's zero-results in first- and second-order perturbation theory only apply to the *three* body problem. For *four* bodies, Clarke, Fejoz, Guardia 2024 produced a counterexample.

Although over the centuries there have been numerous occasional outbursts of hype trying to confuse you into thinking the "stability of the solar system" had been "proved," all those claims appear to be bullshit. In fact, large computations suggest the solar system not only is chaotic, but also (if Newton's laws for N point masses were extrapolated forever) would eventually collide Mercury with Venus or the Sun. But since the time scales required will likely be longer than the sun's lifetime, practical astronomers probably should not care. Mogavero et al 2023 claim the shortest solar system instability lifetimes arise from perturbation theory orders≥6 and lead to expected halflives for Mercury of 30-40 Gyr despite its much shorter Lyapunov time≈5 Myr.

> "Is the solar system stable? Properly speaking the answer is still unknown, and yet this question has led to very deep results which probably are more important than the answer to the original question."
> – Jürgen Moser: Is the solar system stable?, Mathematical Intelligencer 1,2 (1978) 65-71. But I claim that today the answer *is* known, from Mercury's behavior in brute force computer simulations, to be "no"; albeit if the planets were sufficiently less massive (and/or, if the sun were sufficiently more massive) than they are, and if orbital parameters were altered a bit to get rid of solar system "resonances," and if it all were an ideal tide-free lossless Newtonian N-point-body problem, then KAM theory could be used to show "yes."

2.4 Periodicity and "Poincare-periodicity." Call an N-body solution **periodic** if, after some finite timespan (the "period"), it repeats its starting configuration (all mass-points re-acquire the same positions and velocities as they did at start). Call an N-body motion **"Poincare-periodic"** if, after some finite timespan, it repeats its starting configuration in its center of mass frame, *but* perhaps with the whole configuration rotated by some angle. Poincare-periodic N-body motions *are* periodic in appropriate "rotating coordinate systems," making them suitable for KAM-theory purposes.

2.5 Einstein's general relativity. With GR rather than Newton's laws, orbits are *not* exactly ellipses, and indeed generically are *not* closed curves at all and hence not periodic. But the orbit of a non-spinning infinitesimal test mass around a Schwarzschild black hole still is "Poincare-periodic."

> Indeed, even in Newton's pre-Einstein "action at a distance" model, closed-curve orbits are rather miraculous. An 1873 theorem of J.L.F.Bertrand (1822-1900) asserts that the *only* central-attractive force laws that are functions of distance only that generically yield *closed*-curve bound orbits are Newton's inverse-square law Kr^{-2}, and the harmonic oscillator "Hooke law" force Kr^{+1}; in both cases the curves are ellipses, albeit for Newton the center of mass lies at the *focus*, while for the harmonic oscillator it lies at the *center*, of the ellipse. (Other laws such as $Kr^{-2.1}$ still permit "*Poincare-periodic*" orbits, however.) We also have an 1882 theorem of V.G.Imchenetsky that these two are the only central force laws always yielding conic sections as orbits; and the 1889 theorem of G.X.P.Koenigs (1858-1931; which I'm surprised Bertrand did not prove), that they also are the only ones that generically yield *algebraic* curves as orbit trajectories in the xy plane (although Kr^{-3} can yield algebraic-curve trajectories under a countably-infinite set of suitable initial conditions). Rather amazingly, general relativity exhibits special love for Bertrand's two force laws:
>
> a. In flat spacetime, the attractive force between two masses separated by distance s, in the limit where s far exceeds both Schwarzschild radii, indeed is inverse square, yielding in this limit exactly-closed elliptical orbits.
> b. Meanwhile in de Sitter spacetime, two points will tend to separate due to the "expansion of the universe." Counteracting that tendency requires artificially accelerating them toward each other, with each acceleration equal to $(2/H)c^2\cot(2H/s)$ where H is the constant "Hubble length." This in the limit where s is far smaller than H indeed happens to coincide with Hooke's law – albeit with repulsive sign – yielding hyperbola-shaped trajectories.
>
> Central force law powers which yield orbits expressible in terms of elliptic functions include
> $$\{-7, -5, -4, \mathbf{-3}, \mathbf{-2}\} \cup \{0, \mathbf{1}, 3, 5\} \cup \{-5/2, -3/2, -1/3, -5/3, -7/3\}$$
> and this list is complete in the case of integer powers; for the **bold**-font powers ordinary circular functions suffice. (§48 of Whittaker 1937.)

For black hole binaries made of two *spinning* black holes with orbits and spins mis-aligned, spin-orbit coupling causes the orbits *not* to stay in a plane. Hence orbits that Newtonianly would have been circular become "spherical."

The formula (Misner, Thorne, Wheeler ch.31) for the **Schwarzschild radius** of a mass-M spherically-symmetric uncharged nonrotating black hole:

$$\text{Radius} = 2\, G\, c^{-2}\, M.$$

Example: The Schwarzschild radius of the Sun is 2.9 km and of the Earth is 0.88 cm, respectively 240000 and 725000000 times smaller than their observed radii.

The formula for the approximate **orbit lifetime** before event-horizon merger due to orbit **decay** of a circular-orbit black-hole binary (separation distance=R) from losing energy by emitting gravitational waves:

$$\text{Lifetime} \approx (5/256)\, c^5\, G^{-3}\, R^4\, M^{-1}\, m^{-1}\, (M+m)^{-1}.$$

This may be derived from Eddington's radiated-power energy-loss formula for a rotating quadrupole (EQ 10.5.22 in Weinberg's *Gravitation & Cosmology*; §5.4 in Ohanian & Ruffini 1994) under the approximation that this energy loss is slow, i.e. only a small fraction of orbital energy is lost per circular orbit of the binary; and that orbit is approximated as Newtonian. In other words, this formula should become asymptotically exact in the limit that lifetime$\gg c^{-1}$ times the sum of the Schwarzschild radii.

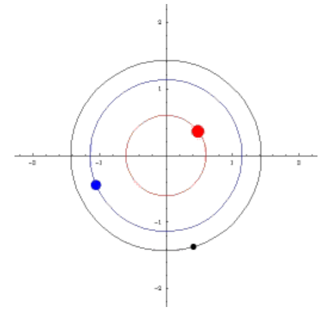
Example. For the earth-sun system (ignoring rest of solar system and ignoring tides) this lifetime estimate amounts to 1.1×10^{23} years, i.e. 24 trillion times the current age of the solar system.

This whole model of orbit decay due to gravitational-wave energy loss (and more!) was excellently observationally confirmed by signal timings from a binary pulsar discovered by J.H.Taylor & R.A.Hulse measured during 1973-2016 using the 92-meter Arecibo radio telescope. E.g. see fig.10 in Taylor's Nobel lecture covering data from the year-range 1975-1993. That won them the 1993 Nobel prize in physics.

(Post)Newtonianism. We shall heavily implicitly use the idea that in certain limits, general relativity is well described by Newton's laws. Specifically, this happens if all Newton's bodies have speeds $\ll c$, while all center-separations stay $\gg$ than the sum of their two Schwarzschild radii. In fact, Newton's laws are the first term in the "post-Newtonian expansion" in general relativity (Misner, Thorne, Wheeler ch.39), which is a power series in powers c^{-n} for n=0,1,2,... The first-order (beyond Newton) post-Newtonian equations of motion are called the Einstein-Infeld-Hoffmann (1938) equations of motion. Our constructions are going to be recursive with the recursion level being an integer $\ell \geq 0$, and we are going to design them so that as $\ell \to \infty$ these Newtonian limits happen. That is very useful because Newton's laws are much easier to understand than Einstein's.

The fact that our lifetime formulas are only approximate (also the period formula, while exact Newtonianly, is inexact general-relativistically) will not matter; e.g. these formulas should be *asymptotically* exact when $\ell \to \infty$ in our constructions, and their validity, even though only approximate, should be good enough for our purposes.

2.6 The Newtonian 3-body problem.



2.6.1 Simpler problems. There are certain important subsets of the set of 3-body problems which are easier to understand. In **"*planar* 3-body problems"** all motions are confined to a 2-dimensional plane, reducing the number of degrees of freedom. In the **"*restricted* 3-body problem"** (Poincare: "probleme restreint," Euler: "problema restrictum") we regard one of the three bodies as negligibly massive compared to the two "primaries" (which therefore move according to the exactly solved 2-body Kepler-Newton solution). We also can have, of course, "planar *and* restricted 3-body problems." In the **"*circular* planar restricted 3-body problem"** the two primaries' orbits are exactly circular, which allows us to rewrite everything (if desired) in a "rotating coordinate system" in which the primaries both are *stationary* and there are only 2 degrees of freedom of motion (both for the remaining tiny-mass asteroid).

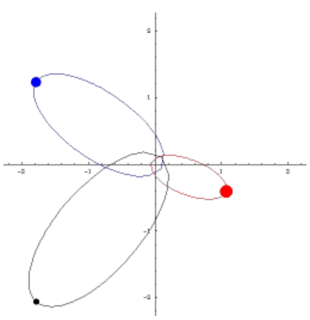
Another simplified subclass of 3-body problems are the **Sitnikov problems**, also called **Isosceles 3-body Problems**. In these, there are two equal masses at (x,y,z) and (-x,-y,z) whose center of mass remains always on the z-axis, plus a third body which forever remains on the z-axis; the overall center of mass stays fixed at (0,0,0). Sitnikov problems have only 3 degrees of motional freedom. We also may *restrict* Sitnikov's z-axis body to have mass→0 in which case there is only 1 degree of motional freedom. This restricted version is, in fact, what Sitnikov 1960 historically did; his results were generalized to the case with all masses nonzero by Alekseev (or "Alexeyev," "Alexeev" – transliterated various ways) about 8 years later. The *circular restricted* Sitnikov case is called the **MacMillan problem** since MacMillan 1911 demonstrated how to exactly solve it using elliptic integrals. Sitnikov motions are always unstable to 3D perturbations and hence seem (at least naively) of no direct physical interest, but their mathematical simplicity makes understanding them feasible, yielding mathematically interesting lessons.

2.6.2 Euler & Lagrange's rigid-rotator (and elliptic) 3-body motions. L.Euler in 1763 began, and in 1772 J.L.Lagrange completed, the task of finding all 3-body Newtonian-motions which happen to be exactly describable as a rigid rotation. Euler's 3 bodies remain *collinear* at both the two endpoints, plus one very-particular fixed intermediate point, on a fixed-length **line segment**; and that line segment rotates in a plane about the system's center of mass at just the right rotation rate. Specifically, if the masses are A,B,C in that order along the line, then the distance-ratio k=dist(B,C)/dist(A,B) obeys (Moulton 1914, EQ 34 p.60) this **quintic equation**:

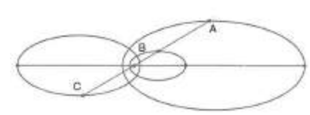
$$(A+B)\, k^5 + (3A+2B)\, k^4 + (3A+B)\, k^3 = (B+3C)\, k^2 + (2B+3C)\, k + B+C.$$

Example: Approximating the Sun-Jupiter system (masses 1048 and 1) as circular with a third tiny mass, we find with A=1048, B=1 that the quintic is $1049k^5+3146k^4+3145k^3=(k+1)^2$ with solution k≈0.0697698, indicating Lagrange point L2 lies 0.0697698 Jupiter-Sun distances further out than Jupiter. We similarly find L3 is 0.999444 Jupiter-Sun distances on the opposite side of the sun, while L1 lies on the Sun-Jupiter line segment 0.066667 Jupiter-Sun distances inward from Jupiter.

In Lagrange's solutions, the 3 bodies form the corners of a rigidly-rotating **equilateral triangle**; the triangle rotates in its plane about the system's center of mass at just the right rotation rate (animated picture above right by Rick Moeckel, who in 2005 proved a conjecture of Saari that these rigid-rotators are the unique 3-body solutions with constant moment of inertia).



There also are "**ellipse rather than circle**" generalizations of Euler's and Lagrange's rigid rotators (which Lagrange knew about; the elliptic version of Euler is sometimes called "Euler-Moulton," see Moulton 1910), in which the bodies follow mutually-similar ellipse rather than circle trajectories (all ellipses geometrically similar and sharing one focus fixed at the center of mass; no longer a rigid rotation). The points remain the corners of an equilateral triangle for Lagrange (for Euler, "remain collinear" as in picture at right) with fixed center of mass – just one whose size increases and decreases as it rotates. The animated picture at left by Rick Moeckel shows an example with eccentricity e=0.9. For the Lagrange triangle with masses A,B,C, the rotational **Period**$=2\pi[(1+e)^3(A+B+C)G/R^3]^{1/2}$, where e with 0≤e≤1 is the eccentricity of the ellipse and it is an equilateral triangle with side R (when maximal). These can also be generalized to replace the "ellipses" by mutually-similar Kepler parabolas or hyperbolas or straight lines to get nonperiodic unbounded 3-body solutions. These remain, even today, the only Newtonian unrestricted 3-body solutions exactly describable by known formulas.

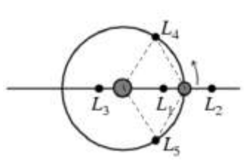


Edward J. **Routh** showed in 1874 (preceded by **Gascheau** 1843; see also Siegel & Moser p.113) that Lagrange's rotating-triangle solution is *not* "linearly unstable" in the plane (i.e: no planar exponentially-growing infinitesimal perturbations exist, i.e. the Lyapunov time is infinite) exactly if

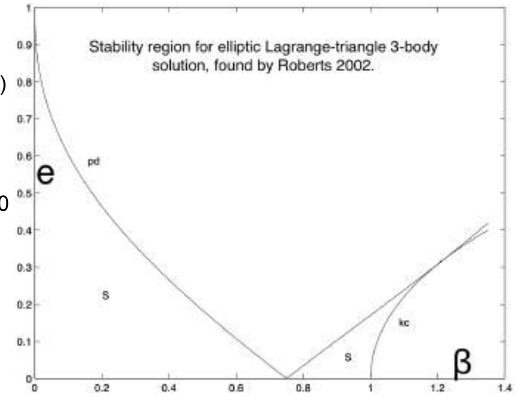
$$(A+B+C)^2 \geq 27\, (AB+BC+AC).$$

Examples: if A=1 and B=C then we have instability if $B>1/(25+18\sqrt{2})\approx 0.019819$. If A=1 then in the limit C→0+ we have instability if $B>2/(25+3\sqrt{69})\approx 0.040064$. So evidently, for stability you want one mass to be much heavier than the other two.

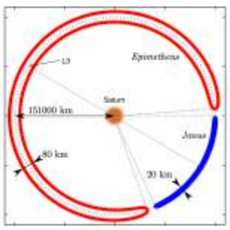
Roberts 2002 found the **stability regions** for the elliptic-motion generalization of Lagrange's equilateral triangle solution in the eβ plane, where e is the ellipse eccentricity (0≤e<1) while $\beta\equiv 27(AB+BC+AC)/(A+B+C)^2$. The regions where no exponentially-growing infinitesimal perturbation exists are the two labeled **"S"** in his picture at right. The point where Roberts' kc and pd curves meet has coordinates β=1.209133, e=0.314508; the corner in the pd curve (where it hits the horizontal axis) has β=3/4=0.75; the kc curve hits the horizontal axis at β=1. Routh and Roberts only analysed *planar* perturbations, not *spatial* (3D) ones. However, that does not alter anything because all Lagrange-triangle orbits are linearly-stable with respect to perturbations orthogonal to the orbit plane.



In all, given two circular-orbiting Newtonian bodies, there are exactly 5 "**Lagrange point**" locations (in a rotating coordinate system fixing the two primaries) where an infinitesimal test mass could remain "stationary." They are denoted L1, L2, L3, L4, and L5. The latter two are Lagrange's equilateral triangle third vertices (L4 is leading, and L5 trailing the smaller primary); the first three are Euler's (always unstable) collinear locations.



Euler's collinear solutions *always* are unstable with exponentially-growing infinitesimal perturbations. For that reason, it seemed clear that Euler's 3-body solutions were not going to be observable in Nature. Incorrect! Saturn's moon Janus (diam.=178km) and its 3.6× lighter sister moon Epimetheus (diam.=117km) are in 1:1 mean motion resonance – both orbit Saturn every 16.67 hours. But in a rotating coordinate system in which Janus's angle is fixed, Epimetheus traces a "horseshoe orbit" in which it oscillates between being slightly ahead of and slightly behind Janus – a huge angular swing of about ±175° relative to Janus. It also oscillates between having orbit-radius about 48km higher than Janus' during backswings, and 48km lower during forward swings. Each such full-horseshoe cycle takes about 8 years, i.e.about 4000 Janus-periods. In an *angularly-averaged* sense, Epimetheus and Janus co-orbit Saturn in *antipodal* positions, i.e. approximately Euler's solution. Counter-intuitively, the large amplitude of the "horseshoe" perturbations away from Euler's L3 point actually *stabilize* Epimetheus' orbit, even though the *un*perturbed Euler solution would be unstable. (Perhaps an explanation of this is that the large angular swings cause Epimetheus to keep *toggling* between being a "Trojan" and "Greek," thus enjoying stabilizing effects from L4 and L5, as opposed to the destabilizing effect of L3 alone.) Niederman, Pousse, Robutel 2020 mathematically *proved* existence and KAM stability of a horseshoe-type planar 3-body orbit solution resembling Saturn-Janus-Epimetheus (sketched below left).

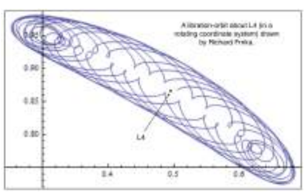


The Gascheau-Routh-(Roberts) non-instability result inspired hope that **Lagrange triangles exist in Nature**. The first example came 134 years after Lagrange's analysis when astronomer Max F.J.C. Wolf (1863-1932) discovered in 1906 the asteroid "588 Achilles" forming the 3rd vertex of the rotating equilateral triangle whose other two corners are the Sun and Jupiter. Lagrange triangles actually happen in several places in our solar system. The most famous are the Trojan asteroids which orbit approximately 60° ahead or 60° behind Jupiter in its (approximately circular) orbit around the sun, and evidently have stayed that way for billions of years *despite*

- perturbations from both non-Jovian planets and
- each other;
- and despite the fact Jupiter's orbit is an ellipse, i.e. not exactly circular (e≈0.0487), causing the equilateral triangle to "breathe."

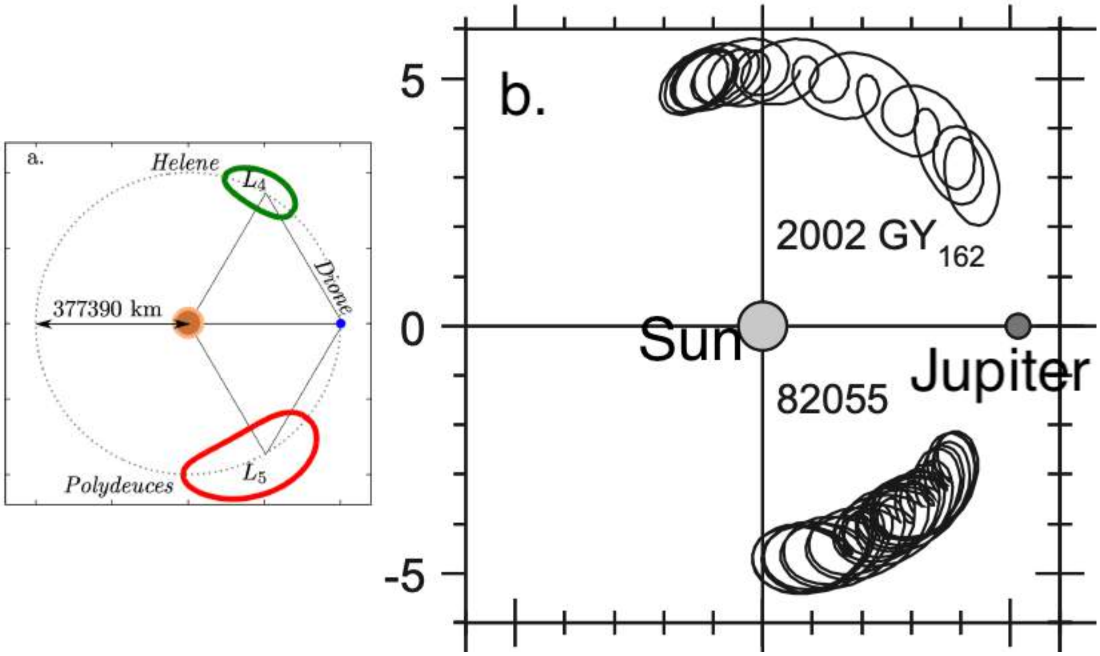
The group 60° ahead of Jupiter (near L4) are called "Greeks" and the group 60° behind (near L5) "Trojans." About 10^4 Jupiter Trojans/Greeks are currently known, constituting approximately 1% of all currently-known asteroids; and in all Jupiter is estimated to have about 10^6 with diameters≥1 km. Neptune possesses the second largest (after Jupiter) number of its own Trojans and Greeks. (Uranus, Earth, and Mars also have them.) Saturn and Venus, however, lack them, which is thought to be because Saturn's 5:2 near-resonance with Jupiter destabilizes Saturn Trojans; and computer simulations similarly indicate that Venus Trojans would be unstable thanks to the effects of other planets.

Saturn also has "**Trojan moons**": Telesto and Calypso are trojans of Tethys, while Helene and Polydeuces are trojans of Dione.



Small perturbations away from stable Lagrange triangle exact solutions result in small oscillatory perturbations with respect to the exact solution, called **"librations."** These in general are not exactly periodic, but can be. If Jupiter orbits "clockwise" round the sun, then Trojans and Greeks librate generally "anticlockwise" round L5 and L4, although it actually not clear how to precisely define that, because a vast variety of libration-orbits are possible, many very complicated. One example libration "tadpole orbit" for a circular restricted planar 3-body problem about L4 (the black dot) is shown at left, drawn by Richard Frnka in the rotating coordinate system. Note that Frnka's orbit stays bounded away from L4 and has **cusps** wherever it hits an impenetrable Hill "zero velocity surface" surrounding L4.

A different kind of libration is "Lissajous orbits" which in the limit of small amplitudes would become simply 2D (or 3D) harmonic oscillator trajectories, i.e. "Lissajous curves." These in general will not be closed curves and hence not periodic (if the harmonic oscillator frequencies have any irrational ratios) and will pass arbitrarily near (and in some cases actually pass through) L4. but Lissajous curves with all-rational frequency-ratios are periodic and then usually would stay bounded away from L4 (example plotted at right). Lissajous curves usually are **smooth**. The simplest possible periodic Lissajous curve would be an ellipse centered at L4. But it is theoretically possible for them to have corners, for example the curve X=sin(t/2), Y=sin(t/3) with period=6π has a corner at X=Y=t=0. For the case of irrational ratios Lissajous curves will fill a rectangle in the plane. In any case, Lissajous orbits only happen *in the limit of tiny libration amplitudes* causing these Trojan orbits to be virtually unobservable by astronomers – or even if they could observe them, they'd be comparatively hugely distorted by perturbations from other planets, so they couldn't observe them. Astronomers can only detect and plot libration-orbits for Trojans with *large* librations. Those certainly exist and like Lissajous can be smooth and cusp-free, but no longer look like Lissajous curves. Below we show (a) an approximation to the actual libration orbits of Dione's two Trojans, which as you can see are enjoyably near-maximally simple (warning: in libration-orbit drawing a, radial direction is exaggerated by factor 200; drawings in rotating frame with Dione "stationary" by Niederman et al.) and (b) also of two Trojan and Greek asteroids (in rotating frame with Jupiter "stationary," orbits drawn by R.Greg Stacey & Martin G. Connors; 2002 GY_{162} later was renamed "195495") whose orbits are not so simple.



Unlike Frnka's, the orbits of Polydeuces, Helene, and 195495 are unaffected by Hill "zero velocity surfaces" because they pertain to bodies whose Jacobi constants J are *less* than the minimum possible value J(L4) for a "zero velocity" point. For that reason, they cannot have cusps. [In contrast, Frnka's asteroid had J *greater* than J(L4). Trajectories like Frnka's that stay bounded away from L4 perhaps could be argued not to be mathematically-"small" perturbations, although we certainly can make the excluded Hill region have arbitarily small diameter.] **Very few Jupiter Trojans have cusps** in their libration orbits. The reason I claim that is because when Rabe 1971 computed the Jacobi constants of all Trojans he knew at that time, he found they ranged **between J=2.673** (1208 Troilus), **and J=2.986** (1143 Odysseus & 1647 Menelaus), whereas in order to permit cusps, we would need J>J(L4)≈2.999. More Trojan-J vaiues were computed independently with improved theory and better accuracy by Vrcelj & Kiewiet de Jonge 1978. They confirmed most of Rabe's values except for Rabe's final decimal place, confirmed 1208 Troilus as the Trojan in their sample with the least J, and confirmed Jacobi's theorem by showing the J-values stayed constant except in their final decimal place across 63 years of astronomical observations for 13 Trojans. They also claimed to find two uncatalogued Trojans in the 1970 "Palomar-Leiden survey of faint minor planets" with new greatest-J records of J=3.0012 and J=3.0006. These were the only two Trojans known to them with J>J(L4)≈2.999, and hence the only two whose orbits *might* have cusps. I wondered whether these two large-J Trojans might have been catalogued during the years 1978-2025 and hence looked them up in the IAU minor planet database – they were not there. This makes me worry that those two asteroids actually do not exist! However, if you look carefully with a magnifying glass, you can see that the orbit of the Trojan asteroid 82055 (diam.≈25km) **has a cusp** near the center of its orbit-coils near L5. That proves its J>2.999 and makes this observation by me now, the first catalogued Trojan asteroid with a cusp-orbit! The rarity of Trojans with J>2.999 is quite remarkable considering that it seems easier to construct Trojan-orbits numerically with J>2.999 than it is to construct ones with J<2.999. For example, when Rabe 1961/2 generated 20 exactly-periodic hypothetical Trojan orbits in the plane circular restricted 3-body problem approximation of Sun-Jupiter-Asteroid using an IBM computer, all of them turned out to have 3.00286<J<3.0064 and all appeared stable. This probably is trying to tell us

> **Cusp Hypothesis:** For some poorly-known reason, having cusps and/or having J>2.999 destabilizes Jupiter Trojans, and by enough to kill them over gigayear time scales.

Rabe 1961/2 in fact conjectured something like that:

> "The unstable orbits computed by Thüring 1959 ... suggest that repeated close approaches to the curve of zero velocity always precede the 'escape from L4/L5' event... [so] it seems quite conceivable that the borderline between stability and instability may be marked by orbits just able to make contact with and be deflected by [Hill surfaces] of zero velocity. [I.e, orbits with cusps.] The study of such trajectories should at least shed additional light on the stability problem."

A citation search revealed that *nobody* had followed up Rabe's conjecture during the 63 subsequent years (at least, nobody who cited Rabe). But I believe there is some amount of truth to it. Cusps in Trojan orbits would probably be hard for astronomers to detect directly because orbits of this ilk tend to be "long and thin" and we are viewing them (from Earth) nearly "edge on." But the cusp hypothesis plainly is not foolproof always (or maybe should only be used for Jupiter), since there seems to be a cusp, or near-cusp, in the orbit of one of *Neptune*'s L5 Trojans, "2011 HM102" (diam≈100km, orbit lifetime≳1 Gyr).

The angular swings in the librations of Jupiter's Trojan/Greek asteroids have pseudo-periods 137-170 years and apparently can continue forever without hurting their 1:1 mean-motion resonance, even for librations that are quite large in an angular sense and/or in terms of inclination away from the plane of the ecliptic. As two of the more-extreme and prominent examples: The brightest Trojan 3451 Mentor has inclination 24.65° off the ecliptic; and the Greek 1437 Diomedes oscillates angularly between about 35.6° and 99.6° ahead of Jupiter as it traverses its tadpole orbit. An orbit which stays in the vicinity of L4 without approaching L5 (or vice-versa) is called a "tadpole" orbit. These are commonplace for Jupiter Trojans and Greeks. Orbits which pass near *both* L4 and L5 are called "horseshoe" orbits and are comparatively rare, in fact zero horseshoe-orbit Jupiter Trojans are known. This absence was explained when Cuk, Hamilton, Holman 2012 found in numerical integration experiments that "horseshoe co-orbitals are generally long lived (and potentially stable) *for systems with primary-to-secondary mass ratios larger than about **1200**.*" The reason there are **no horseshoe** asteroids, then, is because the Sun/Jupiter mass ratio happens to be only 1048. The horseshoe orbit for Saturn-Janus-Epimetheus exists because the Saturn/Janus mass ratio≈3×10^8 far exceeds 1200, providing no obstacle to stability.

Frnka et al's pictures illustrate that the detailed shapes of tadpole and horseshoe orbits can be very complicated (and in general aperiodic), even in the planar circular restricted 3-body case; if we drop the adjectives "planar" and "circular," we should expect even greater complexity. However, orbits like Frnka's appear to be confined in regions with comparatively **simple-looking "outer envelopes,"** i.e. region-boundaries in the rotating-with-jupiter coordinate-frame. The motions of many of the known Neptune Trojans and Greeks under the influence of the 4 gas-giant planets (and sun) are shown, in a "fixed Neptune" rotating reference frame, in a 2016 video by Alex Harrison Parker. NASA put out an analogous video for some of Jupiter's Trojans and Greeks in 2021.

Asymptotic behavior of Lagrange point positions and their J values. In the asymptotic limit of Sun/Jupiter mass ratio $X\to\infty$ (reasonably valid since in reality $X\approx 1048$) and Jupiter/Asteroid mass ratio→∞ (also quite valid since even for the heaviest asteroid Ceres, this ratio is 2×10^6) the locations of L1, L2, and L3 are (assuming circular planar, and in units where the Sun-Jupiter distance is 1):

- L2 and L1 are $(3X)^{-1/3}$ outside and inside of Jupiter, while
- L3 is on the other side of the sun, distance $1-(7/12)X^{-1}$ away from it.

The Jacobi constant J-values at the Lagrange points obey J(L4)=J(L5)<3<J(L3)<J(L2)<J(L1), and they all approach 3 when $X\to\infty$, with

$$J(L1)\approx 3+3^{4/3}X^{-2/3}-0.886\,X^{-1},\quad J(L2)\approx 3+3^{4/3}X^{-2/3}-2.24\,X^{-1},\quad J(L3)\approx 3+X^{-1},\quad \text{and}\quad J(L4)=J(L5)=3-X^{-1}.$$

(Warning: I did not compute the constants 0.886 and 2.24 exactly; they are only *approximate* and probably considerably less accurate than the number of decimals I gave.) The constant-J contours that pass through the Lagrange points are of particular importance since for L1, L2, L3 they are "**seperatrices**," e.g. in the case of L1 separating a region "Jupiter owns" versus a region the Sun "owns." And J(L4)=J(L5) is important because it is the minimum possible Jacobi value a "zero velocity" point can have.

Trojan Lifetimes. The gigayears of solar system Jupiter-Trojan empirical evidence indicate that, at least for many Lagrange triangles, librations stay bounded. Or at least, they clearly usually do *not* quickly grow to infinity: if they do grow to infinity, that growth is very slow; or if fast growth is possible, then it occurs rarely. But instead of resorting to empirical evidence, what if we apply *mathematics*?

2.6.3 Lack of "Lyapunov stability." Other, more-provable "stability" notions. A very strong possible claim – much better than Routh's "linear stability analysis" – would have been a proof of Lyapunov stability, which for us shall mean proving some formula giving an upper bound on worst-case libration amplitudes, which goes continuously to zero for all small-enough initial perturbations. Unfortunately up to year 2025 nobody has ever been able to prove Lyapunov stability of *any* unrestricted 2- or 3-dimensional N-body solution for any N≥3.

And for Lagrange triangles in *three* dimensions no such bound can exist, because **Sosnitskii 2008's full nonlinear instability analysis** (see his p.2539-2540) claims that at least *some* arbitrarily-small perturbations exist in $\mathbb{R}^3$, which cause librations which can eventually attain large amplitude. We shall re-show Sosnitskii 2008's result in a far simpler way later. In the cases where the Gascheau-Routh-Roberts inequality is satisfied, the fastest-growing infinitesimal perturbation necessarily grows *sub*exponentially, but nevertheless unboundedly.

The implications among six different "stability" notions for Hamiltonian systems of ordinary differential equations are as follows:

Full ⇒ Lie(only applicable for "equilibrium" solutions) ⇒ Lyapunov ⇒ Nekhoroshev ⇒ KAM ⇒ Linear
If ≤2 degrees of motional freedom, then: KAM stability ⇔ Full stability. (I call this "2D KAM theory" for short.)

"Linear" stability means the absence of exponentially-growing infinitesimal perturbations. Such perturbations to a known solution obey linear ODEs, therefore with solutions expressible in terms of exp(Mt) where M is a matrix, and what matters is whether any eigenvalues λ of M obey |λ|>1. KAM and Nekhoroshev theory will be discussed later. The notion of "Lie stability," or at least that name, was introduced by Santos 2010. "Full" stability means that all small-enough perturbations (say smaller than ε>0) stay small eternally (e.g. smaller than δ, for some δ>0 dependent upon ε, and approaching 0 when ε does).

Rabe 1967 attempted to extend Routh's first-order L4 stability analysis to *third* order in the perturbation-size. Almost all Trojan/Greek asteroids indeed obey Rabe's approximate stability criterion, with the notable exception of 5144 Achates. Numerical integrations indicate 5144 Achates nevertheless is stable over ≥100 Myr timespans (over 1000× its Lyapunov time of 91 kyr) despite both Rabe's test, and the exceptionally large inclination-angle variations (between about 0 and 10°) observed in simulations of Achates.

Computer simulations (Brasser, Heggie, Mikkola 2004, confirmed by Pitjev & Sokolov 2004) of the 3D restricted circular 3-body problem in parameter-regions occupied by few or no actual librating-Trojan asteroids discovered something rather remarkable: **apparently-eternal chaotic librations of Trojans**. By "apparently eternal" I mean: Trojan asteroids are in 1:1 resonance with Jupiter, and an "escape" from that resonance is readily detectable: the asteroid's orbital longitude crosses Jupiter's. If no such crossing ever happens, then the 1:1 resonance is "eternal." (We may similarly define eternality for other asteroid-Jupiter resonance, for example the Hilda asteroids, for which the resonance is 3 Jupiter orbits per 2 asteroid orbits. Also we could, more strongly, demand that the asteroid longitude remain between, say, 5° and 115° ahead of Jupiter's – that also was satisfied by Brasser et al's simulated Trojans.) Brasser et al's librations are "chaotic" in the sense that infinitesimal perturbations to the initial conditions exist, which grow exponentially with time – and eternally if the chaos is "eternal," which it apparently is. In the cases discovered by Brasser et al, the factor-e≈2.71828 growth time ("Lyapunov time") is 100-300 Jupiter-orbit periods, i.e. 1186-3600 Earth years. But the 1:1 resonance survives for as long as they run their simulator, which ranged from 10^4 to 10^7 Lyapunov times. The asteroids they found to exhibit this behavior have high *inclinations* off the Jupiter-Sun plane (unlike the vast majority of actual Trojan asteroids). Specifically, the "critical" inclination angle separating chaos above it from nonchaos below it approximately equals $(1-1.123\mu)\cdot 61.5°$ where $\mu=M_{jupiter}/(M_{sun}+M_{jupiter})$ and in our solar system $\mu\approx 1/1049$.

Many actual Trojans also exhibit long-term chaotic librations despite having inclinations well below 61.5°. But their chaos presumably is not attributable to the simplest-possible circular restricted 3-body model, but rather requires either Jupiter-eccentricity and/or additional planets.

2.6.4 Complete understanding of stability in planar circular restricted Lagrange-triangle special case. Leontovich 1962 was the first to apply KAM theory to the rigidly rotating Lagrange-triangle CPR3BP. Let the masses of Lagrange's three bodies be M, 1-M, and negligibly tiny (for 0<M≤½). By the Gascheau-Routh criterion, this system is unstable to exponentially growing perturbations when $M>M_{Routh}=2/(27+3\sqrt{69})\approx 0.0385208965$. The borderline case $M=M_{Routh}$ also in unstable as is shown by discussion on p.62 of Meyer & Hall 1992 about "non-diagonalizable matrix" (cf. their theorem 1 on p.57). Leontovich proved KAM stability of the Lagrange-triangle CPR3BP for every M with $0\le M<M_{Routh}$ *except* an unknown zero-measure subset. In the planar case Deprit & Deprit-Bartholome 1967 improved over Leontovich by showing KAM stability for every M with $0<M<M_{Routh}$ except for these three:

$M_{DD1}=(1-[(3265+2(199945)^{1/2})/483]^{1/2}/3)/2\approx 0.010913667677$, $M_{DD2}=[1-(1/45)\sqrt{1833}]/2=32/(675+15\sqrt{1833})\approx 0.0242938971420523$, and $M_{DD3}=[1-(1/15)\sqrt{213}]/2=2/(75+5\sqrt{213})\approx 0.01351601602245$ (M_{DD3} was stated incorrectly by D&DB p.178, but hopefully I have corrected them.) Finally, Meyer & Schmidt 1986 settled the problem completely by showing KAM stability for M_{DD1}; while for both M_{DD2} and M_{DD3} *in*stability was shown by Markeev 1969 and Alfriend 1970 & 1971. These two unique unstable cases arise from 2:1 and 3:1 rational-ratio "resonances" among critical KAM "twists." Finally, 2D KAM theory shows all the KAM-stable cases here are **fully stable**, i.e. all small-enough planar perturbations of the asteroid initial conditions will remain small eternally.

The possibility of small *three*-dimensional perturbations does not alter linear stability in the above problem, but could sometimes destroy KAM stability and presumably will destroy full stability. Leontovich's result remains true even in 3D. Carcamo-Diaz, Palacian, Vidal, Yanguas 2020's theorem 3 shows that all the KAM-stable planar cases above are "Lie stable" in 3D *except* possibly for cases in which the three characteristic "KAM twists" $(\omega_1,\omega_2,\omega_3)$ given in their EQ 1.3 are proportional to a Pythagorean triple (integers a,b,c with $a^2+b^2=c^2$). They also give "Nekhoroshev estimates" in their theorem 4.

Schwarz, Bazso, Erdi, Funk 2012 attempted by brute force computer simulations to determine the region of 3D-full-nonlinear stability for the restricted 3-body equilateral-triangle solution, finding excellent agreement with Roberts 2002's region from mere 2D linear analysis; the topmost discrepancy was "Sicardy's bump," on the right side of their fig.2 near the horizontal axis, which we'll discuss in the section on confined chaos.

2.6.5 "Regular N-gon plus sun." Roberts 2000 (justifying the original nonrigorous work on this by J.C.Maxwell in his 1859 Smith prize essay *On the Stability of Motions of Saturn's Rings*) considered the rigidly rotating (N+1)-body solution consisting of one central "sun" (mass M) and N equal co-orbiting "planets" (each mass=m) at the vertices of a **regular N-gon**. He proved that if 2≤N≤6 then exponentially-growing infinitesimal perturbations exist, i.e. this solution is unstable. (However, Salo & Yoder 1988 found that for 2≤N≤8 there is a unique *non*equally-spaced stable configuration for the N planets.) But for each N≥7, Roberts proved *no* exponentially-growing infinitesimal perturbation exists *provided* M/m>F(N), for a certain positive-real-valued function F(N) with $7^{-3}F(7)=0.4078\le N^{-3}F(N)\le 0.43503$ while $N^{-3}F(N)$ is asymptotic to $7\zeta(3)(13+\sqrt{160})/(16\pi^3)\approx 0.435036580988$ for large N. A complicated explicit formula for F(N) was also found by Vanderbei & Kolemen 2007, and their computer simulation experiments indicated that Roberts' linear-stability condition appears to exactly (or indiscernably nearly) equal the condition for full nonlinear stability.

What I like about this "N-gon plus sun" configuration is its high symmetry. Consequently, its quadrupole moments remain constant (and if N is large enough, then the octupole and higher moments remain constant too; the larger the N, the more moments stay constant). Consequently, its gravitational wave emission wattage is tiny. This causes its orbital **decay** from gravitational wave emission, to be **arbitrarily slow** for large N. Specifically, we saw earlier that lifetime is proportional to $G^{-3}c^5R^4M^{-2}m^{-1}$ with a single planet (N=1) if m≪M. That also would be true for N=2 co-orbiting permanently-antipodal hypothetical planets. But with N≥3 equal-mass planets at the vertices of a rigidly-rotating regular sun-centered N-gon (in practice only for N such that this orbit is stable, i.e. N≥7), gravitational-wave energy-loss caused lifetime instead should be proportional to

$$G^{-1-N}\, c^{1+2N}\, R^{2+N}\, M^{-N}\, m^{-1},$$

which grows *exponentially* as a function of N for large N (assuming R initially exceeds a sufficiently large number of sun Schwarzschild radii). And since this is a *rigid* rotator, it also could enjoy *zero* tide-caused orbit-decay.

2.6.6 Other periodic solutions from Fourier/min-action and related rigorous techniques. The 3-body problem with some parameter tuples exhibits chaos, but with other parameter tuples is non-chaotic. For example, the Sun-Earth-Moon system (ignoring the rest of the solar system) empirically appears to be **non-chaotic** and quasi-periodic, and that seems even more clear for the Sun-Pluto-Charon system. This first became reasonably clear when George William Hill (1838-1914) was first able to predict the motion of the moon to high accuracy. Roughly speaking, Hill's technique was to write a Fourier-like series describing earth-moon-sun motions in a rotating coordinate system, then choose the constants inside that series to minimize the "action." The action is the time-integral of the "Lagrangian," which in turn equals the kinetic minus the potential energy. (In 1895-1947 A.Lyapunov and A.Wintner proved Hill's series converges.) Such theory formed the basis for the 4 volume Tables of the Motion of the Moon published by Ernest W. Brown (1866-1938) in 1919 which continued to be used, with correction for cumulative tide-caused slowdown of day-length (an effect Hill had ignored), until 1983. It usually provides accuracies of 1/1000 arc-second and is one of the most impressive accomplishments of precise physical prediction in the pre-computer era. (Eventually laser ranging from reflectors placed on the Moon could determine its distance to better than ±1 cm accuracy, which in combination with computerized numerical integration outperformed Hill-Brown theory.) The moon-center to earth-center distance (if we ignore tidal losses) appears to stay permanently bounded between 356 and 407 Mm, with mean value 385. With the advent of computers, it became possible to use the Fourier/min-action technique to seek *exactly*-periodic or Poincare-periodic 3-body orbits (the Sun-Earth-Moon system is not either). An impressive success of that technique was Cris Moore's 1993 discovery of the "Moore8" 3-body "choreographic orbit," discussed in an appendix.

A **direct computer implementation of the Fourier/action-min idea** was built by **Vanderbei 2004** for the purpose of seeking *time-periodic* N-body solutions. (Moore 1993 and Nauenberg 2001 built something similar earlier.) Vanderbei found numerous examples with N=3 and N=4 accurate to (say) 10 decimal places, available as viewable movies, but did not *prove* their *existence*. That is, he found initial conditions, which yield *approximately* time-periodic solutions of N-body problems, accurate to some large number of decimal places; but did not *prove* these were near initial conditions yielding *exact* time-periodicity. (I have no doubt they were; I am just saying: Vanderbei did not prove it.) Vanderbei then also claimed that some of his solutions were unstable (i.e, infinitesimal errors in initial conditions exist that grow exponentially as a function of T after T periods) while others were not. Those claims were based on running two numerical ODE-integrators for an hour of computer time and either seeing huge errors build up after a few seconds, or the errors stay small for an hour. Again: while I have no doubt Vanderbei's conclusions about *stability* were correct, these were not proofs.

However, methods are known that are capable of **proving** such existence and infinitesimal-stability claims. We can, essentially by "algorithmic differentiation," compute the matrix mapping infinitesimal changes in conditions at orbit-start, to infinitesimal changes in conditions at its end. This matrix will be calculated only approximately if a numerical integration method is used, but can be done exactly in the rare cases where an exact analytic solution for the N-body orbit is known. In the approximate case, it is necessary to have bounds on the errors in the matrix entries; these can often be produced by using "interval arithmetic" and rigorous error bound theorems for numerical-integration methods. We then compute the eigenvalues of the matrix. If any eigenvalue z provably obeys |z|>1, then we've proven instability of the periodic orbit. If we can prove (despite the numerical errors!) that every eigenvalue exactly obeys |z|=1, then we've proven that kind of instability does not happen. To do the latter, it is necessary to use the theorem that the exact matrices those methods are trying to find necessarily are "symplectic"; and to use "symplectic numerical methods" so that we actually compute an exactly-symplectic matrix (still only approximately, but it's exactly symplectic). Roberts 2007 carries out this process for Lagrange's "rotating equilateral triangle" and the Moore-Chenciner-Montgomery "figure-8 choreography" 3-body solutions (in both cases with all three masses equal) finding the exact matrix in the former, and an accurate approximate matrix in the latter, case.

Roberts then shows Lagrange's instability by finding its eigenvalues $\exp(\pm\pi\sqrt{2})$ include numbers>1; and shows the figure-8's stability by means of his "lemma 4.1" about eigenvalues, which enables him to prove all its eigenvalues must lie on the unit circle, and he evaluates approximately where.

Initial conditions yielding exact periodicity may be obtained by "Newton's iterative method" extremely precisely. That is: since you know that matrix, you can deduce, to first order, the small changes to your initial conditions that you need to make the final state match the initial state. Make them. Then, compute the new matrix and repeat the whole process with ever-increasing accuracy each redo. The fact that this process converges will be evident, but to prove it you need some sort of topological or convexity claim, e.g:

1. If you can prove that matrix cannot change very much when the orbit initial conditions change slightly, then that can be used to prove Newton must converge, hence a periodic solution with initial conditions near your approximate values, must exist. E.g. I claim Roberts' calculations thus establish *existence* for the figure-8, in a rather simpler way than Chenciner & Montgomery's proof.
2. If you can prove the "action" is somehow locally concave-∪ in some space of orbit-perturbations, that proves an action-min must exist, proving a periodic

> Newton N-body solution must exist nearby. E.g. see Chen 2008 where he proves in this kind of way that an infinite number of never-colliding planar 3-body periodic solutions, not necessarily with equal masses, exist, and gives some examples, including the example Vanderbei had called "Ducati3" but Moore has called the "CrissCross," which both had found empirically to be stable.

Hill proved existence of various periodic planar 3-body orbits in the 1800s (see sections II.18-24 and Chapter XI of Brown 1896); and Poincare proved an infinitude of such orbits exist by inventing the first topological fixpoint existence theorems.

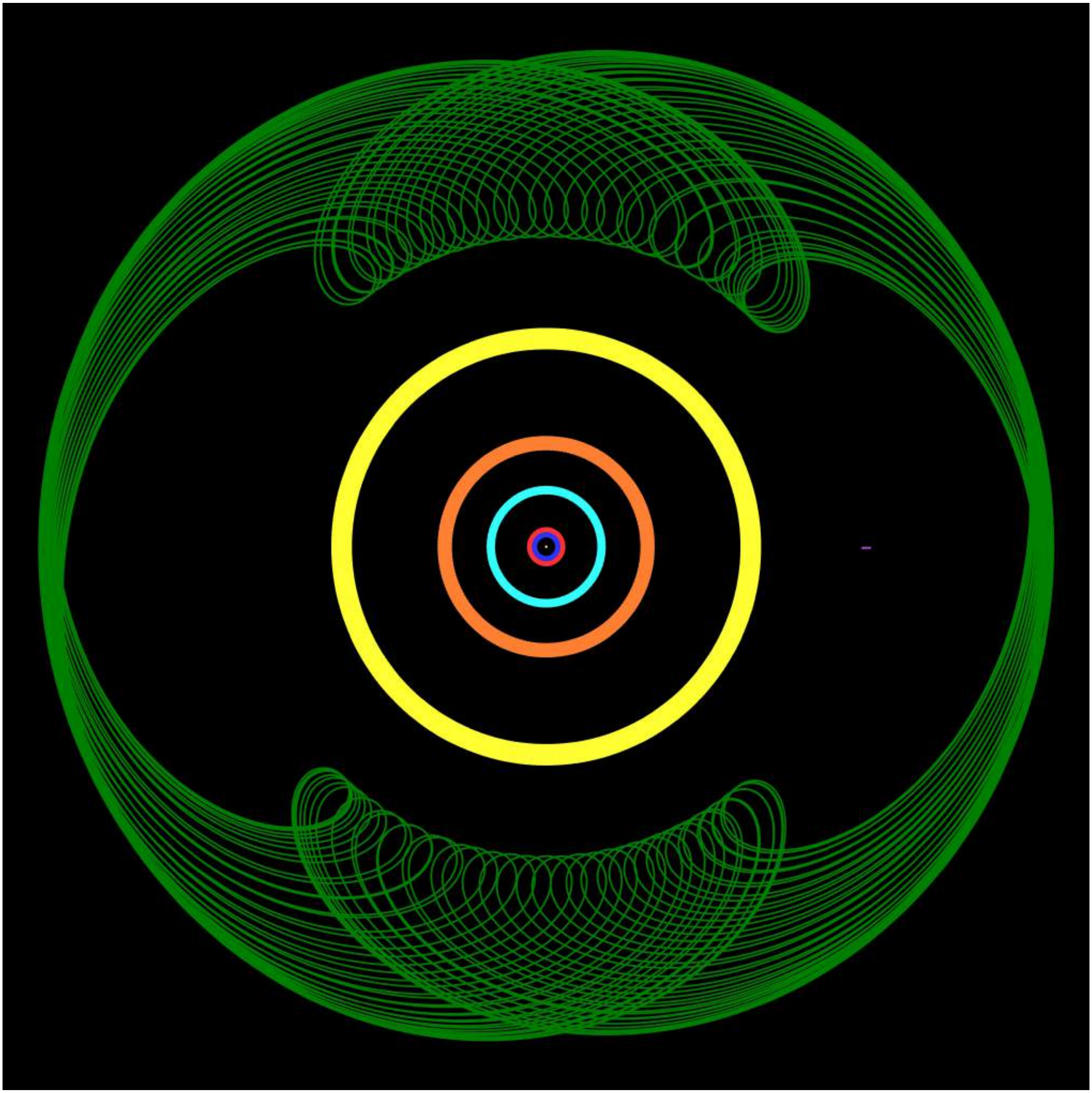
2.6.7 Solar system "stability" and "chaos."

Definition: "Chaos" means the maximum possible error, arising from infinitesimal errors in initial conditions of some ODE (ordinary differential equation) is lower bounded by an *exponentially*-growing infinitesimal function of time; and this persists for arbitrarily long times. The factor=exp(1)≈2.71828 growth time of that exponential (assessed in the limsup sense for large times) is called the **"Lyapunov time."** It is possible for the time-evolution of a Newtonian N-body system with any small finite number N≥3 of bodies, to be "chaotic." However, Newtonian 2-body systems are never chaotic because infinitesimal errors in their initial conditions grow at most linearly with time.

> Chaos is when the present determines the future, but the approximate present does not determine the approximate future.
> – Edward N. Lorenz (1917-2008).

General relativistic 2-body systems (two spinning black holes) empirically can, with the right parameter sets, exhibit chaos. For example Zelenka et al 2020 observed in computer simulations that black hole binaries with one tiny-mass *spinning* hole orbiting a large-mass Schwarzschild (non-spinning) hole can exhibit "chaos" in the sense of exponential amplification of infinitesimal perturbations in initial conditions at astrophysically achievable parameter values – which lasts as long as the system lasts. Indeed (Hartl 2003) chaos is quite common in the relativistic regime if both holes spin, and becomes more common with more spin-orbit misalignment and larger spins. Lyapunov times can be as short as 5 orbits. But we shall not use these facts because we are going to stay with systems well-described by Newton and black holes well-described by Schwarzschild (i.e. non-spinning).

The Sun-Neptune-Pluto system empirically is **chaotic**. (And note: Pluto has high inclination≈17° and eccentricity≈0.249, and crosses Neptune's orbit e.g. was actually closer than Neptune to the Sun during 1979-1999, although Pluto is further about 92% of the time.) A 2D projection of about 20000 years of numerical integration of the planets in our solar system, is shown in the figure below computed by R.J.Vanderbei. (There also is a movie showing the time-evolution of the orbits of the 4 largest planets, plus Pluto, plus the "Plutino" Orcus whose orbit is very similar to Pluto, in a fixed-Neptune rotating reference frame. Both Pluto and Orcus are trapped in 3:2 mean-motion resonances with Neptune hence both have exactly the same period. Both cross Neptune's orbit to occasionally be closer to the sun than Neptune, but neither can ever get nearer than about 18 AU from Neptune itself (versus the Sun-Neptune distance ≈30 AU) because due to the resonance the orbit-crossings always have orbital-longitude quite different from Neptune's. Orcus and Pluto's elongated-ellipse orbits point in nearly opposite directions, with one being furthest from the sun at about the same time the other is nearest. That is why Orcus has been called "anti-Pluto." This relationship should persist forever due to the exact equality (in a mean-motion sense) of their orbital periods forced by their resonances with Neptune. Another video shows Kuiper belt object 1994 JR1, later renamed 15810 Arawn, which *also* is in a 3:2 resonance with Neptune, but unlike Pluto and Orcus never comes closer to the Sun than Neptune. Approximate diameters in km: Naptune=49000, Pluto=2377, Orcus=915, Arawn=142.) Vanderbei's figure is viewed in a rotating reference frame intended to keep Neptune on the positive X-axis and the sun at the origin. (Neptune's positions: the short horizontal purple line segment.) Each planet's orbit has its own color. The orbits of Uranus, Saturn, Jupiter, Mars, and Earth then appear as annular bands because the spacings between their curves are smaller than 1 pixel. The very fancy green curve is the orbit of Pluto. Notice that Pluto's orbit, despite being "chaotic," appears to be "confined." Apparently, due to some effective kind of "repulsion," Pluto never crosses Neptune's orbit closer than about 35° to Neptune's angular position; and if so can never collide with Neptune and never escape from the 3:2 mean-motion resonance. This "confined chaos" behavior has been confirmed by 5 Gyr numerical integrations using all planets (i.e. >20 million Pluto orbits and >250 Lyapunov times) and 100 Gyrs using the outer 5 planets only (Ito & Tanikawa 2002; this is >4×10^8 Pluto orbits and >5000 Lyapunov times; see also our later discussion).



The restricted 3-body problem was already shown by H.Poincare in 1889 often to be chaotic. And even subject to the further restrictions that the two primaries move in a *circular* orbit, while the third (light) body stays in its *plane*, we still can have chaos in certain positive-measure parameter sets according to **Vrbik** 2013: e.g. "sun" with mass=1, "Jupiter" with much smaller mass ε and period=7; and "asteroid" with negligibly tiny mass and period≈4; Lyapunov times of order ε^{-1} Jupiter-periods. (Vrbik's theoretical "order ε^{-1}" Lyapunov time estimate, by the way, *agrees* with the observed fact that Pluto's Lyapunov time 10-20 Myr is equivalent to 40100-80200 Pluto orbits, i.e. 26750-53500 Neptune orbits, indeed of the same order as the Sun/Neptune mass ratio 19410; and agrees with the observed fact that 522 Helga's Lyapunov time≈6900 years, i.e. 582 Jupiter orbits, which indeed is of the same order as the Sun/Jupiter mass ratio 1048.) Vrbik's period ratio≈7:4 seems not to happen for known asteroids with diameter>100km, the closest matches being members of the "Cybele asteroid group," e.g:

Name	diameter	eccentricity	inclination	resonance	Lyap.time
107 Camilla	200km	0.0906	18.797°	20:11	37kyr
909 Ulla	115km	0.0906	18.797°	16:9	>30kyr

(missing)				7:4	
522 Helga	101km	0.083794	4.4174°	12:7	6900yr
65 Cybele	263km	0.1114	3.5627°	5:3	3546yr
Jupiter	139822km	0.0489	1.303°	1:1	

Computer investigations indicate that several aspects of our own solar system are chaotic, causing numerical integration to be incapable of predicting planet positions accurately after 130 Myr:

1. According to direct numerical integrations of Newton's laws by Sussman & Wisdom 1988, confirmed by Wisdom & Holman 1991, the Pluto-Neptune-Sun subsystem empirically is chaotic with a Lyapunov time scale of 10-20 million years, i.e. $(4\text{-}8)\times10^4$ Pluto orbital periods. Although this technically causes the entire solar system to be "chaotic," since Pluto is small and far away its effect on the other planets fortunately is small. Similarly Jupiter's moons are believed to be chaotic but again fortunately that has little effect on the rest of the solar system.) Then Sussman & Wisdom 1992 found that for the "solar system as a whole," the Lyapunov time is 4 Myr; and for the Sun plus 4 heaviest planets only, 5-7 Myr.
2. Laskar 1989, using LeVerrier "period averaged perturbation" approximate integration techniques, found that the Earth's orbit, (and the orbits of *all* the inner planets) empirically are chaotic with Lyapunov time≈5 Myr. If so, a 10 meter error in measuring the position of the Earth today could make it impossible to predict where the Earth would be in its orbit (the angle would be roughly uniformly random from 0 to 360°) after 125 million years.
3. Laskar & Gastineau 2009 ran 2501 simulations using LeVerrier style "period-averaged approximate equations," varying the initial position of Mercury by about 1 meter between one simulation and the next, and including GR post-Newtonian corrections and the Earth's moon. In 20 cases, Mercury went into a dangerous orbit, often ending up colliding with Venus or the sun within 5 Gyr.
4. Brown & Rein 2020 empirically found that errors as small as a millimeter in measuring today's position of Mercury could make it impossible to predict its orbital eccentricity in just over 200 million years. And in 2023 they found that using general relativistic improvements over Newton's laws substantially change the solar system's long-term chaotic character versus plain Newton. That is naively surprising because GR seems to play only a tiny role in the solar system. The largest GR effect seems to be precession of Mercury's perihelion by 43 arc-seconds per century, i.e. about 1 part in 30000 of the circumference of Mercury's orbit per century, which as a distance is about 13000 km (i.e. 2.7 Mercury-diameters) per century. This long term trend for Mercury is large enough to be observable – and has been observed, over centuries – *but* is more than 10× smaller than purely Newtonian effects from the other planets which *also* continually change Mercury's perihelion – also producing a long term trend, although with much more short-term variability. [The nonspherical shape of the sun also causes perihelion precession for Mercury, but its effect is at least 4000 times smaller than the GR precessionary effect according to Park et al 2017.] Brown & Rein claim that these small GR effects tend to desynchronize what otherwise would be damaging resonances, thus considerably lengthening Lyapunov times. Such desynchronization makes sense in view of the fact that GR's effect on Newtonian 2-body problems is to *stop* them from being periodic, and to keep altering the orientations of planetary ellipses at rates different for each planet. I.e, Mercury's orbit completely precesses 360° back to nearly its original orientation each 3 Myr, suggesting that Mercury's "true" period is at least 3 Myr. For the next five planets the "true period" lower estimates in view of their GR precessionary effects would (in a 2-body system: sun and that planet only) be Venus 1.5×10^7 years, Earth 3.4×10^7 years, Mars 10^8 years, and Jupiter 7×10^8 years.

"Topological Universality" for planar 3-body problem. Moeckel & Montgomery 2016 proved Poincare-periodic planar 3-body solutions are rich enough to "do anything you want." More precisely, in the planar case we can have notions of trajectory "topologies." M&M introduced the notion of an "eclipse": an instant when three of the N point-masses are *collinear*. The middle body is labeled by an integer from the alphabet {1,2,3, ..., N}. For example if masses 3-1-2 were collinear in that order, then that eclipse's "type," i.e. the middle body's label, would be "1." An "eclipse sequence" is a word from that alphabet, such as 21332111312111113, regarded as periodically wrapped. We may "reduce" an eclipse sequence by repeatedly removing exactly two consecutive occurrences of the same letter, until no more remain: 21332111312111132→212131211132→2121312132→12131213. At least if we ignore the periodic-wrap issue, there is a 1-to-1 correspondence between "reduced eclipse sequences starting with 1" and "binary words": make the difference between ternary digits $a_{n+1}-a_n$ in the reduced eclipse sequence, congruent modulo 3 to $1+b_n$ where b_n is the n^{th} digit of the binary word. (If we do not ignore wrap, then really it is not binary "words" but rather binary "necklaces" with sum divisible by 3.)

Theorem (Moeckel & Montgomery 2016): *Every* reduced eclipse sequence is attainable as the chronological sequence of eclipse-types that occur during some Poincare-periodic planar never-colliding 3-body motion obeying Newton's laws (indeed one with all three masses equal, and also one with all three different but near-equal; in both cases with nonzero total angular momentum and negative total energy).

Remark: A different kind of "universality" arises from Sundman and Wang's theorems. Their analytic-solutions-as-series constructions make it clear that as their variable z approaches 1 from below, we generically get an essential singularity. Then Picard's great theorem tells us that *every* complex value (with at most one exception) occurs infinitely often as the value of any analytic function F(z), within any z-neighborhood, no matter how small, of the location of any essential singularity.

Remark: All the motions arising from M&M's proof technique are chaotic/unstable, but for the three particular eclipse sequences 12 (Pluto-Charon-Sun type systems; also tight binary stars with faraway near-circular orbiting planet), 123 (Chenciner & Montgomery 2000), and 1232 (Henon 1976) they claimed they also know how to provide KAM-stable examples. The lattermost claim was misleading because Henon 1976 only *conjectured* that the 15 linearly-stable 3-body periodic solutions he'd found, were KAM-stable (see Henon's p.282) – apparently nobody ever proved that. (But: nevertheless M&M were correct to claim that KAM-stable 1232-type periodic 3-body planar orbits exist, as we'll see when discuss KAM theory more later.)

Li, Li, Liao 2020 found 13315 apparently-stable periodic 3-body solutions with near-equal distinct masses – and conjecture infinitely many exist – but again LL&L only have linear-stability proofs, not KAM proofs.

Periodicity Corollary of M&M theorem: At least a countably infinite number of topologically-distinct exactly-**periodic** planar 3-body solutions exist. (Basically, the topological types correspond to the binary expansions of *rational* numbers p/q with odd q>2 – or a countably-infinite subset of these.)

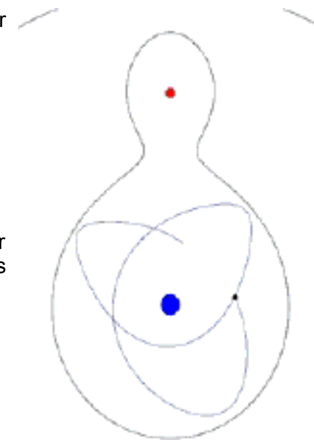
Among those periodic solutions, we shall see that some can be chaotic, i.e. infinitesimal perturbations will grow exponentially (and presumably most arbitrarily-small finite perturbations will not be periodic anymore). We also shall see that *non*chaotic periodic 3-body solutions exist.

(New) Bound *Non*Periodic Theorem: An uncountably infinite number of topologically-distinct ***non*periodic permanently-bound never-colliding** planar 3-body solutions exist, indeed with all inter-mass distances eternally bounded both above and below by positive constants. Furthermore, this also is true (if desired) eternally into *both* the past and future.

Proof sketch: (See also Sitnikov-Alekseev universality theorem.)

Make a tight binary star (both stars equal mass) orbiting approximately circularly with short period (e.g. 1 week), plus a much lighter asteroid whose orbit is (approximately) a high-eccentricity ellipse with one focus at the center of mass of the stellar binary, and period much longer than that of the binary star, e.g. 100 years≈5000 weeks. The ellipse should intersect the binary's orbit-circle at 2 or 4 points. The idea is for the asteroid to, once per asteroid-orbit, choose one among ≥2 distinct possible topological-types of interactions with the binary, then begin another long wait before the next one. These interactions are fast and exciting, involving 0 to 3 close star-flybys. Although in the picture I oriented the asteroid's long thin orbit-ellipse in the rightward direction, each time we allow its orientiation angle to change almost arbitrarily; and that ellipse's length and width can vary (within bounds); and the asteroid can switch between traversing its ellipse clockwise versus anticlockwise. (The *binary star* always orbits anticlockwise, but the *asteroid* can switch.) The asteroid's arrival-time relative to the orbital phase-angle of the binary, and speed, both need to be finely adjusted to make the chosen interaction-topology happen right on interaction #1, and to keep the binary star's orbital diameter approximately constant and its orbital eccentricity small. Very small adjustments will result in large changes during the next asteroid-star interaction, due to "amplification lemmas." This keeps happening: each successive interaction has much larger cumulative "amplification factor" (since the amplification factors keep multiplying), enabling it to be adjusted by successively tinier adjustments of the initial conditions – so tiny they do not noticeably affect any of the prior interactions. The requisite amplification lemmas are in my prior Newton/Church paper (Smith 2006) which in turn was based around the arguments of Gerver 1991, who provides a rather complete set of such lemmas, and still more are in Gerver, Huang, Xue 2022 and Xue 2015.

To obtain the bidirectional (both past and future) claim, apply König's "infinity lemma" to the "tree of possibilities." **Q.E.D.**



2.6.8 Hill regions. Hill considered the circular planar restricted 3-body problem in a xyz coordinate system *rotating* with angular velocity=ω in which the two primaries remain "stationary." (The rotation axis is their center of mass.) Hill also considered "**Jacobi's 1836 constant of the motion**"

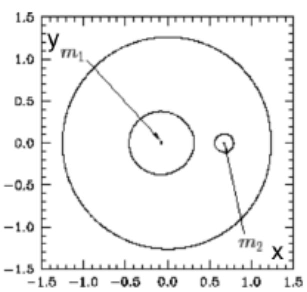
$$J = (x^2+y^2)\omega^2 + 2(M_1/r_1+M_2/r_2)G - (\dot{x}^2+\dot{y}^2+\dot{z}^2)$$

where r_k denotes the distance from the asteroid to body k∈{1,2}.

> **Jacobi's theorem** is that J remains constant throughout the motion. Notice that Jacobi's formula for J takes the form of (-2) times a specific pseudo-energy. The word "specific" here means "per unit mass" for the tiny-mass asteroid. The factor -2 is there for probably-stupid historical reasons and could have been changed to any other constant. The first term arises from the pseudo-potential arising from "centrifugal (pseudo)force"; the second from the (genuine) gravitational potential; and the third from the "kinetic energy" which of course also is only a pseudo-energy since we are in a rotating coordinate system.

Hill concluded from this the existence of **"zero-velocity surfaces"**

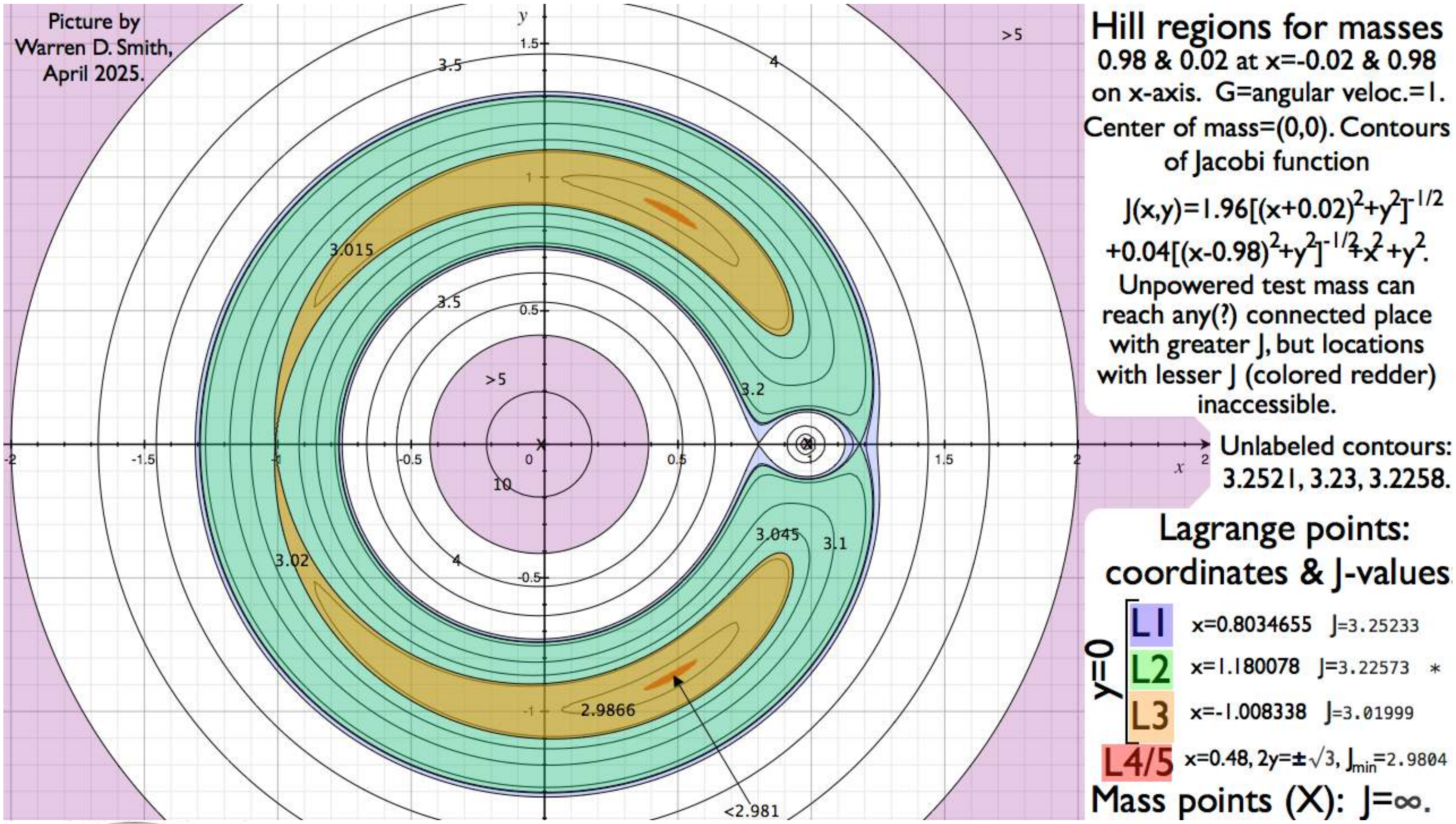
$$(x^2+y^2)w^2 + 2(M_1/r_1+M_2/r_2)G = J$$



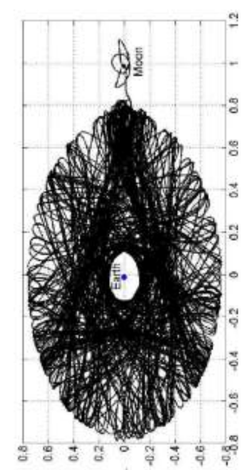
such that, if the "tiny-mass asteroid" point mass were to reach them, it necessarily would have zero speed; and if it were to cross them, it would have negative squared-velocity. [Note: that meant zero speed *in the rotating frame*.] Consequently it is impossible for that asteroid ever to cross the surface, so Hill could prove "confinement theorems" saying the asteroid is permanently confined within certain regions bordered by those algebraic surfaces. These were called "Hill regions." We show two. At left (drawn by Richard Fitzpatrick) M_1/M_2≈7, the center of mass is at (0,0), and the asteroid is *excluded* from the region lying between the outer and the two inner curves. The one at right is an animated picture by Rick Moeckel in which, now, the permitted regions near M_1 and M_2 are connected.

More generally, for any given primary/secondary mass ratio, we can draw a single picture showing *all* its zero-velocity surfaces simultaneously, simply by plotting the contours of J(x,y), which suffices because J(x,y,z) does not depend upon z. This also finds all 5 Lagrange points in the sense that L4 and L5 are (the only) local minima of J(x,y), while L1, L2, and L3 are saddlepoints locally-stationarizing J(x,y). We do that below for mass ratio M_1/M_2=49. In this picture, a test mass with Jacobi constant J≥10 will orbit within one of the three purple regions forever, either circling one of the primaries, or unbounded, or circling the entire binary-system. If its Jacobi constant is J≥J(L1)≈3.25233, then it will stay in the white regions forever, and if J is a bit below 3.25233, then can occasionally transfer between the "near M_1" and "near M_2" white regions but can never breach the "wall" to reach the outer annular white region.

Any test mass with J<J(L2)≈3.22573 has "**universal access**" in the sense that Hill's theorems do not prevent it from going anywhere outside the green region. And indeed, I claim that a test mass started arbitrarily near L2 with arbitrarily small speed relative to L2, can reach any point of $\mathbb{R}^3$ outside the green region, including "escaping to infinity." The fact that the test mass initially has energy well below "escape velocity" from the binary does not prevent its escape to infinity. That is because energy (from the point of view of the test mass alone) is *not conserved* – it extracts energy from the binary in order to escape.



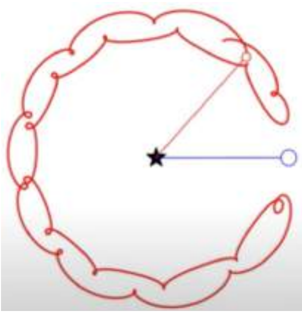
Similarly, test masses with J<J(L3)≈3.01999 enjoy even-greater "universal access": I believe that a test mass started arbitrarily near L3 with arbitrarily small speed relative to L3, can reach any point of $\mathbb{R}^3$ outside its *orange* forbidden-region, including "escape." One **L3-escape trajectory** of this kind is shown at left, drawn by Richard Frnka in blue. A J=J(L3)-ε contour is drawn in black; the two masses lie at the centers of the circles; and the five Lagrange points are black dots.



All this is quite useful for space agencies trying to send spaceprobes to destinations while consuming as little rocket fuel as possible. In the early days of space exploration, they relied on 1925 ideas of Walter Hohmann to plan trajectories by modelling everything as a sequence of 2-body problems (one body being the spacecraft, the other whichever celestial body was currently most affecting it). It is better to model things as a succession of *three*-body problems. It then is possible to, e.g. escape the Earth-Moon system despite expending considerably less fuel than needed to reach Earth-escape velocity 11.186 km/sec; similarly we may cheaply escape the solar system with aid from Jupiter. The trajectory at right by Salazar, Macau, Winter 2011 shows how to enter orbit around the Moon, starting from a circular 59669km-radius "parking orbit" around the Earth (in contrast, the Earth-Moon distance is typically 384000km and Earth's radius is 6367km), despite expending considerably less fuel than a classic Hohmann transfer would require. [Bollt & Meiss 1995 found that this kind of strategy required 38% less rocket velocity-delta than a comparable Hohmann transfer orbit. That can yield very substantial fuel savings in view of the usual "rocket equation" $\Delta v=v_{exhaust}\ln(M_{fueled}/M_{unfueled})$.] Their strategy is simple: burn your rocket just enough to make the Jacobi constant J reach the critical value (in this case 3.17948); this requires a velocity change of 744.4 meter/sec. For any J near that value we empirically almost always find ourselves in "the chaotic regime," and this chaos can usually be relied upon, after enough time, to explore enough to eventually enter the narrow passage between the Hill surfaces to reach the vicinity of the Moon. Unfortunately, it might instead first cause a collision with the Earth, but their design prevented that by simply discarding thus-failed attempts. It empirically seems easy, just via random trial and error, to find chaotic trajectories of this kind that never go substantially inside the original parking orbit. Once the moon is neared, another rocket burn could be used to re-raise J above the critical value, thus trapping it in orbit round the moon. Or instead, if desired, they then could perform a stronger rocket burn during a "powered lunar swingby" to change J to ≈J(L4) and thus reach the vicinity of either L4 or L5, which then automatically would be approached with relative speed tending to zero, and then we can arrange to stay near L4 or L5 forever. The Earth-Moon L4 and L5 points are KAM-stable equilibria points for a test-mass stationed there; it can be made to librate about either like a harmonic oscillator with periods about 28 and 92 days. This particular trajectory takes 25 years to reach the vicinity of the moon, although the authors point out that by adding a few small rocket-burns intended to "control the chaos" via ad hoc intentional "butterfly effects," they could reduce that wait, e.g. Bollt & Meiss 1995 got a 2.05-year wait time with total Δv=749.6 meter/sec, which Salazar et al reduced to 311 days at the price of more rocket fuel. Even more economical space travel strategies (for those willing to wait long enough) involve taking advantage of effects from N≥3 bodies.

A spacecraft needs roughly the same amount of energy to reach the Sun-Earth and Earth-Moon L1 and L2, a lucky fact that makes the Earth-Moon-Sun system particularly rich for trajectory designers.

(New) algorithm to find *optimal* orbit changes. So far in the literature, the design of "chaotic trajectories" like that intended to consume minimum fuel to accomplish some orbit-transfer goal has been something of a "black art." That is annoying and silly. I now point out there is a simple algorithm to find *optimal* such orbit transfers, where "optimal" can be defined by any desired positive-weighted sum (user-defined "cost") of (a) total rocket Δv and (b) total travel-time. It is, essentially, "dynamic programming." You make a big table of possible orbit-defining parameters. Actually, this is a continuum-infinite set, but approximate that with a large finite sample. Now for each pair of table entries, especially pairs nearby in parameter space, try to find a low-fuel small-time rocket burn to take you from one to the other. These may be found by "shooting method with iterative correction." Once enough such low-cost pairs have been found to form a highly-connected graph, you then apply a "shortest path in graph" algorithm. The resulting trajectory will be unnecessarily "wiggly" due to the finite sampling, but could then be "smoothed out and optimized" further by methods that ought to be pretty obvious. This approach should, in the limit of large samples, produce arbitrarily-close approximations to optimal (i.e. least cost) trajectories.



Instructive error by Edward Belbruno. In his otherwise-enjoyable book *Fly me to the moon* about chaos and spacecraft trajectory design (Princeton Univ. Press 2007), Belbruno committed a serious error, leaving his chapter 14 in smoking ruins. Belbruno considers a spacecraft sitting at the L4 point (of, say, the Earth-Moon or Sun-Jupiter system, for which L4 and L5 are stable) or perhaps at some small distance d from it. He then asks: What is the minimum velocity V(d) the spacecraft needs to acquire (by firing its rockets) to escape? From the stability of L4, Belbruno inferred that V(d) must be *maximized* at d=0, where it is some positive speed, then went on to make further deductions. That all is garbage! In fact, V(d)=0 for *all* small-enough d. I find it best to think about this time-reversed: we start from someplace far away, then fire our rockets to acquire the near-minimal value J=J(L4)+ε for some trajectory intended to get near L4, then ride that trajectory. As the trajectory approaches the nearest possible point to L4 permitted by our J-value, we approach zero speed in the rotating reference frame. This causes the trajectory to have a cusp where it hits (at distance d from L4) the Hill zero-velocity surface surrounding L4. You can see many such cusps in Frnka's picture of a planar tadpole orbit circling L4; this orbit can never penetrate a Hill zero-velocity surface and hence is unable to approach L4 arbitrarily closely, but does keep winding around it. [Planar CR3BP horseshoe orbits can also exhibit cusps, as is illustrated in the figure at left from Burkhard Militzer's 2021 video Horseshoe orbit of hypothetical asteroid shown in a rotating frame where the "Sun" and "Jupiter" remain stationary. (To make the angular story clearer, Militzer also drew line segments connecting the Sun to Jupiter and the asteroid.) These cusps are *needed* to stop Militzer's asteroid from penetrating L4- and L5-containing Hill surfaces, because its J value obeys J(L3)<J<J(L4). However, most real tadpole and horseshoe orbits do *not* have such cusps because they orbit an L4-containing surface, and/or have J<J(L4) so that they can pass directly though L4's location in the XY plane, although in 3D they usually would do that at different Z. These everywhere-smooth orbits have a much simpler character than the cuspy ones; indeed in the limit of small amplitudes they become just 3D harmonic-oscillator trajectories.] Now since Newton's laws are time-reversible, we conclude that **arbitrarily small initial speed suffices** to escape arbitarily far from L4, starting from such a trajectory-cusp, which can lie arbitrarily close to L4. What Belbruno did not understand is: The nature of L4's "stability" is *unlike* the stability of an object in a potential well, such as on Earth's surface. To escape Earth you need to acquire a large velocity (many km/sec), and any vaguely-upward direction will work. (Downward directions would also work – in fact every direction would – if you could magically losslessly burrow through rock like a neutrino.) In contrast, to escape the vicinity of L4, arbitrarily small velocities suffice, *but* your initial velocity needs to be carefully chosen – many choices will just yield a "tadpole orbit" that stays near L4 forever. (Also see Sosnitskii 2008.)

Using Hill surfaces to help prove "stability" of 3-body systems. Suppose the Sun-Earth-Moon system were a circular planar restricted 3-body problem. It isn't, but it comes close since (a) the Earth's orbital eccentricity e≈0.0167 is small, and (b) the Moon:Earth and Earth:Sun mass ratios are approximately 1:81.3 and 1:333000, so modeling the moon's mass as "0" seems plausibly acceptable. (Another real-world problem is that these three bodies are extended, not *point*, masses, hence can exhibit tidal energy losses, whereas Newtonian N-point-mass mathematics conserves energy. Years were 424 days long 600 Myr ago, as opposed to the present 365. That was proven by examination of annual and daily growth features in fossilized corals, see fig.1 of Wells 1963. This continuing slowdown of the Earth's rotation rate is the result of tidal drag from the moon.) In that case, Hill could prove the Moon could **never escape** from a roughly spherical region centered at the Earth, with radius≈0.01 AU≈1.5×10^6 km ("Hill radius"; this number happens to be about 4 times the Earth-moon distance). Also, the **sun can never get near** either the Earth or Moon. This has been called "Hill stability." A rough estimate of the Hill radius adequate for my purposes, is

$$\text{Hill Radius} \approx 3^{-1/3} [1+M_{sun}/M_{earth}]^{-1/3} (1-e)\, D/2$$

where D/2 is the Earth-Sun distance while e with 0≤e<1 is the Earth's orbital eccentricity. Since $M_{sun}/M_{jupiter}\approx1048$ with $1048^{1/3}\approx10.16$, Jupiter has a huge Hill radius≈1/15.4 times the Jupiter-Sun distance.

Innanen 1979's more refined Hill-radii formulae. The maximal "limiting radii of satellite orbits," according to Innanen's EQs 12 and 11, are

$$\text{Prograde Radius} \approx 3^{-1} [1+M_{sun}/M_{earth}]^{-1/3} L \quad \text{and} \quad \text{Retrograde Radius} \approx [1+M_{sun}/M_{earth}]^{-1/3} L$$

assuming the small-mass satellite orbits the Earth prograde and retrograde, respectively (and assuming Earth's orbit is circular).

Hill's zero-velocity-surface technique can also be used to prove that in planar systems resembling Sun-Mercury-Neptune, neither Mercury (regarded as infinitesimal mass) nor the Sun can ever near Neptune.

The defect that Hill's theorem is only applicable in unphysical "circular restricted" cases, i.e. with $M_{moon}=0$, was overcome by **Marchal** & Saari 1975, then Marchal & Bozis 1982. Their confinement theorems can yield Hill-like confinement regions defined by analytic formulas, but now valid for *un*restricted not-necessarily-planar not-necessarily-circular Newtonian 3-body problems. (These again can be regarded as arising from uncrossable surfaces, but those surfaces *move*.) Saari (1984 & 1987) produced "the best possible configurational velocity surfaces." Ge & Leng 1992 produced the same result as Saari 1987 in a different way. In limits where the planar restricted circular case is approached, the Marchal-Bozis-Saari regions approach Hill's – a useful fact. As an example of the fact the Marchal-Bozis provides weaker bounds than Hill because of allowing nonzero lunar mass: Marchal-Bozis is *not* numerically strong enough to prove lunar confinement within any Hill-like sphere (as computed on page 327 of Marchal & Bozis' paper); but if the Sun-Earth-Moon system had somewhat more favorable parameters (in particular, as Hill pointed out, if the moon were massless and stayed in the plane), then it would have been able to. E.g. I think Sun-Pluto-Charon, or certainly Sun-Saturn-Moon for some suitably close moon of Saturn (rest of solar system removed), should work. On p.331: "most triple stellar systems are Hill-stable, sometimes strongly Hill-stable; the close binary cannot be approached by the third star and its major axis has only very small perturbations."

More Jacobi-like constants of the motion. The genius of C.J.G.Jacobi (1804-1851) in discovering his new "constant of the motion" for the circular restricted 3-body problem (beyond the usual ones such as energy, momentum, and angular momentum) was the foundation of Hill's proof. We may ask: are there any further surprising constants of the motion? Bozis 1966 & 1967, and Contopoulos 1965, and Vrcelj 1978/9 discovered another for the restricted 3-body problem, but it remains poorly understood compared to Jacobi's and has no known finite-length formula.

Excluding collisions and therefore also excluding escapes. The Hill (and Marchal et al's more general Hill-like) theorems did not by themselves exclude the possibility that the "Earth" and "Moon" will collide, or that the Earth-Moon binary will escape from the Sun. And Hill stability for a system like Sun-Mercury-Neptune, does not exclude the possibilities that Neptune will escape or a Mercury-Sun collision. (Marchal & Bozis in fact explicitly pointed out the non-exclusion of the collision possibility on page 332, while other authors have warned us about the unexcluded escape possibility, e.g. Barnes & Greenberg 2006.)

However, Barnes & Greenberg conjectured based on their computer experiments that most Hill-stable systems are (more strongly) "Lagrange stable," which in the Sun-Mercury-Neptune case prevents Neptune from ever escaping, and in the Sun-Earth-Moon case prevents the Earth-Moon system from ever escaping, and also eternally prevents Sun-Mercury and Earth-Moon collisions. In fact, they think tightening the Marchal-Bozis criterion by a factor of about 1.1 is enough to ensure Lagrange stability.

Also, *retrograde* orbits empirically cause greater stability and wider Hill/Innanen regions. One intuitive explanation for the greater immunity of retrograde planet-moon systems against escape, is as follows: In order for the Earth-Moon subsystem to acquire enough kinetic energy to escape from the sun, the only possible source of that energy (since Marchal & Bozis assure us neither can ever get near the sun) would be from the Earth and Moon (regarded as point masses) closely approaching each other. That would only be possible, in view of the Marchal-Bozis confinement theorem, by making the minor-axis of the Earth-Moon ellipses get very small, forcing the Earth-Moon subsystem's angular momentum to get very small. If our Earth-Moon system were orbiting the sun *retrograde* (e.g. anticlockwise while the Earth-Moon orbited each other clockwise) then this would force the orbit of the Earth round the sun *also* to lose angular momentum (since the total angular momentum is conserved), which would tend to hinder, not help, escape.

A different stability-enhancing feature of retrograde orbit-pairs is: shorter-duration encounters.

I can *prove* Neptune-escape is impossible in systems resembling Sun-Mercury-Neptune (but with 10^7× lighter Mercury) and I can *prove* Earth-Moon subsystem escape is impossible in systems resembling Sun-Earth-Moon (but with the Moon lightened by some large-enough constant factor), by resorting to Einstein gravity instead of Newton. The problem with Newton is that by bringing two point masses arbitrarily close together, one can garner unboundedly huge energy, then perhaps that energy could power bad phenomena. With Einstein, that cannot happen: the most energy obtainable from a mass M is Mc^2. So with 10^7× lighter Mercury, there simply would not be enough energy available from Mercury to ever expel Neptune; and with 10^7× lighter Moon, there simply would not be enough energy available from the Moon to ever cause the Earth-Moon subsystem to escape from the Sun.

So between Marchal-Bozis/Hill confinement theorems and this kind of "**Einstein cheat**," we can rigorously conclude no-escape Hill stability. However, none of that excludes the possibility of an Earth-Moon (or Sun-Mercury) *merger*. Marchal & Bozis 1982, Milani & Nobili 1983, Barnes & Greenberg 2006, S.P.Sosnitskii in the 2020s, etc, all bemoaned the fact that they were unable to prove the impossibility of that and regarded it as an open problem.

But they all were wrong. It wasn't an open problem. It was an "already solved problem" as we shall see when we discuss "KAM theory." For example, an artificial satellite orbiting the Earth roughly according to Kepler-Newton laws, will be perturbed by the Moon (if that "Earth" had a Moon) or by the Sun, *but* this third-body perturbation (if the Moon is far enough away compared to the satellite's orbit-size) will usually *never* be enough to destabilize the orbit enough to make it ever hit the Earth or escape – and such statements are provable using KAM theory.

Chazy final-behavior classification. P.Painleve proved that 3-body solutions either exist forever, or there is a 2-body or 3-body collision at some time. D.G.Saari proved the (≤3)-body initial conditions leading to a 2-point or 3-point collision, are sets of measure=0. In 1922 J.Chazy proved that when time t→+∞, one of the following possible behaviors of 3-body solutions defined for all future time must occur (described in terms of the three interbody distances A,B,C and with the origin fixed at the center of mass):

1. (+@) All three interbody distances approach ∞, with $\dot{A},\dot{B},\dot{C}$ all approaching positive constants.
2. (+) All three interbody distances approach ∞, with two of $\{\dot{A},\dot{B},\dot{C}\}$ approaching positive constants while the third approaches zero.
3. (@) One body escapes infinitely far from the others with relative speed tending to a positive constant; the other two have separation eventually-bounded between two positive constants.
4. (-) One body escapes but with relative speed approaching zero; the other two have separation eventually-bounded between two positive constants.
5. (0) min(A,B,C)→∞ but with all velocities approaching zero.
6. (-@) All three of {A,B,C} remain within a bounded set.
7. (-) Limsup max(A,B,C)=∞, but all interbody distances return to a bounded set an infinite number of times with liminf max(A,B,C)=0.

I have added **(+), (-), or (0)** to denote cases which can only occur if the total energy has that sign, and "**@**" to denote cases arising from a positive-measure set of initial conditions. It is not known whether type-7 motion can happen from a positive measure set of initial conditions; V.M.Alekseev conjectured its measure=0. Already in Chazy's time the first six types were known to exist. E.g. the Lagrange equilateral-triangle solution but with ellipses, hyperbolas, and parabolas, demonstrates existence for cases 6, 1, and 5. It is easy to see examples for 2,3,4 exist. All of {1,3,6} occur for positive-measure sets of initial conditions. The only difficult case is the Chazy class #7, which is far more mysterious. Examples of that were first produced in 1960 by Sitnikov for the Sitnikov subclass of 3-body problems, and later by Guardia, Martin, and Martinez-Seara for restricted (both circular and elliptic), and finally with Paradela in 2022 for unrestricted *planar* 3-body problems. Due to time-reversibility of Newton's laws, Chazy's classification also works when t→**–**∞, and indeed it has been shown that *every pair* of (past, future) Chazy-behaviors that is not ruled out by total-energy sign incompatibility, can be (simultaneously) achieved; the difficult cases of that pair-claim were shown by Alekseev 1956/1969. Furthermore "exchange reactions" are achievable, such as {PQ,R}→{PR,Q} for binaries plus singletons. As Alekseev discusses, any (past, future) Chazy-type-pair (V,W) with V,W∈{1,3} arises with positive measure for either positive or negative total energy, including both with and without exchange in the (3,3) case. The case (6,6) happens with positive measure (consequence of KAM theory) and only with negative total energy. The "capture" (or its time-reversal "escape") pairs (3,6), (6,3), (3,7), (7,3), (6,7), (7,6) all happen; but always with measure=0 (due to a phase-space-volume argument by Littlewood 1953), and only with negative total energy.

Type-7 motions can occur in Sitnikov 3-body scenarios (Sitnikov 1960, Alekseev 1968/9). Indeed,

SITNIKOV/ALEKSEEV UNIVERSALITY THEOREM: In the Sitnikov 3-body problem (either restricted or not; both work), whenever the z-axis body starts close enough to the plane of the other two bodies with small enough initial speed, then the motions remain eternally bounded. There exists an integer L≥0 such that for *every* possibly-bi-infinite sequence of integers $s_k \ge L$, there exist initial conditions for these Sitnikov problems that cause the successive crossings of the z-axis body across the plane of the other two to occur after they orbit the z-axis s_k times since the preceeding crossing. This theorem also allows coming from and/or escaping to infinity, in which case the s-sequence stops at one or the other end (or both).

This theorem guarantees the existence of an uncountably-infinite number of topologically-distinct Sitnikov motions, and the vast majority of (i.e. nearly all of) those topological specifications (i.e. s-sequences) force Chazy type 7 in both the past and future, and force the motion to be eternally chaotic. (Only a mere *countable* infinity of the topological types arise from periodic motions.) Nevertheless I would expect Chazy type 7 motions to arise from a *zero measure* subset of Sitnikov initial conditions because *escaping* motions ought to have full measure.

Furthermore, almost all the topological specifications with $L \le s_k \le U$ for all k (where L<U; these correspond to eternally *bounded* Sitnikov motions) arise from necessarily eternally-chaotic motions; and I conjecture the set-union over *all* U with U≥L of Sitnikov initial conditions which make such an s-sequence happen, has **full Hausdorff dimension** but zero measure. For any particular *finite* U, though, I conjecture positive Hausdorff codimension.

Extensions: Chazy-like statements for 4 (and sometimes more!) bodies. Fleischer & Knauf 2019 proved (for each fixed d≥2 and N≥2) that the subset of initial conditions for N-point-body problems in d-dimensions leading to a *collision* had relative Liouville measure *zero*.

Knauf 2018 proved that with N=4 bodies in d≥3 dimensions, when t→∞,a full measure set of initial conditions yield limiting values of the "Cesaro mean" of velocities. [This indeed works for a class of pair potentials that include all the homogeneous ones of degree âˆ’A for A∈(0,2).]

Gingold & Solomon 2017 established the existence of an open set of initial conditions through which pass solutions without singularities to Newton's gravitational equations in $\mathbb{R}^3$ on a semi-infinite interval in forward time t, for which every pair of particles separates like At, A>0, when t→∞ These solutions are constructable as series with rapid uniform convergence, their asymptotic behavior to any order is prescribed, and this family of solutions depends on 6N parameters subject to certain constraints.

Saari 1977 already claimed to show that for N≤4 bodies in d-space (any d≥2) the set of initial data giving rise to singularities has measure=zero and also is a "meager" set. (All also true if restrict to systems with center of mass=(0,0,0), fixed-energy systems, and/or fixed angular-momentum systems.) P.Painleve 1897 had shown that any singularity must cause the distance between some two bodies to shrink to 0; and if N=3 that all singularities are collisions. [Gingold & Solomon 2017: "to the best of our knowledge there is no equivalent result for N≥5."]

Saari in the 1970s had also shown that the set of N-body initial data yielding a collision singularity has measure=0 and is meager (for any N).

Marchal & Saari 1976 showed that the (N≥4)-body problem as t→∞, there is a dichotomy: either asymptotic velocities in the Cesaro mean sense do not exist and system diameter is not boundable by any linearly-growing fincton of t; or the system decouples into several subsystems moving apart linearly with each subsystem growing at most like $O(t^{2/3})$.

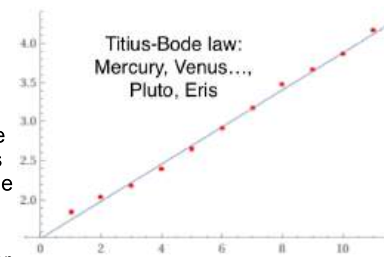
Corollary of the above results: For N≤4 bodies in d-space (any d≥2): the sets of initial conditions leading to either collision or *non*collision singularities have measure zero.

MOECKEL'S UNIVERSALITY THEOREM (Moeckel 2007): Every "symbolic dynamics" describable as a path in a certain finite graph is achievable by three positive masses in the Newtonian 3-body problem.

Note that only type 6 and 7 Chazy behaviors could hope to be chaotic for an infinitely long time. And in fact, Euler's unstable collinear periodic planar 3-body solutions prove at least one eternally-chaotic solution exists in case 6; indeed an infinite number of topologically-distinct linearly-unstable periodic 3-body orbits exist; and an *uncountably*-infinite number of topologically-distinct linearly-unstable bounded 3-body trajectories exist. And the Guardia-Martin-Paradela-Seara 2022 proof's constructed Chazy-type-7 motion for 3 bodies is chaotic.

Open question: For some N≥3: Can eternally-bounded chaos can happen from a positive-measure set of N-body initial conditions? (I conjecture "yes.") A positive-measure set of initial conditions exists producing *non-chaotic* eternally-bounded 3-body motions – this indeed is true (as a consequence of KAM theory) just within a neighborhood of just any one periodic KAM-stable solution.

2.7. Titius-Bode "law" and attempt to "derive" it via scaling theorem.



J.D.Titius in 1766 and J.E.Bode in 1772 noticed the sun-planet distances of the planets in our solar system approximately lie on a *geometric progression* – *except* one member was missing. The discoveries of Uranus in 1781, the first and largest asteroid Ceres in 1801 (filling in the "missing" planet!), and Neptune (1846), Pluto (1930), and Eris (2005) all seemed to confirm Titius-Bode. Since certainly solar systems that disobey this "law" are theoretically possible – and have occasionally been observed – I would prefer to call it a "**tendency**" rather than "law." Unfortunately for Ttius & Bode, according to current IAU definitions, Ceres and Pluto are "dwarf planets," not "real planets." This decison by the IAU is peculiar because (a) Pluto has five moons – and (b), peculiarly, if the genuine-planet Earth magically suddenly replaced either Ceres or Pluto, then Earth magically would get demoted to non-planet status! So for the present purpose I will use my own **non-IAU definition of "planet":** "Anything heavier than 5×10^{22}kg that orbits the Sun." Under my definition, Pluto and Eris count as planets. Ceres would not, but I nevertheless call it a planet because it *represents* the entire asteroid belt. The asteroids are known from "geological" evidence to have once been part of one or more small planets (if only one, then it would have been about 1200 km in diameter to account for present-day total asteroid mass 2.4×10^{21} kg) which broke up at least 3 Gyr ago, either due to collision or tidal disruption from a close flyby with Jupiter. It is plausible that a large fraction of asteroids were expelled from the solar system or ended up inside Jupiter, in which case the protoplanet(s) would have needed to be larger, e.g. increase 1200→5275 km diameter if 99% of asteroid mass got expelled/eaten (in comparison, Mercury's and Pluto's diameters are 4878 and 2377 km). I have redone Titius & Bode's calculation with modern data by plotting **Y**=$\log_{10}$(maximum sun-planet distance in 10^6 km) versus **X**=planet number 1,2,3,...,11, then drawing the least-squares line-fit. which is Y=0.2368X+1.52172. (Note: **maximum** sun-planet distance yields a better line-fit than either *mean* or *minimum* distance, or planet orbital *periods*, although usually not statistically-significantly better.) So for some reason each planet likes to have maximum sun-distance about 1.725 times the preceding planet's. The minimum such ratio is Pluto/Neptune≈1.618 and the maximum Uranus/Saturn≈1.992. Some dismissed this as mere meaningless luck. They're wrong: Bovaird & Lineweaver 2013 examined 68 exoplanet systems containing ≥4 planets. They found that "a majority" of those systems obeyed a generalized Titius-Bode relation "to a greater extent than our Solar System," while 96% obeyed it to a comparable-or-greater extent.

Even 260 years after Titius, the theoretical explanation for his empirical law remains elusive. I will point out, however, that among over a million known asteroids, essentially zero have orbital periods in (⅔, 1) times Jupiter's period (the "forbidden zone" between the "Hildas" at ⅔ and the "Trojans" at 1); and aside from the Hildas, there are virtually none in (0.57, 1) and few in (½, 1). This suggests that each planet *must*, in order to form and survive without expulsion, have orbit-ellipse diameter at least $(3/2)^{2/3}\approx1.3104$ times the preceding; and if we refused to permit planet-pairs in exact 3:2 resonances because that would require "too much luck," then at least 1.4489 times; and probably $\geq2^{2/3}\approx1.5874$. Also, I mention the **s-scaling theorem** for the Newtonian N-body problem:

> If $\vec{X}(t)$ is a solution, then so is $\vec{X}_{scaled}(t)=s^{-2/3}\vec{X}(st)$ for any s>0. Furthermore, the total energy E obeys $E_{scaled}=s^{2/3}E$, velocities as $s^{1/3}$, while the total angular momentum L obeys $L_{scaled}=s^{-1/3}L$ causing "**Dziobek's constant**" L^2E to remain invariant under scaling.

This theorem suggests that, if we approximate the whole solar system as a succession of {sun,$planet_K$,$planet_{K+1}$) three-body systems, then if one particular planet-orbit size-ratio (e.g. 1.725) were somehow preferred for one such system, then all the others would prefer it too, yielding (approximately) a geometric progression.

2.8 "Commensurable" finite sets of real numbers. Let $\vec{w}$ be a vector of d≥2 real numbers. If there exists an *integer* vector $\vec{k}\neq\vec{0}$ such that $\vec{k}\cdot\vec{w}=0$, then we say that $\vec{w}$ (and in particular the subset of the entries of $\vec{w}$ corresponding to the nonzero entries of $\vec{k}$) are "commensurable" reals. If $\vec{k}\cdot\vec{w}\neq 0$ for every integer-vector $\vec{k}\neq\vec{0}$ then the entries of $\vec{w}$ are "incommensurable." The latter generically happens – i.e. if the entries of $\vec{w}$ are independent samples from any probability density, then Prob($\vec{w}$ is incommensurable)=1. If some positive constants c and v exist such that $|\vec{k}\cdot\vec{w}|>|\vec{k}|^{-v}c$ for *all* $\vec{k}\neq\vec{0}$, then the entries of $\vec{w}$ are "*strongly* incommensurable." That also generically happens. Finally, if strong incommensurability holds for every v>d-1, then call the entries of $\vec{w}$ "*optimally* strongly incommensurable." The subset of reals commensurable with 1 is precisely the rational numbers – a countable (albeit dense) set, therefore with zero measure. The subset of reals incommensurable with 1 is precisely the irrational numbers – also a dense set, but now with full measure. The subsets of reals strongly incommensurable and optimally strongly incommensurable with 1 are full-measure everywhere-dense sets, but topologically are "meager," and their complement, despite having zero measure, is everywhere-dense and has the same cardinality as the reals. (A real subset is called "meager" if it is a countable union of nowhere-dense subsets.)

Although the above incommensurability notions have been adequate for most previous authors, let me now go further by stating some theorems that allow *infinite*-dimensional vectors $\vec{w}$.

THEOREM: Any bounded infinite set of reals is always commensurable.

Proof. If a finite subset of them is commensurable, we are done, so assume not. Assume wlog the first real equals 1 and all the others are in (0,1). Then among the other reals, any N of them must have the property that there exists some linear combination of them, using coefficients in {0,-1,+1} only, with absolute value greater than 0 but less than $N/(2^N-1)$. [Because: There are 2^N subsets of them, each of whose sums lie in (0,N), and hence by the pigeonhole principle at least two such subset-sums must differ by less than $N/(2^N-1)$. Subtract one subset sum from the other to get a suitable linear combination.] Now simply apply an "iterative correction algorithm." That is, begin with sum S=1. Now for K=2,4,8,16,...: find a linear combination among the yet-unused reals less than 1/K, and subtract off the right integer multiple of it from S to make |S| less than 1/K. The limit-result of this iterative correction process is a nonzero integer linear combination of a countable subset of your reals, which equals 0. **Q.E.D.**

THEOREM: With probability=1, a countably-infinite set of random-uniform reals in (0,1) has no commensurable finite subset.

Proof. For a contradiction, suppose not. Let the cardinality of the finite subset be N. Since the integer N-tuples are countable (a fact we now shall use *twice*), we can enumerate all possible integer linear combinations of all possible N-element subsets of our reals, one by one: their sums therefore form a countable real subset. Since countable sets have measure=0, the probability than any of those sums equals 0 (or indeed, that any of them assume any value in any pre-specified measure=0 set), is zero. We can do all that for *every* N=1,2,3,... not just one; but since the union of a countable number of measure-0 sets has measure=0, that cannot change the conclusion. **Q.E.D.**

And indeed, essentially the same proof shows the **stronger theorem** that, with probability=1 every finite subset will be *strongly* incommensurable.

2.9 Survival times before collision – Crude model. If two "planets" were orbiting a "sun" (say, one clockwise and the other anticlockwise) in the same plane, with typical sun-planet separations of order L, and typical orbital periods of order P, then if their "collision cross section" were twice the sum R_1+R_2 of their radii (we assume $L\gg R_1+R_2$; if the "planets" actually are black holes, use *Schwarzschild* radii), then we might crudely expect the typical time between planet-collisions to be of order $(R_1+R_2)^{-1}PL/4$. If, on the other hand, the two planet orbits were *not* in the same plane, they were in 3-space, then this crude expected before-collision wait instead would be of order $(R_1+R_2)^{-2}PL^2/2$, which will be **longer** than in the 2D case for all large-enough $L/(R_1+R_2)$. Both formulas would follow under the approximate model that each candidate-collision was a probabilistic event. (That **probabilistic model** presumably would work well in 3D scenarios with "chaotic" 3-body dynamics.)

Survival time: comparison of numerical examples with observations: Using P=11.86 years=orbital period of **Jupiter**, R_1+R_2=69946km (average radius of Jupiter), and L=6.717×10^8km (typical Jupiter-sun distance), we find $(R_1+R_2)^{-1}PL/4\approx 28500$ years and $(R_1+R_2)^{-2}PL^2/2\approx 500$ Myr. The smallness of these timespans compared to the age 4.5 Gyr of the solar system explains why no asteroids are found in the "forbidden zone" with periods in (0.7,1) times Jupiter's. Three-body simulation experiments by Lecar & Franklin 1973 indicate that most asteroids with periods in (2/3,1) times Jupiter's get "ejected" within 2400 years (≈200 Jupiter orbits) except for a few near 3/4; and 85% of asteroids between Jupiter and Saturn got ejected within 6000 years in 4-body (Sun+Jupiter=Asteroid+Saturn) simulations. These confirm my crude 2D time-estimate 28500 to within about one order of magnitude. But you might worry about the discrepancy that the true survival times are lower than my crude 2D estimate by a factor of 4-11. Looking at L&F's paper more carefully, we see on their p.423 that their word "ejected" does not necessarily mean what you think it does. If asteroids ever "approach Jupiter closer than 10 present Jovian radii," L&F counted that as an "ejection" and terminated their simulation. That factor 10 explains the discrepancy; our estimate actually was quite accurate.

If we instead use P=1 year=orbital period of Earth, R_1+R_2=6371km (average radius of Earth), and L=1 AU=149597870.7km we find $(R_1+R_2)^{-1}PL/4\approx 6000$ years and $(R_1+R_2)^{-2}PL^2/2\approx 280$ Myr. There are zero presently-known asteroids which (a) are considered possible **Earth**-colliders and (b) have diameter>10km. The largest known example has diameter 5-7km and orbital period≈9/2 years.

That same-plane survival-time formula also would follow under the *different* (non-probabilistic) approximation that both planets were much lighter than the sun, so that they do not gravitationally affect each other, hence each follow exactly-elliptical orbits – where we assume these ellipses have bounded aspect ratios, intersect, and that the **two planet-periods have *irrational* ratio ζ with the RCF of ζ having all partial quotients bounded**, and finally assume (so that probabilistic "expectation value" has meaning) random initial locations for each planet along its ellipse. (Of course, if there were only *one* planet, or two but their ellipses did *not* intersect, then no collision would ever happen; and if the period-ratio were *rational* then generically no collisions would ever happen since the whole system would be periodic.) But if they do intersect, with irrational period ratio, then the two planet-centers are *guaranteed* eventually to get arbitrarily close, whereupon the approximation that they do not gravitationally affect one another and do not collide, will break down. E.g. if the sun has mass=1 and the two planets mass ε each, then the planetary-non-interaction assumption breaks down whenever the distance between the planets gets smaller than O(εL) – causing the planet-planet gravitational potential energy to be of the same order as the sun-planet energy – a criterion which happens to coincide (to within a factor proportional to L) with the "black hole merger" condition that their Schwarzchild-radii balls intersect. Our crude model shall be: exact ellipse 2-body planet orbits, until collision. Then the longest lifetimes arise from irrationals with all partial quotients in their regular continued fraction (RCF) expansions *bounded*. This set of irrationals is a subset of "the reals optimally strongly incommensurable with 1" and is a zero-measure subset of the real line having the same uncountably-infinite cardinality as the real line. A countably-infinite subset of that subset, is the *quadratic irrationals*. By a theorem of L.Euler & J.L.Lagrange, the quadratic irrationals are precisely the irrationals with ultimately-*periodic* continued fraction expansions. (See books by Khinchin, Olds, Rockett etc to learn about number-theoretic continued fractions.)

Examples: the "golden ratio" φ=(1+√5)/2≈1.60803, whose regular continued fraction is [1;1,1,1,1,...], and √2≈1.41421 with regular continued fraction

[1;2,2,2,2,...], and √3≈1.73205 with regular continued fraction [1;1,2,1,2,1,2,1,2,...].

However, ***generic*** **irrational numbers ζ also work provided we shrink the time-to-collision estimate by a logarithmic extra-safety factor**. To explain that: The Gauss-Kuzmin-Wirsing theory of RCF partial quotients for generic real numbers, shows that generic real numbers have the following "probability distribution" for the partial quotients Q in their RCF:

$$\mathrm{Prob}(Q) = -\log_2\left(1 - [Q+1]^{-2} \right) \quad \text{for } Q\ge 1. \quad [\text{If } Q<1 \text{ then Prob}(Q)=0.]$$

Eduard Wirsing showed, for random reals X sampled from any reasonable density, the L^2 distance between this formula and the true probability distribution of the N^{th} partial quotient is upper-bounded by 0.3037^N for all large-enough N. Consequently, the maximum among the first N partial quotients in a random real's RCF, will be O(N) with probability=1, and the number N of its partial quotients required to determine a random real X accurate to D decimal places, with probabilility=1 obeys both

$$\lim N/D = 6\pi^{-2}\ln(2)\ln(10) \approx 0.97027 \quad \text{(Loch's theorem)},$$

i.e. for almost all reals there is almost exactly 1 partial quotient per decimal digit. It then follows from the theory of RCFs and "best rational approximations" that the closest rational approximation p/q, among those with 1≤q≤Q, to a real ζ obeys

$$(1):\quad Q^{-2}(\ln Q)^{-1} = O\left(\min_{p,q \text{ such that } 1\le q\le Q} |\zeta-p/q| \right) \quad \text{and} \quad (2):\quad \min_{p,q \text{ such that } 1\le q\le Q} |\zeta-p/q| < 5^{-1/2}\, Q^{-2}.$$

where:

1. The set of ζ for which the left-hand inequality is false, is a real-subset with measure=0
2. The right-hand inequality is, for any ζ, valid for an infinite set of Q (and for any ζ in a full-measure subset of ℝ, this sequence of Q's grows in a manner upper bounded by an exponentially-growing function).

The upper bound arises from an 1891 theorem of A.Hurwitz that among any three consecutive among ζ's RCF convergents, at least one must be a rational p/q obeying $|\zeta-p/q| < 5^{-1/2} q^{-2}$. The fact that Hurwitz's constant $5^{-1/2}$ is least possible is shown by ζ=(1+√5)/2. It also is known that **algebraic** irrational numbers all are optimally-strongly incommensurable with 1 (Thue-Siegel-Roth theorem).

To return from recounting the theory of Diophantine approximation to our 2D two-planet crude collision-time estimate: my point is that with random-real period ratio, with probability→1 as the constant hidden inside the "O" gets large, the **2D crude collision time estimate should be valid if weakened by a logarithmic factor as follows:**

$$\text{Survival Time} \ge PL(R_1+R_2)^{-1} / O(\log(L/(R_1+R_2))).$$

Dismissing possible alternative crude survival-time estimates. Instead of making the planet-planet gravitational potential *energy* the same order as the sun-planet energy (planet/sun mass ratio ε), one could ask that the *forces* have the same order. In that case the survival times would shrink by multiplication by a factor $\varepsilon^{1/2}$. So: Why energy and not force? And: why not some third alternative?

The rationale for the energy-based approach is that it causes velocity changes comparable to the original orbital velocity, clearly destroying the original orbital parameters. The energy condition is equivalent to the scattering angle in planet-planet scattering being non-negligible. (If the planet-planet flyby separation grows by a factor X, then the scattering angle shrinks by a factor of order X^{-2}.) The models above need (for their validity) for the *cumulative* effects of a large number of tiny scatterings, to tend to cancel out. That indeed happens in both first-order, and Poisson argued also second-order, 3-body perturbation theory.

2.10 "KAM theory" was developed between 1954 and 1963 by these three mathematicians: Andrey Kolmogorov (1903-1987), his student Vladimir I. Arnold (1937-2010), and Jürgen Moser (1928-1999). The contribution of Nikolai N. Nekhoroshev (1946-2008), a former student of Arnold at Moscow State University, occurred in 1971. Percival 1979 found a variational principle for KAM "invariant tori of fixed frequency." Further important contributions (as far as celestial mechanical KAM is concerned) were made by Michael Herman & Jacques Fejoz and Luigi Chierchia & Gabriella Pinzari during 1998-2015; the lattermost found a KAM theorem for rotation-invariant systems. All that concerned finite-dimensional KAM theory. It is sometimes also possible to apply KAM theory to infinite-dimensional Hamiltonian systems, but that topic still seems under development. For reviews of KAM theory's nature and accomplishments, see Moser 1978, §6.3 (pp.273-313) of Arnold-Kozlov-Neishtadt 2006, §15.4 of Knauf 2011/2018, Chierchia 2012, and Fejoz 2012. There also is some KAM discussion in the book by Montgomery 2025. (Avoid the inadequate and wrong discussion of KAM in ch.11 of Goldstein-Poole-Safko.)

Given: a finite-dimensional Hamiltonian system, and either one, or a continuous analytically-parameterized-family of, periodic orbits, that is/are solutions of that system. (Actually, more general classes than "Hamiltonian" systems can be allowed, but we shall not discuss them.)
Suppose: those orbits are known all to lack exponentially-growing infinitesimal perturbations.
Then: Here are the two main results that KAM theory gives you:

1. KAM theory proves that a full-measure subset of the orbits in the continuum family are "KAM stable." It also can often be used (which usually is harder) to prove that some particular one orbit is "KAM stable," or to partly or completely classify all orbits in the family as KAM stable/not. It is best to do KAM theory with the Hamiltonian system re-expressed in "action-angle variables." For the rest of my KAM discussion, assume that was done. More precisely, in the linearized stability analysis, if any eigenvalues λ have |λ|>1 then we have instability (exponentially-growing infinitesimal perturbations exist). Otherwise, all λ are of the form λ=exp(iθ) for various "twist angles" θ per period. If the set of twist angles are strongly incommensurable (even if, to "play it safe" we include 2π as an honorary "twist angle") – which they generically are, then bingo – that implies KAM stability. If not ("resonance" or "near resonance"), then we may or may not enjoy stability, depending on the severity of your particular resonances; and that can be difficult to decide. The most obvious source of difficulty: nobody knows how to prove irrationality for the vast majority of irrational reals.
   Nonrigorous empirical observation: In most celestial mechanics applications, most resonances are not damaging; there usually are only a small finite subset of resonances severe enough to destroy stability. An intuitive reason why: all resonant $\vec{\theta}$ are infinitesimally near an infinite number of *non*resonant $\vec{\theta}$ all yielding KAM-stability. This exerts a strong protective effect.
   For a Hamiltonian system with ≤2 motional degrees of freedom, KAM stability automatically implies full stability. "Full stability" of a periodic orbit means that an ε>0 exists such that all perturbations with norm<ε of the orbit's initial conditions, stay bounded forever, and that bound approaches 0 when ε does. If the Hamiltonian system has d≥3 degrees of motional freedom, then KAM stability does not necessarily imply full stability. However, it still suffices to show that the fractional **measure** of perturbations<ε which do not remain bounded forever (and with a bound that approaches 0 when ε does), approaches 0

when ε does; indeed FractionalMeasure=$O(\varepsilon^{1/2})$. [And there are conjectures that "1/2" can be improved to "0.999," i.e. arbitrarily near, but below, 1.]

2. Instead of perturbing the *initial conditions* of our orbit by ε, we can consider perturbations of the *Hamiltonian* $H\to H+\varepsilon\tilde{H}$ for some analytically-smooth $\tilde{H}$ which is bounded everywhere within some neighborhood of the trajectory in the unperturbed system. If that trajectory and Hamiltonian is KAM stable, then KAM theory tells us that the trajectories in the altered Hamiltonian system, but arising from our original trajectory's initial conditions, stay forever near our original orbit if dimensionality≤2. If dimensionality≥3, then the fractional **measure** of perturbations<ε which do not remain bounded forever (and with a bound that approaches 0 when ε does), approaches 0 when ε does; indeed FractionalMeasure=$O(\varepsilon^{1/2})$.

   Furthermore, KAM theory implies "Nekhoroshev stability." That means that positive constants A,B,C,D,E exist (0≤A≤1), such that *all* the perturbations within norm<ε of the orbit's initial conditions, stay smaller than $E\varepsilon^{A}$, throughout a huge amount (far longer than any power law) of time≥$D\exp(C\varepsilon^{-B})$. By **d-Nekhoroshev stability** I shall mean for a real-analytic Hamiltonian system with **d** motional degrees of freedom whose unperturbed form obeys the "steepest" form of Nekhoroshev "steepness" conditions, namely "convexity" and "quasiconvexity" (i.e. level surfaces are convex), in the terminology of Pöschel 1993 and Lochak & Neishtadt 1992. For example, the Hamiltonian of the solar system with inter-planet interactions switched off – or of any N-body system regardable as a set of 2-body bound systems when a certain set of small interactions (regarded as small perturbations) are switched off – is a quasiconvex function of the planet-orbit ellipse-diameters (albeit just barely!). For these we may use $A=B=(2d)^{-1}$; and Pöschel says A can be improved to A=1/2 in some circumstances.

Any planar never-colliding periodic 3-body solution that is KAM-stable automatically is fully stable, i.e. small-enough perturbations will stay small forever. That is because KAM stability assures full stability in Hamiltonian systems with ≤2 degrees of freedom. Planar 3-body solutions have 6 degrees of positional freedom, but if we demand that (even after perturbation) the center of mass stay fixed at (0,0), and the total energy and angular momentum both remain fixed, then only 2 degrees of freedom remain.

The "KAM heuristic." I have not heard the name "KAM heuristic" before, in which case I am coining that terminology now. It is: The vast majority of linearly-stable 3-body periodic solutions *are* KAM stable, because of the heuristic that a "random" periodic N-body solution that lacks exponentially growing infinitesimal perturbations ("linearly stable"), ought to enjoy both KAM and Nekhoroshev stability with probability=1. Hence for a parameterized *family* of periodic N-body motions (parameters continuously variable within some real intervals with nonempty interiors) lacking exponentially growing infinitesimal perturbations, all but a zero-measure subset of the parameter-tuples should yield both KAM and Nekhoroshev stability.

"**Lagrange stability**" for an N-body solution denotes the claim that all pair separations permanently are both upper- *and* lower-bounded by positive constants.

Disproof of Poincare's 1892 conjecture. A related conjecture, dating back to H.Poincare in 1892, is as follows. To quote Poincare [translated French→English and with some clarifications added]:

> "There is zero probability for the initial conditions of the motion to be precisely those corresponding to a Poincare-periodic solution... but **Conjecture:** Given... *any* particular solution of [the Newton N-body] equations [with negative total energy], there always exists a Poincare-periodic solution (whose period, admittedly, might be very long), such that the difference between the two solutions is as small as we wish, during a timespan as long as we wish."

Poincare's conjecture, no matter how many times people repeat it, clearly is **false**. A cheap counterexample is the Sun-Jupiter system (ignore the rest of the solar system) plus one interstellar asteroid that never comes anywhere near the Sun or Jupiter. Because of the small mass of the asteroid, its positive energy (since it, say, is traveling with a speed 10× solar escape velocity) is not enough to outweigh the huge negative energy of the Sun-Jupiter system. You might still hope Poincare could be true if we restrict attention to *permanently bound* N-body solutions. But the {Sun, Jupiter, 522 Helga} system and the other 4 such systems examined in Tsiganis, Varvoglis, Hadjidemetriou 2002, look like they provide counterexamples, although as I said I do *not* agree that TV&H *proved* their nonexistence claims, and those authors were blissfully unaware that they had "disproven" Poincare's conjecture. Nevertheless, theirs at least seem good candidates to be Poincare counterexamples.

Applications of KAM and Nekhoroshev theory to the N-body problem.

KAM theory works best for ODE systems with d≤2 degrees of freedom, such as the circular planar restricted 3-body problem (**CPR3BP**).

In KAM stability analyses of scenarios without exponential growth of infinitesimal perturbations, it is commonplace, in fact usual, to prove stability for all but a measure-zero, and indeed often merely countable, set of exceptional parameter-tuples corresponding to "possible resonances." Then with more effort, one can try to examine all those possible unstable-via-resonance scenarios, trying to prove them genuinely unstable or show stability. I should warn you that the computations involved in proofs of KAM stability are almost always very long and painful, especially "higher order" analyses of the kind often required to settle the final few recalcitrant cases, i.e. beyond the ability of un-aided humans – you need aid from computer symbolic manipulation. That is why settling the KAM stability question for the Lagrange triangle solutions of the circular planar restricted 3-body problem – the *simplest* nontrivial N-body problem! – required *five* papers over a span of *twenty-five* years 1962-1986, a count that does not include the prior papers by Kolmogorov, Arnold, Moser, and Pinzari developing the underlying theory. It also empirically seems to mean, in practice, that papers that solve KAM stability problems are effectively unrefereeable, unpublishable, and/or undigestible.

Routhian stability, i.e. the lack of exponentially growing infinitesimal perturbations, arises from solving a certain eigenproblem and showing all eigenvalues λ obey |λ|≤1. The |product| of the eigenvalues always equals 1 because of Liouville's theorem that Hamiltonian system time-evolutions preserve volume in momentum-position "phase space." Therefore, once Routhian stability is established, all λ necessarily obey |λ|=1, i.e. the λ all are complex numbers of the form exp(iθ) for some "twist angles" θ. If some two twist angles (where again, to "play it safe," I am adjoining 2π as an honorary additional "twist angle") have *rational* ratio, then we have a "potential resonance" situation which might cause KAM stability to fail. But rational numbers are a countable set. Also, irrational numbers that are approximated better than any fixed power<-2 of q by an infinite sequence of rationals p/q, form a zero-measure set. Hence these potential resonances have "measure zero," which is the rationale for the heuristics I mentioned before that KAM stability almost always ought to happen once Routhian stability is known.

In KAM-stable ODE systems with *two* degrees of freedom, slightly-perturbed time evolution trajectories are "trapped" within "invariant tori" – assuring in KAM-stable cases that they cannot ever deviate more than a bounded amount from the original unperturbed trajectory.

Thanks to the fact that CPR3BP has only *two* degrees of freedom, KAM stability proves that all small-enough Lagrange triangle librations, for each $M<M_{Routh}$ with $M\notin\{M_{DD1}, M_{DD2}\}$, will remain bounded and small *eternally*. Unfortunately, real world masses always *exceed* zero, so restricted 3-body problems are not physical.

Therefore: what if we ask the **same question, but for librations of *un*restricted Lagrange-triangle solutions** (which *are* physical)? First of all, the set of Lagrange-triangle solutions without any exponentially growing infinitesimal perturbations is a 2-parameter family delineated by the Gascheau-Routh-Roberts criterion. A full-measure subset of that family necessarily consists entirely of KAM-stable 3-body solutions. Every planar 3-body solution may be regarded as a 2-degree-of-freedom system provided we demand that its center of mass remain fixed at (0,0), and that its total energy and angular momentum remain fixed at specified values (the energy being negative for our bound systems). Therefore by 2D KAM theory,

THEOREM [Full stability of a full-measure subset of Gascheau-Routh-Roberts-stable planar 3-body Lagrange triangle solutions]. All KAM-stable 3-body Lagrange triangle solutions are *fully stable*, i.e, if perturbed by any small-enough planar perturbation, then the perturbation will remain small and bounded *eternally*. The KAM-stable ones are a full-measure subset of those satisfying the Gascheau-Routh-Roberts non-instability inequalities. (See also Meyer & Schmidt 2000.)

REMARK: Xiang Yu in a 2019-2022 paper that, apparently, nobody besides me ever checked, claimed to prove that *all* Lagrange-triangle 3-body rigid-rotator solutions obeying the Gascheau-Routh inequality are KAM-stable, *except* for a list of 6 specific possibly-exceptional values v with $0<v\leq 1/27$ of $v=(A+B+C)^{-2}(AB+BC+AC)$. I emailed Xiang Yu arguments that his paper contained errors. He ignored me. Therefore I am unwilling to accept his paper's claims. However, if and when something resembling his claims can be proven, that would be good because it would completely settle the question of which Lagrange-triangle solutions are KAM-stable – as opposed to my current "non-constructive" stance which is that "all Gascheau-Routh-Robert-acceptable ones are KAM-stable except for some *unspecified* zero-measure set of exceptions."

Xiang Yu in a different (2020) paper that again, apparently, nobody else ever checked, claimed to prove that an infinite set of distinct exactly-periodic orbits exist in any neighborhood of Lagrange triangle initial-data.

THEOREM [KAM and (2N-1)-Nekhoroshev stability of a full-measure subset of Roberts-stable "regular N-gon plus sun" planar (N+1)-body solutions]. The "regular N-gon plus sun" rigidly rotating symmetric (N+1)-body configurations whose linear stability was analysed by Roberts 2000 and Vanderbei & Kolemen 2007, are KAM and (2N-1)-Nekhoroshev stable against planar perturbations for a full-measure subset of the linearly-stable cases. [Roberts stability requires N=1 or N≥7, and that in the latter cases the sun to planet mass ratio M/m be sufficiently large, namely M/m>F(N) for a certain known function F(N) with $7^{-3}F(7)=0.4078\leq N^{-3}F(N)\leq 0.4350366$ which for large N is asymptotic to $7\zeta(3)(13+\sqrt{160})N^3/(16\pi^3)\approx 0.43503658N^3$.

THEOREM [Full, KAM, and 3-Nekhoroshev stabilities of full-measure subsets of certain linearly-stable "BHHH orbit" 3-body Poincare-periodic planar solutions. The BHHH 3-body Poincare-periodic planar (and also 3D) solutions discussed in the BHHH appendix fall into 1-parameter analytically-smooth families if the three mass values are held fixed, and also one can vary the masses yielding at least one more variable parameter. Papers cited in that Appendix claim in the planar case that finite-length arcs inside these families exist such that no member of the arc has any exponentially-growing infinitesimal planar perturbation. A full-measure subset of such linearly-stable BHHH orbits are KAM and 3-Nekhoroshev stable. Furthermore, if we restrict attention to the planar ones and if we demand the perturbations preserve total energy and algular momentum, then we enjoy 2D KAM stability, and hence *full* stability.

THEOREM [Full, KAM, and Nekhoroshev stabilities of full-measure subsets of certain linearly-stable "Moore8" type 3-body Poincare-periodic solutions]. The "Moore8" planar choreographic periodic 3-body solution with zero total angular moment for three equal masses, and three rotating variants (some 3D) with 1 parameter governing rotation rate, are discussed in the Moore8 appendix, and the papers it cites show analytic-smoothness and the existence of arcs in parameter space, all along which we enjoy linear stability. A full-measure subset of the linearly-stable orbits in these families are KAM stable; for the planar case ("rotating Eights") we enjoy 3-Nekhoroshev stability and if we demand the perturbations preserve total energy and angular momentum, then we enjoy 2D KAM stability, and hence *full* stability. For the 3D cases we enjoy 5-Nekhoroshev stability and if we demand the perturbations preserve total energy and angular momentum, then 3-Nekhoroshev stability.

THEOREM [KAM and 4-Nekhoroshev stabilities of full-measure subsets of certain linearly-stable "Ouyang-Xie pentagram" type 4-body periodic solutions]. The "Ouyang-Xie" planar choreographic periodic 4-body solution for four equal masses, was found by Ouyang & Xie 2015 and shown by them to enjoy linear stability; then in 2018 they showed by varying the masses that it was part of a continuum family of planar 4-body solutions, all of which enjoyed linear stability. A full-measure subset of the linearly-stable orbits in this family are KAM stable; and if we demand the perturbations preserve total energy and angular momentum, then we enjoy 4-Nekhoroshev stability.

KAM theory proves

THEOREM (Well-behaved 3-body and parallelogram 4-body solutions). A positive-Liouville-measure set of never-escaping, never-colliding, never-too-close-approaching 3-body solutions exist (i.e. with all inter-body distances both lower and upper bounded by positive constants forever), under pure Newton laws, without need for my "Einstein cheat." Indeed, at least a positive-constant-measure fraction of initial conditions within ε of any KAM-stable periodic or Poincare-periodic planar 3-body solution X, for any sufficiently small (X-dependent) $\varepsilon>0$, will work. (And that measure fraction approaches 1 when $\varepsilon\to 0+$.) Of those, an infinite number will be periodic or Poincare-periodic, but a full-measure subset will not be.

This eternal-stability claim also holds for a positive-Liouville-measure set of planar 4-body solutions obeying certain symmetries, e.g. say the four bodies are A, A', B and B' with center of mass fixed at (0,0), where the motion keeps the positions of A and A' always the negation of each other, and the same for B and B' (causing the bodies always to form the corners of a parallelogram, whose shape changes with time) *provided* we demand the perturbations preserve that symmetry.

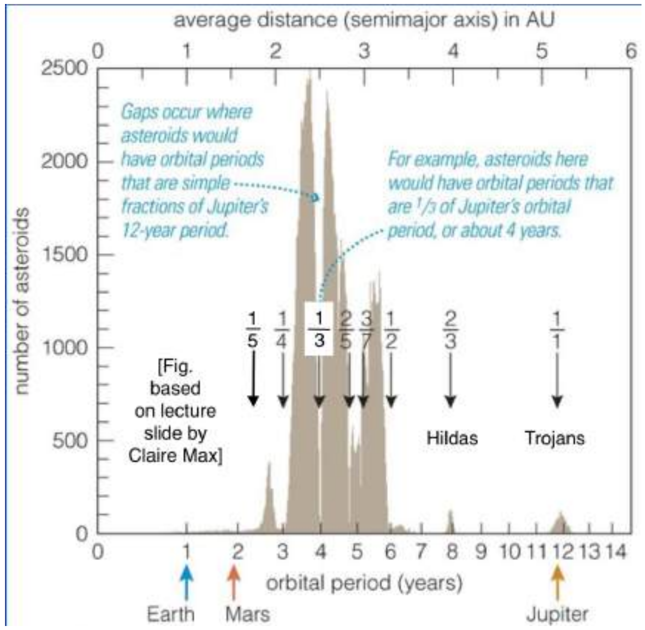
In the **"*planetary* (N+1)-body problem"** one of the bodies ("sun") is regarded as far heavier than the other N ("planets") and the inter-planet interactions are regarded as small by comparison to the sun-planet interactions, and hence hopefully treatable by some kind of perturbation theory starting from N exactly solved sun-planet 2-body problems. Imagine the planet masses all are multiplied by ε with $0\leq\varepsilon\leq 1$. Here is a theorem originating with V.I.Arnold in his original 1963 KAM theory paper, and later proved by others including P.Robutel, M.Herman, J.Fejoz, L.Chierchia, G.Pinzari during 2000-2015. (Arnold only proved it in the planar case with N=2, but the later authors allowed 3-space and arbitrary finite N):

Theorem on planetary system "stability" by V.I.Arnold & successors: *If* when $\varepsilon\to 0$ the N planet orbit-ellipses are near-enough to circular and with small-enough inclinations of their planes away from one particular plane, and their periods are generic-enough to avoid "resonances," *then* if we "turn on" inter-planetary interactions by increasing ε above 0, there will exist some $\delta>0$ such that for any ε with $0\leq\varepsilon<\delta$ a set of such N-body problems with positive Liouville-measure will exist, whose time-evolution will permanently resemble the ε=0 version of the problem with no planet's eccentricity or inclination or orbital period ever changing by more than small amounts (which go to 0 when δ does).

Nekhoroshev extension: Furthermore, if we are not interested in a "positive measure" subset of perturbations, but actually all of them, then we can show long times are required before the perturbations can grow large.

Unfortunately that theorem does *not* prove our actual physical solar system is "stable" for at least **three reasons:**

1. The solar system has δ=1. The KAMsters, to make their proofs work, need δ to be small. (How small depends on the author and how hard they worked,



and tends to improve slowly if you wait for harder-working KAM-authors to come along. For example, perhaps $\delta=10^{-25}$ might suffice for solar systems resembling ours.)

2. Our solar system contains **resonances**, most notably the Pluto:Neptune period ratio 3:2. Jupiter's satellites Io, Europa, and Ganymede (1:2:4); and Saturn's satellites Mimas and Tethys (1:2), Enceladus and Dione (1:2), and Titan and Hyperion (3:4) also are in resonance; and Uranus' moons Miranda and Umbriel are approximately in 3:1 resonance. Various known asteroids resonate with Jupiter, e.g. the Hildas with periods 2/3 that of Jupiter. (Asano, Noba, Petrovsky 2024 argue this is because they can devise an approximation of the restricted 3-body Sun-Jupiter-Hilda system which is integrable and hence enjoys stable motions. Cosmographia put out a video of Hilda orbits in 2010.)

   How did those resonances originally arise? A big part of the answer (everybody believes) is planet and moon **migrations**. The orbits of moons can and do "migrate," usually causing low-orbiting moons to gradually rise, as a result of nonHamiltonian effects, namely tidal drag versus the faster-rotating planet. As a moon rises, it can reach a suitable orbital resonance with another, originally higher-up, moon. Once that happens, the resonance is "locked in" and any further tidal-drag-caused migration does not destroy the resonance, but rather from then onward affects *both* resonating moons as though they were just *one* rigid body. It also is believed that many planets migrated during the early history of the solar system, due to nonHamiltonian effects of aerodynamic drag from gas which then was present, and/or due to a large number of planetismals which no longer are present becauee they were expelled or got eaten by, e.g. Jupiter. In particular, Jupiter is thought to have migrated closer to the Sun, while Neptune migrated 10 AU outward. The latter caused the Pluto, Plutino, and Twotino resonances with Neptune. Migrations of Jupiter and/or asteroids engendered a lot of Jupiter-asteroid resonances.

   A somewhat opposite resonance effect is the "Kirkwood gaps": several resonances (e.g. 2:1, 7:3, 5:2, 3:1, 4:1) are *not* seen in the asteroid belt with respect to Jupiter, i.e. there are remarkably few, or no, asteroids with ≈5 periods per 2 Jupiter orbits. Moser 1978 on p.70 claims that Jupiter:Asteroid period ratios A:B with A>B>0 integers with $|A-B|\ge5$ correspond to KAM-***stable*** exactly-periodic solutions of the restricted circular planar 3-body problem and hence should not yield Kirkwood gaps; but if $|A-B|\le4$ then in at least some cases we get chaos and instability. An international collection of 6 high-school students (Xue et al 2020) proposed a simple model, yielding a fairly simple formula estimating how relatively "severe" the effects of such A:B resonances ought to be:

$$\text{Severity} = A^{-1} \sum_{1\le k\le A} [\cos(2\pi k/A)-(B/A)^{2/3}] [(B/A)^{4/3} + 1 - 2 (B/A)^{2/3} \cos(2\pi k/A)]^{-3/2}.$$

   I do not agree that their severity formula is computing the right thing – really, we want to compute Lyapunov times – but nevertheless they found that their formula's predicted **ordering** from most to least severe of the A/B with $0<2B\le A\le9$ with gcd(A,B)=1 defining candidate Kirkwood gaps:

   2/1, 3/1, 5/2, 7/3, **9/4**, **8/3**, 4/1, **7/2**

   agreed with observation (based on ordering the gaps to make the counts of observed-asteroids from an asteroid dataset in *Mathematica* with major orbit axis within ±0.1 AU of the proposed gap-center *decrease*) *except* that based on asteroid-counting 4/1 was the *most*-depleted gap (with only 29 asteroids), while their formula predicted 4/1 to be the second-*least* severe. They explained this discrepancy by arguing the 4/1 asteroids were the nearest to Mars, plus suffer a damaging near-3:1 resonance with Earth. I have em**bold**ened the A/B with $|A-B|\ge5$, which Moser had predicted should not correspond to "gaps" at all. (The high school students had been unaware of this claim by Moser.) Each of those emboldened "gaps" indeed contains over 1400 asteroids.

3. Computer numerical integrations showing that our solar system is "chaotic" *refute* the theorem!

Starting from any mass 3-tuple obeying the Gascheau-Routh inequality, we can vary it continuously while keeping $M_1+M_2+M_3$ fixed, until reaching one of the KAM-stable *restricted* 3-body solutions, i.e. with the lightest mass infinitesimal, and doing this in such a way that the Gascheau-Routh inequality always remains satisfied. I would imagine using a line-segment or circular arc, or perhaps a cubic curve, as the "path" in the mass plane usually would work. (Or any analytically-smooth curve.) By our KAM heuristic, a full-measure subset of the orbits on essentially any such path ought to enjoy KAM stability.

Extending circular planar restricted N-body theory to N≥4. If N=4, we can make the 3 massive bodies rigidly rotate according to Lagrange's equilateral triangle solution, then enquire about the behavior of the fourth, negligible-mass, body. (More generally we could have N-1 massive bodies in a rigid-rotator configuration, then enquire about the behavior of the N^{th}, negligible-mass, body.) The whole Jacobi-Hill theory can be extended to handle this. The "Jacobi constant of the motion" in the rotating xyz coordinate system is

$$J = (x^2+y^2)\omega^2 + 2G \sum_{1\le k\le N-1} (M_k/r_k) - 2(\dot{x}^2+\dot{y}^2+\dot{z}^2)$$

where r_k denotes the distance from the asteroid to body $k\in\{1,2,...,N-1\}$ (all these bodies "remain stationary" in the rotating coordinate system) and the rotation is at angular velocity ω in the xy plane about the center of mass at (0,0,0).

Just as in the usual Jacobi-Hill theory with N=3, we still have uncrossable Hill "zero velocity surfaces," we still have a finite set of Lagrange-like "points of equilibrium" where the tiny-mass-astreroid would "remain stationary"; and these equlibrium points can be unstable to exponentially growing infinitesimal perturbations, or not. In the case N=4, it turns out there are always 8, 9, or 10 of these equilibrium points, and in the case where the 3 primaries (forming a rigidly-rotating equilateral triangle) are Routh-stable, always exactly 8. And of those 8, at most 3 can enjoy linear stability, where these 3 always lie outside the equilateral triangle and near its edges not its vertices; and it is known, as a function of the 3 primary mass-values, which subset of these 3 are stable. For a review of the N=4 case see papers by Alvarez-Ramirez & Vidal 2009, and Ramirez & Alvarez-Ramirez 2022.

2.11. Cauchy problem for Einstein general relativity.

Even though Einstein introduced his equations in 1915 it was not until 1952 that it was firmly established that they allow a formulation as an initial value problem. The seminal paper was written by Yvonne Choquet-Bruhat and it contains a proof of local existence of solutions. Due to the

diffeomorphism-invariance of the equations, the step from local existence to existence of a maximal development is non-trivial. The reason is that even if there is a development maximal in the sense that it cannot be extended, there is no reason to expect this development to be unique. In the end one does in fact need to restrict one's attention to a special class of elements in order to get an element which is maximal and unique in the given class. Partly as a consequence of this it was not until 1969 that Choquet-Bruhat and Robert Geroch demonstrated given additional data there *is* a unique maximal globally hyperbolic development (MGHD). The existence of an MGHD does not say anything about the global properties of solutions, but it nevertheless fundamental theoretical starting point for any analysis of solutions to Einstein's equations. To take as but one example, the question of predictability in general relativity, as embodied in the strong cosmic censorship conjecture, is phrased in terms of the MGHD.
– Hans Ringström: *The Cauchy Problem in General Relativity* (2009) introductory paragraph.

3. Survey of the "confined chaos" controversy

This section will explain, discuss, and highlight the open question of whether Newtonian N-body time-evolution can exhibit "eternally confined chaos." (My guess is "yes.") I consider this the single most important open question about the Newtonian N-body problem, even though it has not been included in published lists of such questions. Fortunately, we shall not actually need the answer to this open question. It would *help* if its answer were "yes," but we shall not *need* that, because much weaker claims, such as "Lecar's law," seem adequate for the purposes in the present paper. Unfortunately those weaker claims also are mathematically open because they arose from empirical evidence, not mathematical proof.

Euler's simplest 3-body solution – three equal masses at the endpoints and midpoint of a rotating line segment – suffices to disprove the following

Widely stated, but false, conjecture: Any chaotic Newtonian 3-body solution will eventually expel one of the bodies.

[E.g. a conjecture that includes this is in §3.2.3 of George Voyatzis, John D. Hadjidemetriou, Dimitri Veras, Harry Varvoglis: MNRAS 430,4 (Apr.2013) 3383-3396.]

What *might* be true (and seems reasonably consistent with computer-experiment evidence suggesting expulsions happen 100%, or nearly, e.g. perhaps 99%, of the time for random chaotic 3-body problems selected from certain probability distributions, e.g. see Valtonen 1975 and Valtonen & Karttunen 2006, and references therein) is

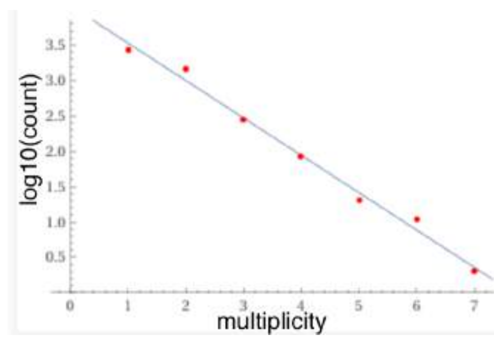
Possibly-true "expulsion conjecture:" *Almost* every (i.e. a full measure subset) chaotic 3-body solution with negative total energy and center of mass fixed at the origin will eventually expel at least one of the bodies to spatial infinity.

More likely (since weaker) "equal mass expulsion conjecture:" Almost every chaotic 3-body solution *with all three masses equal* with negative total energy will eventually expel one of the bodies to spatial infinity.

My main conclusion: I believe the first of those two conjectures (and possibly also the second) is **false**. The reasons I believe that will become clear by the end of this section – there is a lot of evidence, ideas, and claims (some wrong!) to survey.

Criticism of Valtonen: Obviously, the probability distributions in Valtonen's experiments were *not* the probability distributions of real-life asteroids, a large fraction of which have chaotic orbits but have *not* yet been expelled by the (approximately 3-body) Sun-Jupiter-Asteroid system even after 4.5 Gyr. If Valtonen had simulated *those* probability distributions, he would have reached the exact-opposite conclusion.

I unfortunately must *denounce* the so-called statistical studies discussed in ch.7 of Valtonen & Karttunen 2006 as being **very misleading** because their probability distributions obviously differ enormously from the distributions that actually occur in astonomy. (And indeed they misled, e.g. Wakker 2015, who falsely wrote near the bottom of his p.45, within any chaos-demand: "The centuries of investigations in the three-body problem have reached the point where most theoreticians believe that, when each of the three masses is non-zero, all solutions are basically unstable, in the sense [of eventual escape].")



Wakker Counterexample: Bright multiple-star systems. Eggleton & Tokovinin 2008 examined 4559 bright stellar systems (including our Sun), containing 6971 individual stars, finding the counts of systems with stellar-multiplicities N=1, 2, 3, 4, 5, 6, 7 to be 2718, 1437, 285, 86, 20, 11, 2 (with sum=4559). This probability distribution is empirically fit well by Prob(N)≈exp(9.345-1.216N), see logarithmic plot. Indeed, the closest 3 stars to our sun form the Alpha Centauri triple star system. Based on this, Wakker's claim that based on "centuries of investigations," triple stars ought to be a "probability zero" phenomenon, is absurd. In fact, 20% of the stars in E&T's bright subsample were members of triple (or greater) multiple-star systems, which usually have survived for Gyr timescales. A specific example is the triple star system HD 188753, located ≈149 light-years away from Earth in the constellation Cygnus. Its three stars are A, a yellow dwarf (1.06 solar masses); B, an orange dwarf (0.96 solar masses); and C, a red dwarf (0.67 solar masses). B and C orbit each other every 156 days, and, as a group, orbit A every 25.7 years. The estimated age of this system is 5.6 Gyr, i.e. over $2{\times}10^8$ of the longer kind of orbit, and $1.3{\times}10^{10}$ of the shorter kind. The 6-star system TIC 168789840Z (estimated age 3.2±0.6 Gyr) contains three separate binaries of which two orbit each other, while the third pair orbits the other two at a greater distance, all eclipsing each other from Earth's point of view.
And if Valtonen, Wakker, etc tried to claim the empirical Gyr survival of triple & higher multiple-star systems was misleading in the sense that, mathematically, they all *eventually* will self-destruct, then they still would be wrong. Because: KAM theory shows the **mathematical theorem** that the measure of never-escaping Newtonian 3-body solutions, is *positive* – indeed even if we also demand that there be a positive lower bound on all interbody pair separations – and indeed approaches *full* measure within neighborhoods of (known) exactly-periodic or Poincare-periodic KAM-stable 3-body solutions. (Also, other known theorems state that the measure of eventually-colliding 3-body solutions, is zero.) Although this rigorous mathematics is only capable of proving tiny positive lower bounds on that measure, Eggleton & Tokovinin's observational data suggests that the fraction of natural 3-body bright stellar systems that never suffer either escape or collision, is somewhere between 15% and 100%.
However: Eggleton & Tokovinin did not say how many of their systems were chaotic. Alpha Centauri (4.2-4.4 light years distant; estimated to be 5-6 Gyr old), is *not* chaotic, but rather "hierarchical." It consists of a binary (A and B) of two sun-like stars (masses 1.1 and 0.9 M_{sun}) with period≈79.8 years, plus a faraway red dwarf C (mass=0.12 M_{sun}), which orbits the binary with period≈511 kyr, with orbit inclination angle (with respect to the AB orbital plane) about 30° according to the end of §3 of Kervella, Thevenin, Lovis 2017. Apparently the closest known *chaotic* multiple star system is HD 5005, which is about 10000 light years from us and contains 5 stars, all apparently young (3 Myr?), including 2 close binaries, so that if viewed through a telescope without very high resolution, it appears to be a chaotic "approximately triple" star system.

On the other hand, enough "rogue planets" have been detected to make it clear that ejection of planets from (their former) solar systems has happened frequently. Sumi et al 2011 had estimated based on microlensing observations that there are 1.8 mass≥Jupiter rogue planets per star in the Milky Way galaxy (with a large error bar), but Mroz, Udalski et al 2017 revised that downward to "≤0.25." Nevertheless Sumi et al 2023 estimated based on microlensing observations that there are 20 rogue planets (each with mass between 0.33 and 6660 earth-masses) per Milky Way star, to within a factor of 2, amounting to about 80 Earth-masses worth of rogue planet mass per star. Bhaskar & Perets 2025 based on simulations estimate that 40-80% of planets in solar systems get ejected between 10^8 and 10^9 years after formation, with mean ultimate velocity of 2-6 km/sec. The ejected ones are usually lighter than the non-ejected ones. Based on that, probably 1-40 planets were ejected from our own solar system (say) 4 Gyr ago, in which case they (if traveling at 4 km/sec) would be ≈50000 light years away from the sun by now and almost impossible to detect.

Empirical nature of triple-star systems with all 3 stars having roughly equal masses. Perets 2025 claims that "broadly speaking" there are 4 kinds of near-equal-mass 3-star systems, which he classifies by approximately regarding them as a binary plus one extra star orbiting that binary, and considering the outer/inner radius ratio X and the angle of inclination θ between the binary's (oriented) orbit plane and the outer star's orbit plane.

1. "**Highly hierarchical**" systems. Consist of a close binary orbited by the third star at much greater radius. X≥7.5, inclination angles (of binary's plane versus third-star's orbit plane) θ≤40° or θ≥140°. These systems empirically stay stable with fairly constant main orbital parameters, namely approximately constant semi-major axes, eccentricities, and mutual inclination. Example: Alpha centauri.
2. "**Secularly evolving hierarchical**" systems X>7.5 but high inclinations 40<θ<140°: The eccentricity and inclination exhibit long-term oscillatory changes, driven by mechanisms such as the von Ziepel-Lidov-Kozai effect. These systems are stable in the weaker sense they do not expel anybody, nor do collisions occur.
3. "**Quasi-secular**" systems (3≤X≤7.5 and high inclinations): Dynamics intermediate between hierarchical and non-hierarchical regimes. Often experience extreme eccentricity and inclination oscillations. Often chaos. Often evolve into case (2) or (4).
4. "**Chaotic non-hierarchical**" systems (1≤X≤3). Typically leads to the ejection of one component and/or collision or strong tidal interactions between two stars. Behavior complex with details unpredictable for all practical purposes. Abt & Corbally 2000 searched for such systems, and found 14. Based on star spectra types, all 14 seem younger than 50 Myr. This suggests that there are few or no *old* triple stars of this kind – as predicted by the near-equal-mass case of the expulsion conjecture.

If the above 4 cases were known to be the only possibilities, and if all the chaotic cases were known to be temporary, then that would prove the equal-mass weakening of the expulsion conjecture.

The "ergodicity" argument in favor of expulsion conjecture... Suppose, or hope, that N-body chaos is "quasi-ergodic." That is, in (say) the case of the Newtonian 3-body problem, with probability=1 the chaos visits arbitrarily near every part of the "accessible" region of phase space, i.e. with the visited-set ultimately becoming everywhere dense if the accessible chaotic region is a compact set. Here "accessible" means "not prevented by any of the classic conservation laws, and not prevented by Hill-like surfaces."

Then: if there were any way out, i.e. some positive-measure way that one of the three bodies could escape, we would expect it eventually to happen. Once it did, then by definition it would be irreversible.

...and its failure. In reality, it is not necessarily obvious which parts of phase space are "accessible," and nobody has ever proven quasi-ergodicity of 3-body chaos. **I've become convinced that even the revised "3-body chaos⇒ergodicity" conjecture is still wrong** (and certainly any "rapid mixing" version of it must be wrong) for the *restricted circular* 3-body problem after seeing the numerical experiments of Brasser, Heggie, Mikkola 2004, confirmed by Pitjev & Sokolov 2004. These showed the "confinement of chaos" by (in this case) a 1:1 "Trojan resonance." E.g. in one positive-measure set of restricted circular 3-body setups resembling Sun+Jupiter+Trojan asteroid, the asteroid can librate chaotically in $\mathbb{R}^3$ for very long times (apparently forever), without *ever* escaping, and without ever converting Trojan↔Greek. Such a conversion is not prevented by any of the classic conservation laws, nor by any uncrossable Hill surface (nor is expulsion); instead their prevention in those experiments is *dynamical*. Another positive-measure set of (at least initially) chaotic 3-body trajectories that apparently never become quasi-ergodic was found in numerical work by Sicardy. Other resonances also seem able to confine 3-body chaos. For example in 3:2 resonances such as Sun-Neptune-Pluto and the Sun+Jupiter+"Hilda" asteroids, chaos can persist apparently forever, with the angular orbital-longitude relationship between Neptune & Pluto (or between Jupiter & asteroid) exhibiting mutually-inaccessible positive-measure subsets. E.g, Pluto can never convert into Orcus. And Hilda asteroids which by definition orbit the sun 3 times for each 2 Jupiter orbits, never phase shift by ±1 orbit versus Jupiter. I'll discuss the apparent-failure of the ergodicity argument more later. My point is that ergodicity, even its weaker form "quasi-ergodicity," and apparently even my further weakenings above, all are *mythical* for 3-body problems. And after their downfall, very little underlying theoretical rationale for the expulsion conjecture remains.

The expulsion conjecture represents one side of a controversy about the Newtonian 3-body problem. The other side is the following (which I favor)

100% opposed conjecture: "stably-confined eternal chaos": It is possible for "**eternally *confined* chaos**" (I reject the oxymoronic name "stable chaos"!) to happen for a positive-Liouville-measure set of initial conditions for 3 bodies (and/or perhaps N bodies for some moderate N≥3, or each N≥3), in which all pairwise distances between bodies remain permanently bounded between two positive constants, and this happens for a positive Liouville-measure set of initial conditions, despite the time-evolution being permanently chaotic, i.e. with Lyapunov time (assessed over timespans→∞) bounded between two positive constants.

The purpose of this section is to critically discuss this controversy. And I shall add a few contributions of my own that they'd been ignoring.

Eternally confined chaos certainly is possible for *some* Newtonian mechanical systems. The frictionless double pendulum (Shinbrot et al 1992) is a simple planar mechanical system which is (a) gravitational, (b) is (with the right parameters) *chaotic*, and (c) the distances between the masses (by definition, thanks to the two rigid rods) remain permanently upper- and lower-bounded by positive constants. Another example is geodesic motion on a "double torus" closed and bounded negative-curvature surface – this is permanently chaotic (as was first pointed out by J.Hadamard in 1898), but always has constant speed. A third is chaotic rotation of certain rigid bodies about a fixed point in 3-space in a homogeneous gravity field (Borisov, Kilin, Mamaev 2008).

But those were not *celestial mechanics* Newtonian N-body problems. Vrbik 2013 finds chaos in a positive-measure subset of initial conditions for the ***restricted* circular planar 3-body problem**, and this chaos is *eternal* thanks to (1) Hill region confinement, and (2) the fact that orbits involving collisions arise from a zero-measure set of ≤3-body problems (and anyhow even if collisions did occur in Vrbik's scenario, they would be regularizable to elastic bounces).

A different kind of eternally confined chaos in the **restricted** circular planar (or nonplanar) 3-body problems, is, I conjecture, something like the Earth-Moon system as the two primaries, and a small spaceprobe with the right Jacobi constant so that it is just barely able (i.e. Hill surfaces almost, but do not quite, prevent) to switch between being in orbit round the Earth, versus being in orbit round the Moon. The spaceprobe traverses the narrow "neck" of the dumbbell-shaped Hill surface near L1 whenever it "switches." The dumbell-shaped Hill surface fully encloses, and thus prevents the spaceprobe from ever escaping, the Earth-Moon binary system. I believe that with probability=1 any such spaceprobe will make an infinite number of switches, do so nonperiodically, and the sequence of positive integers saying #Moon orbits, #Earth orbits, ... the probe successively makes between switches, can be *any* sequence made from a certain alphabet of suitable positive integers (alphabet contains at least two "letters") i.e. uncountably many topologically-distinct options. That in turn would force the spaceprobe's motion to be permanently chaotic. The underlying reason for this chaos is that each time the probe passes through or near L1, it stays there a long time traveling at very slow speed (in the rotating coordinate system) and is then very sensitive to small perturbations – for example a small perturbation can make it "switch" or not.

> Stoffer & Kirchgraber 2001 claim to come close to proving (with computer aid) that the above Earth-Moon-spaceprobe system indeed is chaotic, and indeed for essentially the reason I proposed. But they say they did not "strictly speaking" prove this, because they and/or their computer "replace certain rigorous error bounds by what [they] call realistic estimated error bounds."

Let me also remark that it is known that any Hamiltonian system containing in its dynamics a "Smale horseshoe" necessarily contains both (1) a countable infinity of periodic orbits, and (2) an uncountable infinity of eternal-cycling nonperiodic orbits, one corresponding to every infinite binary-word. My construction (preceding paragraph) does contain a Smale horseshoe. However, (2) could fail to have positive measure because "escapes from the horseshoe" might have full measure. [If so, then (2) still should have positive Hausdorff dimension.] But in my particular case, due to Hill confinement I think the escapes must have zero measure and hence the set of chaotic orbits must have positive measure.

However, the possibility of arbitrarily close approaches of his "asteroid" to his point-"sun" was not excluded by Vrbik (and my construction similarly does not exclude arbitrarily close approaches of my point-spaceprobe to the point-Earth and Moon); and Vrbik's work (and my Earth-Moon conjecture too) concern *restricted*, not *genuine*, 3-body problems.

Also, the Moeckel-Montgomery theorem and my own new nonperiodic theorem plainly show that permanent chaos is possible in an infinite number of ways in the Newtonian 3-body problem (indeed the same-cardinality infinity as the reals) but those do not show the set has *positive measure*.

Nine pieces of evidence supporting, or at least "consistent with," the confined eternal chaos conjecture (and some related analysis):

(1) The long lifetime (at least 100 Gyr in simulations, i.e. ≥5000 Lyapunov times) of the Sun-Neptune-**Pluto** system, featuring "mean-motion resonant" 3:2 period ratio for Pluto:Neptune. Note that Pluto is nearer to the Sun than Neptune about 8% of the time. The mass ratios Sun/Neptune≈19410 and Neptune/Pluto≈7000 (where for this purpose I have included Pluto's moons as "part of Pluto") have the same order. Malhotra & Ito 2022 simulated Sun+Pluto for 5 Gyr accompanied by either Neptune, Uranus+Neptune*, Saturn+Neptune, Jupiter+Neptune, Saturn+Uranus+Neptune*, Jupiter+Uranus+Neptune, and Jupiter+Saturn+Uranus+Neptune (7 versions in all) finding survival in the 5 unstarred cases – and Ito & Tanikawa 2002 simulated the J+S+U+N case for 10^{11} years without a problem – despite Pluto's Lyapunov time being only about 20 Myr (equivalent to about 80000 Pluto orbits) in the J+S+U+N case – so that 100 Gyr is 5000 Lyapunov times. (Uranus seems to have a destabilizing effect on Pluto, but Neptune with any nonempty subset of {Saturn, Jupiter} – or just Neptune alone if Uranus is removed – suffice to overcome that and keep Pluto's chaos confined.)

(2) The asteroid "522 Helga," which seems to be "in a stable but [extremely] chaotic orbit in resonance with Jupiter" (to quote Wikipedia) with 7:12 mean-motion period ratio for Helga:Jupiter. **Helga**'s Lyapunov time is only 5-11 kyr. See Milani & Nobili 1992, who simulated Helga plus Sun,Jupiter,Saturn,Uranus,Neptune for over 1000 Lyapunov times, finding no appreciable change. Holman & Murray 1996 then numerically-integrated 10 clones of Helga for 5 Gyr each, i.e. nearly a million Lyapunov times each, along with Sun, Jupiter, Saturn, Uranus, and Neptune, both confirming M&N's Lyapunov-time estimate of 6900 years, and finding 5 Helgas escaped while the other 5 survived. M&N write: "[Although] Helga is the first clear-cut example of a stable chaotic orbit, we argue that 'stable chaos' may be rather common." But H&M contend their 5 Helga escapes indicate that Helga is *not* an example of "stable chaos" since its "halflife" is about 5 Gyr, not infinity. H&M indeed vociferously denied the very existence of "confined chaos" in N-body systems, albeit providing no real basis for that denial aside from their unshakeable self-confidence.

More importantly, Franklin, Lecar, Murison 1993 produced, I suppose, *some* basis for such a denial, finding based on a lot of computer simulations **Lecar's empirical law** relating Lyapunov time T_L to expected collision/escape time T_E for 2 classes of asteroids (both times measured in units of Jupiter orbit-periods using simulations; tried 3-body Sun-Jupiter-Asteroid and 4-body Sun-Jupiter-Saturn-Asteroid systems, plus many classes of artificially-generated circular restricted 3-body systems):

$$T_E \approx A\,(T_L)^B \quad \text{with the constants} \quad B=1.8\pm0.1, \quad \text{and} \quad A=30\pm6.$$

(Quote from LF&M: "The tight clustering of the exponents B [near 1.8] was remarkable... the maximum departure of B from 1.8 was 14%, and on average ≤7%. The [relation] holds over at least 6 orders of magnitude in T_E..." Exceptions to the Lecar law were so rare they did not see any in 150 samples from each class, amounting to about 1500 in all.)

Murison, Lecar, Franklin 1997 refined and further-examined Lecar's law, now finding B=1.74±0.03 and A=20±1.4. Fig.3 on their p.10 shows their loglog line fit (using $\log_{10}$) justifying that claim, while fig.4 p.11 shows their residuals are Gaussian. Their table II gives T_L and T_E (both in units of Jupiter periods) for 25 outer-belt resonant asteroids (and states their resonances) with $260 \le T_L$ up to "≥8100." and T_E from 340000 to ">128270000."

But Lecar *admitted* in his 1996 review that his law fails for the **Hilda** asteroids, many of which appear to exhibit *eternally*-confined chaos, and see his quote below.

Milani & Nobili 1992 arbitrarily chose "survival for 1000 Lyapunov times" as their threshold defining "stable chaos" – call that the "**1000×Lyapunov** criterion." What would seem a "more logical" or "less arbitrary" choice would have been "survival times 10× longer than the Lecar law predicts" – call that the "**10×Lecar** criterion." By the 10×Lecar test, Pluto's 100 Gyr≈$5000T_L$ survival time in simulations is too short by a factor of 4 to constitute evidence for confined chaos (for Neptune-period-based Lecar law, rather than Jupiter-period-based), despite M&N's 1000×Lyapunov criterion accepting it since 5000 exceeds 1000 by a factor of 5.

522 Helga, however, really does well-satisfy both the 10×Lecar and 1000×Lyapunov tests, in the sense that its 5 Gyr halflife ($T_E \approx 4\times10^8$ Jupiter orbits), exceeds $(T_L)^{1.8} \approx 95000$ by a factor>4000. Nevertheless Holman & Murray would contend that the finiteness of T_E refutes Helga as an exemplar of "eternally confined chaos." But arguments by **Wisdom 1980 and Gladman 1993** indicate that "*improved* Helgas" might really be eternally chaotic. Gladman's criterion (based on Jupiter's Hill radius) that he claims (at least after slight modification of Jupiter and Helga to make their orbit ellipses nearer to circular) would provably prevent close approaches of Helga to Jupiter for all time, is satisfied. And on pages 258-259 Gladman indicates his belief (based on analyses and computer simulations) that 3-body systems of this nature can be "[permanently] trapped in an isolated chaotic region" (and he provides a putative example) with such permanent chaos never "producing any macroscopic signature" e.g. never causing an orbit crossing; and that this "*usually*" happens within a region of parameter space slightly within, but not hugely within, the "Hill stability" bounds. Gladman says this is most clear for near-circular orbits. (This point bolstered and clarified earlier ideas by Wisdom 1980, who had not employed Hill regions.) **Quotes:**

"The topological stability criterion from the three-body problem furnishes a useful stability formula for the case of the system of two planets [and a mass=1 sun] (although as a test of instability it is less useful for initial eccentric orbits). For initially circular orbits ($e<M^{1/3}$) a fractional initial orbital separation of at least $\Delta\approx2.4(M_1+M_2)^{1/3}$ insures Hill stability [this valid for $0\le M_j \ll 1$; higher order correction given in Gladman's EQ 23]. Systems with slightly larger orbital separation *usually* display chaotic behavior. The observed chaotic behavior is confined away from the zone of ... planet-crossing behavior by the Hill stability surface [or its generalizations by Marchal & Bozis 1982, etc]. Many of the chaotic systems show no macroscopic signatures [e.g. escape] of the chaos in orbital elements over thousands to millions of Lyapunov times."
– Gladman 1993's "Conclusion" p.262.

"The region of 'overlapping residences' (Wisdom 1980) extends inward from Jupiter a distance $1.49\mu^{2/7}$ using the coefficient found by Duncan, Quinn, Tremaine 1989. On the other hand, orbits further from Jupiter than $2.04\mu^{1/3}$ are prevented from having a close approach to Jupiter by a Hillian 'zero velocity curve' (Gladman 1993, Birn 1973) where $\mu=M_{Jupiter}/M_{Sun}$. If $1.49\mu^{2/7}>2.04\mu^{1/3}$, then this overlap region contains orbits with positive Lyapunov exponent that are nevertheless bounded away from Jupiter i.e. a region of orbits with positive Lyapunov exponent which *never* closely approach Jupiter, in violation of 'Lecar's law'."
– p.164 of **Lecar** 1996, summarizing thoughts related to Gladman (corrected). The Wisdom / Duncan-Quinn-Tremaine / Gladman long-lived chaos zone exists when $1.49\mu^{2/7}>2.04\mu^{1/3}$, which is satisfied when $0<\mu<0.0013636\approx1/733.3$, which is satisfied by our solar system's actual $\mu\equiv M_{Jupiter}/M_{Sun}\approx1/1048$. Lecar, following Wisdom 1980, is using length units in which the Jupiter-Sun distance equals 1. The "overlapping resonances" concept as a mechanism to generate chaos – in some cases "eternal chaos" – began with Walker & Ford 1969 and Chirikov 1979. It is based on the idea that you do not need an *exact* resonance (integer frequency ratio); merely an approximate resonance (near-integer ratio) can suffice after a moderate finite number of orbits, to kick you into some new regime where another near-resonance can then affect you similarly, and so on – perhaps forever, in which case we have "eternal chaos."

"I have applied the resonance overlap criterion to the planar circular restricted three body problem and compared its predictions to some numerical experiments. Since the predictions are in remarkably good agreement with my numerical experiments, great confidence has been gained in the usefulness of the resonance overlap criterion for providing a qualitative understanding of the instabilities of the solar system."
– Conclusion of **Wisdom** 1980.

Tsiganis, Varvoglis, Hadjidemetriou 2002 followed up M&N by proposing the following hypothetical explanation different from Gladman's:

"The occurrence of stable chaos in [Helga's] 12/7 mean motion resonance with Jupiter is related to the fact that there ***do not exist* families of periodic orbits** in the planar restricted elliptic [non-circular 3-body] problem, and in the 3D circular problem, corresponding to this resonance. In the present paper we show that nonexistence of resonant periodic orbits, both for the planar and for the 3D problem, also occurs in other Jovian resonances, namely 11/4, 22/9, 13/6, and 18/7, where cases of real asteroids on stable-chaotic orbits have been identified [50 Virginia, 33 Polyhymnia, 86 Semele, 2 Pallas]."

Tsiganis et al numerically-integrated each of these asteroids, both with Sun+Jupiter only, and also, in a different run, also including Saturn+Uranus+Neptune, for both 10^5 and 10^8 Jupiter-periods (each 11.86 Earth-years). The Lyapunov times were always between 5 and 20 kyr, *except* that Polyhymnia appeared non-chaotic in the Jupiter-only simulations. In all cases, all asteroids' orbital parameters remained within bounded intervals *except* that Virginia got ejected after about 1 Gyr of the all-4-outer-planets simulation. These survivals all easily pass both M&N's $1000T_L$ and the 10×Lecar test for "permanent chaos." Tsiganis et al's "proof of nonexistence" of periodic orbits with those periods appears to be based on trusting that a computer search using numerical ODE methods would have found them if they existed – which seems plausible to me, but not a "proof." They claim the alleged absence of such periodic orbits is explainable as a "topological defect," but I do not understand that. However, it might be possible to actually prove this absence using Mather 1986, which would be nice since as far as I can tell Mather 1986 has not yet ever been used for anything.

"This [absence of periodic resonant solution] property may provide a 'protection mechanism,' leading to semiconfinement of chaotic orbits and extremely slow migration in the space of proper elements, so that diffusion is practically unrelated to the value of the Lyapunov time."

Tsiganis et al's "no periodic orbits" explanation suggests that a positive-measure set of *non*periodic "eternally stable chaos" restricted 3-body solutions must exist with *exact* 12:7 mean-motion ratio with Jupiter (assessed in the limit of large times). But Tsiganis et al's explanation is *incompatible* with Gladman 1993's; and cannot hold for those "Jupiter Trojan" asteroids (since their periods are in **1:1** mean-motion resonance with Jupiter's) which exhibit chaotic "librations," for example 2594 Acamas with Lyapunov time≈25000 years, 1173 Anchises, 1404 Ajax, 1869 Philoctetes, 2207 Antenor, 2797 Teucor, 4754 Panthoos, and 4827 Dares. Milani 1993 does not consider either Acamas or Anchises to be "stably chaotic," but says the other six are. A **variant Lecar law specialized for use with *trojan* asteroids** was proposed by Tsiganis-Varvoglis-Dvorak 2005, involving somewhat different constants A=0.75±0.08 and B=1.62±0.03 than the main Lecar law intended for non-Trojan asteroids. However, *eternal* Trojan chaos seems to occur (Brasser, Heggie, Mikkola 2004, Pitjev & Sokolov 2004; and the "Sicardy bump") in certain parts of the Trojan parameter space that TV&D never examined.

Also Franklin, Lecar, Murison 1993 claim that some of the Hilda asteroids (all of which, by definition, are in **3:2** mean-motion resonances with Jupiter), such as 499 Venusia and 1269 Rollandia, exhibit long-term chaos, surviving for $\ge2.5\times10^6$ Jupiter orbits in Sun-Asteroid-Jupiter-Saturn simulations in which their Lyapunov times were only 148 and 324 Jupiter-orbits respectively. It is known that exact-periodic orbit solutions exist featuring Hilda-like 3:2 resonances (Jorba, Nicolas, Rodriguez 2024). Also, Ferraz-Mello 1994 remarks that the vast majority of the members of the small family (exemplified by 3789 Zhongguo) of asteroids in long-lived **2:1** mean-motion resonance with Jupiter have Lyapunov times$<10^5$ years in 4-body Sun-Jupiter-Saturn-asteroid models, yet several exhibit lifetimes≥1 Gyr in simulations. There are *two* "islands of stability" visible in Poincare section pictures for the 2:1 asteroids, (Voyatzis & Hadjidemetriou 2005) one represented by Zhongguo, the other by some recently-discovered small asteroids such as "2003 SA197" with diameter≈7.15km. These are not "stable" in the sense of no chaos – they are chaotic. Rather, they are stable in the sense of surviving long times in simulations, and also in the sense the Poincare section pictures show a lot of related dots forming "islands." Speaking of 2:1 resonance, the orbits of Mimas and Tethys around Saturn are in a 2:1 mean-motion resonance and are chaotic with Lyapunov time of 300-600 Earth years (i.e. order 10^5 orbital periods), but presumably have been there for at least 10 Myr.

Winter, Mourão, Giuliatti Winter 2010 instead proposed that the reason for asteroid "eternally confined chaos" is that the asteroid motion's chaos is due entirely or nearly entirely to the *angular* component of its motion, not its *radial* component. They tested 901 hypothetical asteroids by computer simulation, finding a precise version of their hypothesis that seemed to work in 100% of their cases. It also seems to work for the chaotic Jupiter Trojans, unlike TV&H's hypothesis.

My conclusion from that debate: I believe that the Winter, Mourão, Giuliatti Winter 2010 "angular only" idea, and the Gladman 1993 idea combining Wisdom 1980's "overlapping resonances" with Hillian uncrossable surfaces are *compatible* and can explain and predict "eternally confined" (or at least, empirically very long-lived) chaos in Newtonian N-body systems, including 3-body systems. (The "no periodic orbits" idea of Tsiganis, Varvoglis, Hadjidemetriou 2002 might sometimes be valid too, but cannot be the full explanation; and they needed to prove nonexistence but were unaware of rigorous techniques such as Mather 1986 that might have enabled them to do so.)

(3) Saturn's moons **Atlas**, Prometheus, Pandora, and Mimas. According to Pereira, Mourão, Winter 2024, Atlas's orbit appears to be in a state of "eternally confined chaos" with Lyapunov time≈8.9 years, or equivalently 5410 Atlas-orbits. Meanwhile Prometheus and Pandora have Lyapunov time≈3 years according to Goldreich & Rapaport 2003. Yet their very existence suggests Atlas & co. have survived for at least 10 Myr, i.e. over 10^6 Lyapunov times.

(4) Deck et al 2012 found that the two-planet "Kepler-36" exo-solar system has chaotic orbits with a Lyapunov time of only ≈10 years – equivalent to ≈216 orbits of its outer "mini-Neptune" planet (since its orbital period is 16.87 days, 5 times shorter than Mercury's period≈88 days in our own system), which orbits only 11% further out than its inner "super-earth" planet. [Masses: star 1.07 solar masses, outer planet 7.1 Earth-masses, and inner planet 3.8 Earth masses for mass ratios 1:1/50200:1/94000 or equivalently 23594000:470:251. These two planets are 30× more closely spaced than any adjacent pair of planets in the Solar system; and have a factor-8 density contrast, the same as the Earth/Saturn density ratio in our own solar system.] Deck et al claim this apparently-eternally-confined chaos is a consequence of orbital mean-motion resonances: the inner planet orbits 34 times for every 29 orbits of the outer. Exactly *one, contiguous* region of phase space, accounting for ≈4.5% of the sample of initial conditions they studied by computer, corresponds to planetary orbits that do not show huge orbital instabilities on the 200 Myr timescale of their numerical integrations. (That 200 Myr is *20 million* Lyapunov times!) That long-lived subset of the allowed initial conditions closely corresponds to "the ones satisfying the Hill stability criterion by the largest margin."

Personally I find Deck et al's **the most impressive** astronomically-observed example: an entire solar system (made of heavy planets, not little asteroids) that appears to be permanently very chaotic; and I think its survival for $2×10^7$ Lyapunov times in simulations is a world record. Its *actual* survival time is even more impressive: Kepler-36 as a spectral type G1IV "subgiant" is a quite *old* star, 7.8±0.5 Gyr old, equivalent to $8×10^8$ Lyapunov times.

(5) See discussion in our Moore8 appendix about **Nauenberg 2007**'s numerical discovery of a positive-measure set of apparent eternally-confined chaotic 3-body systems arising from the P_{12} family of unstable periodic 3-body orbits near the "Moore8." These examples have no resemblance to any astronomer's observation ever, but arose from pure mathematical curiosity. If genuine, they refute the equal-mass weakening of the expulsion conjecture.

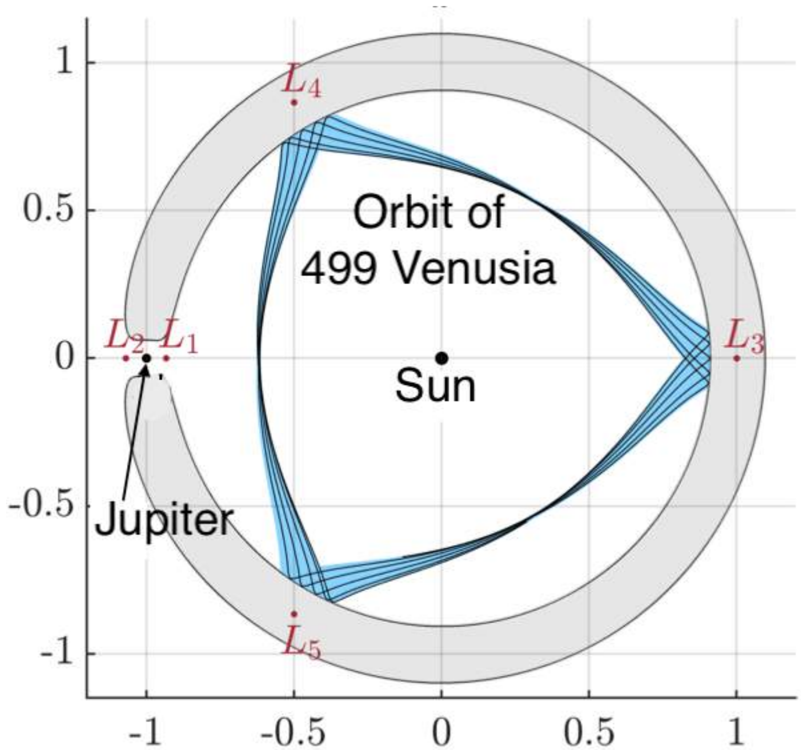
(6) As we already saw, Lagrange's rigidly-rotating equilateral triangle solution of the planar 3 body problem has exponentially-growing infinitesimal perturbations in the "restricted case" (masses Sun=1-M, Jupiter=M, and Asteroid=infinitesimal) exactly when $M>M_{Routh}=2/(27+3\sqrt{69})≈0.0385208965$. *However*, **Sicardy** 2010 based on work that is partly human analysis and partly computerized simulations (the latter seem independently confirmed by fig.2 of Schwarz, Bazso, Erdi, Funk 2012, and by p.687 of Lohinger & Dvorak 1993) claims that when 0.0385208965<M≤0.0397, the Lagrange triangle, as a restricted 3-body problem solution, remains "stable" in the sense that all or almost all asteroid perturbations stay small forever, i.e. the asteroid stays boundedly near L4 or L5 (in the rotating coordinate system) forever, e.g. **empirically exhibiting "confined chaos" when 0.0394<M≤0.0397**. Indeed, Schmidt 1994's theorem 6 *proves* KAM stability, which due to 2-dimensionality is full stability, of "detached" periodic orbits that exist nearby; and then §3 of Meyer & Schmidt 2000 proves these KAM-stable periodic orbits for the *restricted* 3-body problem, in fact yield KAM-stability for the **unrestricted** 3-body problem for relatively-small-enough third mass. However for M≥0.04 we no longer enjoy Sicardy confinement, since "chaotic diffusion allows the particle to escape" the vicinity of L4 or L5, Its escape time is very large (>10^8 orbit periods) when M is slightly above 0.0397, but decreases rapidly when M increases to 0.041.

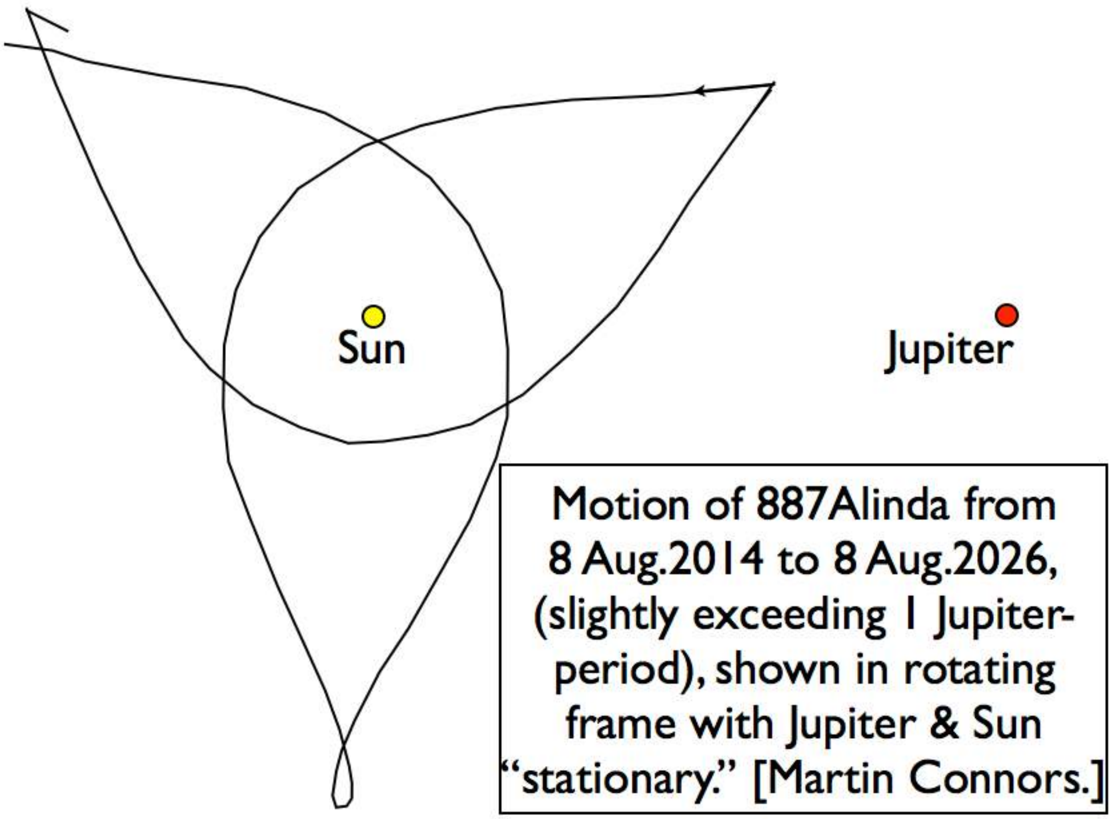
(7) Since the 1990 Belbruno-Miller rescue of Japan's Hiten moon probe using chaos-aided trajectory-design ideas, it has been taken for granted in the spaceprobe-trajectory-planning community that in restricted near-circular 3-body problems, any Jacobi invariant near the critical value J(L1) causes the spaceprobe orbit to enter "the chaotic regime." Belbruno indeed proposed the "weak stability boundary" concept to take advantage of chaotic "fuzziness" to accomplish desired trajectory goals with less rocket fuel (Belbruno, Gidea, Topputo 2013). The point is that any planar restricted 3-body solution with J≈J(L1) is "temporarily captured" when the orbit passes near L1, since by definition its velocity will be near-zero at that time, and this is a point of equilibrium; and these "temporary captures" can last arbitrarily long times. During these long sojourns the future trajectory (after temporary capture ceases) obviously is extremely sensitive to very small perturbations. This makes it seem obvious that any such orbit *must* be chaotic if it passes near enough to L1 often enough. Similar intuitions ought also to apply re the other Lagrange points L2, L3, and perhaps L4 and L5. Perhaps more importantly, but less clearly, they apply to some extent to any trajectory **cusp** where that trajectory hits a Hill zero-velocity surface (or near-cusp where it nearly hits it). This is less clear because trajectories do not stay arbitrarily near the surface for an arbitrarily long time at a cusp. (They would have done so if the speed behaved linearly proportionally to the distance to the surface, at small distances. But instead, speed behaves asymptotically proportionally to the *square root* of that distance, corresponding to a linear relationship between speed and time near the cusp.) Trajectories, in contrast, can linger arbitrarily long arbitrarily near near L1, L2, L3, L4, or L5. Nevertheless, at cusps speed hits zero (in the rotating coordinate system) and the trajectory is lingering there much longer than anywhere where the speed is nonzero, making it more sensitive to small perturbations. The orbit also is eternally *confined* thanks to the Hill surface. Almost every confined chaotic orbit with J near J(L1) should be expected to return near to L1 infinitely many times. Orbits with cusps where it hits a confining Hill surface, should be more likely to be chaotic than orbits in general.

Also, planar orbits that "bounce" off Hill surfaces and are approximately "**polygonal**" if drawn in a rotating coordinate system, have a great resemblance to "**billiards**" trajectories on certain peculiar-shaped "billiards tables" with algebraic-curve boundaries. Near-polygonal examples: Hilda orbits (3:2 resonance with Jupiter) resemble triangles. Asteroids in 4:3 resonance with Jupiter (the only prominent example is 279 Thule) have a near-trapezoidal orbit. Asteroids in 5:4 resonance with Jupiter (no examples known, but theoretically appear possible) have an orbit resembling a regular pentagon. It is believed that almost all curved billiard table shapes have chaotic orbits. The billiard-table boundary curve can be designed to cause "resonance-restoring pseudoforces" permanently confining some classes of orbits within certain angular intervals, e.g. that happens for trajectories crossing the inter-focus line-segment on a elliptical billiard table (and provably; this is a rare curve that does *not* yield chaos, and indeed a conjecture attributed to G.D.Birkhoff and/or H.Poritsky sometime between 1927 and 1950 is that the ellipse is the *only* C^2-smooth convex curve whose billiards is "integrable"). But this "angular trapping" is not at all unique to the ellipse, since, e.g. elliptical arcs glued to non-elliptical curves will work. And by connecting "Bunimovich mushroom" billiard-table shapes one can obtain dynamics featuring an arbitrary (finite or infinite) number of "KAM islands" coexisting with an arbitrary (finite or infinite) number of chaotic components, all with positive measures in phase space (Bunimovich 2001). See our appendix on billiards.

The trouble with all that for our present goal of "confined chaos" is: why can't our L1-approaching confined orbit **hit the Sun** (or hit whatever mass lies in the middle of the confining Hill-region)? If it does, the orbit is not "eternally confined," but rather "ending." That complaint is good reason to be interested in "tadpole" and "horseshoe" orbits enclosing L4, or enclosing L3, L4, and L5. These can repeatedly approach Lagrange points closely and can have numerous cusps, in which case they seem likely to be chaotic; *but* since there is no mass near there for them to collide with, they are immune to this collision-complaint.

More about cusps and chaos: Two asteroids whose orbits (in a rotating reference frame with Sun and Jupiter both "stationary") feature cusps (or at least something very near to being a cusp) are 887 Alinda and 499 Venusia. Both are known to have highly chaotic orbits; and Venusia's chaos is long-lived, perhaps eternal. Alinda is in 3:1 resonance with Jupiter; while Venusia with 3:2 is a prototypical Hilda asteroid. (I've also previously mentioned the Neptune Trojan 195495 and the Jupiter Trojan 82055 as having cusps; I do not know whether their orbits are chaotic, but in the case of 82055 would guess "yes.") Alinda (diam.=4.2 km) may ultimately pose collision danger to Earth; e.g. on 8 January 2025 it passed within 0.0822 AU. If you wanted to try to **save Earth** by deflecting Alinda with a nuclear bomb, then probably the best detonation time would be when Alinda was at or near its orbit-cusp since that presumably is the moment its orbit would be most sensitive to perturbation; and presumably the greater the timespan before the predicted Alinda-Earth collision, the easier this would be (provided you had good enough data and prediction ability). The plot of Venusia's orbit is taken from Jorba, Nicolas, Rodriguez 2024 (shown with unreachable Hill region for its Jacobi constant J≈3.0280 in gray); and apparently its librations remain trapped within the blue region forever. JN&R also plot the orbits of various other Hilda asteroids in the same fashion. All of them are roughly triangular in shape with corners near L3, L4, L5. This permanent trapping is *not* forced by the Jacobi constant alone, since that by itself would not prevent Venusia from escaping the solar system via the narrow passage near Jupiter, L1, and L2 between the boundaries of Hill's horseshoe-shaped excluded region. Rather, it is *dynamical*, caused by the 3:2 resonance keeping Venusia's three orbit-cusps always near L3, L4, and L5. Because L3 is (in Poincare's terminology) a "homoclinic point," we expect suitable orbits having cusps at repeated visits near L3, to contain a "Smale horseshoe" in their dynamics and hence to be chaotic. But Wisdom, in his 1987 Urey prize lecture, contends based on his numerical integration surveys that Hilda orbits in the restricted circular and noncircular *planar* 3-body problem are *not* chaotic. If Wisdom's claim is correct, then, since most (all?) Hilda orbits *are* chaotic, the reason is either their *non*planarity and/or perturbations from planets other than Jupiter, probably acting in concert with the cusps (or near-cusps) near L3; and the alleged non-chaoticity in the planar case is somewhat miraculous.





(8) Fig.5 of Li, Zhou, Sun 2014 suggests that "Plutinos" (objects which, like Pluto, are in 3:2 resonance with Neptune) survive in a state of chaos for >4 Gyr, and perhaps forever, without ever escaping the resonance, from a set of initial conditions having large positive measure.

(9) Large computer surveys of big swaths of parameter space in a restricted circular 3-body problem were done by Zotos and collaborators in a number of papers starting in 2016. Their idea was to define a 2-parameter family of possible initial conditions, and for each member of that family attempt to determine the fate of the asteroid by numerical integration. "Fates" include (a) colliding with primary #1, (b) colliding with primary #2 [both regarded as small balls, not points], (c) escaping, or none of the above, just staying apparently-permanently bound in a state which could be either (d) chaotic, or (e) not, and possibly there could be several "orbit types," such as "moon of primary #1" or "#2" or "neither"; or prograde versus retrograde. After color-coding the fates and orbit types, the result was a multicolored map of that part of parameter space – which they printed. Many of Zotos' investigations involving mass-ratios ≤83 for the two primaries, yielded essentially no evidence for "confined chaos" – which is not surprising in view of Gladman 1993's proposed confined chaos mechanism which needed mass ratio>733 to work (at least, in its simplest form). Unfortunately in those papers Zotos & co. appeared unaware of Gladman, and for some reason did not try to simulate the Sun/Jupiter mass ratio≈1048. Also, "Trojans" seemed to be artificially excluded (?) from Zotos' maps. However, the widest-ranging *one* among Zotos' studies (Zotos 2020) – intended to shed light on "exoplanetary systems" – did find large amounts of apparent "confined chaos" (color code yellow) using mass ratios 10^4, 10^3, 10^2, and 4.3, especially at high initial asteroid eccentricities. (Confined chaos seems more likely if the asteroid is *not* confined to the primary-plane, but rather allowed to travel in 3-space.)

Another blow to the "ergodic argument" against eternal 3-body chaos by Nauenberg 2007. Even if you accepted the ergodic argument for expulsion, you'd face the problem that the naively expected "escape time" ought to be not much longer than *one* Lyapunov time T_L. But we have seen many examples of survival much longer than that. Analogy: The radioactive isotope Mn-56 (halflife=2.5785 hours) was tested by Norman et al 1988 for a timespan ranging from 0.3 to 45 halflives after creation, finding no evidence against the well-known "exponential law of radioactive decay." But if you found even a *single atom* of Mn-56 that you knew had survived for ≥1 month, you would consider that a clear refutation of the exponential decay law! We've seen examples of 3-body survival up to $\approx 10^9$ Lyapunov times! So: should we regard the ergodic argument as clearly refuted? A defender could *counter-argue* that perhaps in 3-body scenarios featuring large mass-ratios X, or large interbody distance-ratios X, we might need not one, but rather order X^k Lyapunov times to get a decently-large escape probability, where presumably 0.1<k<10. None of our long-surviving chaos examples from nature refuted *that* possibility. At best they refuted it for k<2.5, with 3≤k<10 remaining untested.

However: Nauenberg 2007's artificial examples (example #5 above) of 3-body chaos that apparently *never* causes either escape or close 2-body approaches refute that counterargument! Because for them, all three masses are *equal*, and all distance ratios have order *one*. With Nauenberg's 3-body scenarios, the full range 0.1<k<10 is experimentally refuted.

> Nauenberg 2007's wording unfortunately made it unclear whether he claimed eternal chaos, *or* claimed the chaos ends after some positive amount of time has passed, yielding eternally-confined nonchaotic quasiperiodic end-states. However, that won't matter for the purposes of this paper. For us, the important things are that, (1) the chaos persists for a positive amount of time, that can be made arbitrarily long by starting near-enough to the linearly-unstable P_{12} orbit; (2) the end-state depends sensitively on the initial conditions. and, as far as Nauenberg's computations could tell: (3) no body ever escapes to ∞, (4) no pair ever closely approach.

So I am unaware of any reason why, if the ergodic argument is valid, the escape time should not be of the same order as the Lyapunov time *in Nauenberg 2007's class* of 3-body scenarios. But according to Nauenberg's experiments, it isn't. That disagreement makes me think **ergodicity does not happen in Newtonian 3-body problem chaos, and therefore "eternally confined chaos" is possible**.

By considering arbitrarily small perturbations of the Euler 3-collinear-masses example, or for that matter any of Moeckel & Montgomery's infinite set of topologically-distinct unstable periodic 3-body solutions, we see that even if the expulsion conjecture is true, the wait before expulsion can in principle be made **arbitrarily long**. Also, Lyapunov times (at least initially) can be made arbitrarily long by simply taking initial conditions close enough to the boundary with a non-chaotic region.

My view: relation to Turing. I believe that resolving the "eternally confined chaos" controversy is an **important mathematical open question** about the 3-body (or N-body, for moderate finite N) problem. And it bears a considerable resemblance to Turing's undecidable "**halting problems**." I.e: you can prove a Turing machine halts, if you wait long enough to see it happen. But if you do not see halting happen, then maybe it will halt some time in the future, or maybe never – you often do not know which, and it can be Turing-undecidable which. You can prove some 3-body problem expels a body, if you simulate it long enough and accurately enough to see the expulsion happen (e.g. waiting until it passes some simple test, such as "body A is ≥10 radii outside a ball containing the other two and centered at their center of mass, and is moving outward with speed≥1.1 times escape velocity"). But if you do not see expulsion happen, then maybe it will expel a body at some future time, or maybe never – you often do not know which, and it might be very difficult to tell which.

This is more than a mere analogy. In fact, if the participants in this whole debate had been aware of my 1993 proof of the Turing-unsimulability of the planar Newtonian N-body problem for moderate finite N, then they would have known that such questions as "will body A escape from body B (ultimately reaching unboundedly large separations)?" in general are **beyond the algorithmic power of Turing machines**.

"Einstein/billiard cheat" (altered-Newton laws) arguments that would settle the whole question. Consider some 3-body system which some claim exhibits eternally-confined chaos immune to small perturbations of the Hamiltonian. But others claim it will eventually expel one of the bodies. Which?

Place this system in orbit around some much heavier faraway fourth mass. Suppose the total mass M of the 3-body system is such that even if Mc^2 energy were supplied to one of the 3 bodies, that could not be anywhere near enough to make it achieve escape velocity from that ultraheavy fourth object (although far more than enough to expel one of the three bodies from the other two). The "Einstein cheat" is: We modify Newton's laws so that if two bodies get closer than some constant times their Schwarzschild radii, then the inverse-square attraction is replaced by something milder which allows at most mc^2 potential energy to be converted to kinetic. As the simplest example, assume an elastic "bounce" like for hard ideal billiard balls; or some short-range repulsion, e.g. a potential energy function qualitatively like $\Phi(r)=r^{-3}-r^{-1}$ or another different possibility would be $\varphi(r)=-\min(r^{-1},10^8)$. No collisions will ever occur (in the sense that point masses have zero probability to ever collide).

Now whenever the claimed expulsion happens, then thanks to the ultraheavy 4th object, it will not really be an expulsion. All 3 bodies still will be orbiting it as a tightly bound pair plus an individual; and their two orbit-ellipses will intersect. Therefore, either (a) they will at some future time re-interact, in which case it is reasonable to claim "permanent chaos" or (b) they never will, the two orbits will remain separated and almost non-interacting forever, in which case it is reasonable to claim "permanent non-chaos" for the 4-body system. In the limit of very heavy, very faraway, 4th body, I claim (b) cannot happen. I claim the net effect then is similar to ideal frictionless lossless billiard balls permanently bouncing around on some compact-domain billard table: eternal chaos (Sinai 1970).

In short, I am claiming that *if* both arbitrarily-far and arbitrarily-close body-approaches were artificially prevented by, e.g, adding rigid walls with lossless bounces, or modified interbody potentials $\Phi(r)$, with ultraheavy faraway 4th body – *then* permanent chaos obviously would generically happen. And note that the modification $\Phi(r)$ of the Newtonian r^{-1} potential can occur *only* at *arbitrarily small* interbody separations r. In short:

MY CLAIM: Eternally-confined chaos really does happen for an Einsteinian modification of Newtonian 4-body systems 3 of whose masses are bounded between two positive constants, provided Newton's r^{-1} interbody potentials for those three masses are modified for all sufficiently-small r (say $0\le r<\varepsilon$) to prevent them from reaching arbitrarily-negative values. This claim should be valid for any fixed $\varepsilon>0$, no matter how small.

4. New kinds of cosmic censorship counterexamples

You will need to be familiar with this section to read the next one.

"The cosmic censorship hypothesis introduced by Penrose thirty years ago is still one of the most important open questions in classical general relativity."
– Shahar Hod 1999.
"The question of cosmic censorship remains very much an open one at the present time – possibly the most important unsolved problem in classical GR."
– Roger Penrose 1999.

First construction: a naked point singularity for 1 instant of time. Let us assume that near-Schwarzschild black holes are available with any desired mass, and you can have as many as you want, provided

1. the sum of their masses (including kinetic energies for holes moving relative to your reference frame) adds up to a finite amount, e.g. ≤100 solar masses in total,
2. no two holes of masses M and m, ever get closer than 200(M+m) distance apart if M, m, and distance are measured in "Planck units" with G=c=1; and among any pair of holes, neither *ever* has kinetic energy exceeding 1% of its rest mass (as regarded by the other); this way none of their geometries ever gets distorted much away from Schwarzschild and the dynamics never gets distorted substantially away from Newton's laws.

Concerning the fact that it is mathematically possible to construct "many black hole Einstein initial data" of this kind, see the review by Corvino 2025.

So imagine we have an infinite sequence of black holes centered at points on the X-axis in 3-space of mass=$3^{-J}50$ solar masses for the J^{th} hole, J=0,1,2,3,... The J^{th} hole is located at $X=10^7 2^{2-J}$ kilometers. In that case the point at X=0 (which is the **limit point** of hole-center locations) is a "**naked singularity**":

- It is singular since any limit point of singularities must itself be a singular point. [Incidentally, a theorem in complex analysis states that any limit point Z of poles of an analytic function F(z) also must be a limit point of an infinite sequence $z_n \to Z$ such that $\lim_{n\to\infty} F(z_n)=C$ (valid for *every* complex constant C); see Conway 1978 exercise 15 p.111 in §V.1.]
- It is naked since not inside any event horizon. Indeed, the escape velocity from the naked singularity (reckoned in the Newtonian approximation) is only about 1000 km/sec≈c/300. You can verify that by computing the Newtonian potential energy Φ per unit mass for a test mass:
$$-\Phi = (1/4)\ (G / 10^7\ \text{km}))\ (50\ \text{solar masses}) \sum_{J\ge0} (2/3)^J.$$
whereupon $V_{esc}=|2\Phi|^{1/2}$.

Similarly, each hole has escape velocity from this whole configuration of order c/300 or less.

Discussion. Assuming suitable holes are available, what prevents us from assembling this configuration? Well, **quantum objections:**

1. "Quantum gravity" suggests it is not possible for black holes to exist with mass below roughly 21 micrograms ("Planck mass").
2. Currently, only a finite set of elementary particles are known that have nonzero masses, the lightest being the electron (mass=9.11×10^{-28} grams) and the three neutrinos (unknown masses, but probably all above 10^{-37} grams). And I have good reason to believe there are only a finite set of elementary particles. So (1) and (2) make it difficult to construct a black hole lighter than the lightest massive elementary particle.
3. Also, even if you can acquire them, Heisenberg-style uncertainty principles would make it difficult to localize the tiny holes exactly where you want them to be.

But since throughout this whole *mathematical* exercise we are assuming *non*quantum laws of physics, and *never* using any matter particles at all (according to Einstein GR, black holes of arbitrary mass can be created from gravity waves in pure vacuum): I don't care about objections 1-3. If the holes are located near enough to where I want them to be in $\mathbb{R}^3$ (I don't demand exactness) i.e. with all coordinate additive errors ≤10% of the X-coordinate specification, and their kinetic energies all are ≤10% of their rest masses, that will suffice. (Some might even regard those accuracy demands as weak enough that they would qualify as "generic" initial data.) Furthermore, I consider it obvious that near-Newtonian [indeed, near-straight-line, e.g. with the n^{th} body ultimately moving with asymptotic velocity perpendicular to the X-axis with angle=2πn/φ where 1/φ=2/(1+√5)≈0.61803] trajectories exist for the holes, that separate them such that any two particular holes become arbitrarily far apart, increasing monotonically and asymptotically linearly with time, if you go far enough into either the past or future; throughout this movement the "total tide" remains bounded so the Newtonian solution keeps existing; and every hole's speed always remains well sub-relativistic in a geometry which always is pretty nearly flat Minkowski spacetime except in regions within O(1) Schwarzschild radii away from each hole.

Math rigor-mortis objection. While I believe that a solution of the Einstein vacuum field equations corresponding to the above scenario *exists* eternally (arbitarily far into both the past and future; and if desired with a repulsive cosmical constant Λ) – and certainly it is obvious a Newton solution exists – unfortunately in the Einstein case humanity, embarrassingly, does not currently know how to *prove* that. And (for example) despite a large number of black-hole collision and near-collision events simulated at a cost of $millions, e.g. as part of the LIGO project, they were never able to prove any sort of validity claim about those simulations. And, even more pathetically, they did not and could not prove even the *existence* of any Einstein solution during their simulation timespan.

Thermodynamic Objection: "Thermal noise" would prevent precise localization of the holes at any given positive temperature. Specifying this countably-infinite set of locations and momenta this accurately requires an infinite number of bits of "entropy," and hence assuring the desired placement accuracy (in the presence of thermal noise trying to stop you) is not thermodynamically achievable at any given positive temperature without your refrigerators expelling an infinite amount of heat energy.

But that is no problem if we are allowed to assume absolute zero temperature.

My riposte to those objections: Actually, the very *existence* of "Minkowski flat spacetime" (or any "asymptotically Minkowski" spacetime) is incompatible with any assumption of any "fixed positive temperature" since then the Minkowski flat spacetime would be filled with a constant-density blackbody radiation bath at that temperature, with infinite total mass, which is not permitted by general relativity (which demands Minkowski space, since flat, must contain only vacuum). However, it *is* permitted in "expanding-universe FLRW cosmological models" instead of "Minkowski spacetime," and those automatically asymptotically redshift their blackbody radiation *approaching* zero temperature when t→∞. Indeed, for *de Sitter* background (assumes a repulsive Λ), which agrees much better with supernova observations than Minkowski, that approach is exponential.

I suspect anybody who cares about Penrose's cosmic censorship conjecture who is ever going to raise that thermodynamic objection has assumed asymptotic-Minkowski-flatness at some point in their lives (certainly Penrose himself has). Indeed, consider these two quotes:

> **1.** "To formulate the statement of the weak cosmic censorship conjecture more precisely, we first need to make precise the notion of the "finiteness" of the resources available to our mad scientist. This notion is well modeled by restricting consideration to space-times containing an asymptotically flat initial data surface..."
> **2.** "Abstract: We review the status of the weak cosmic censorship conjecture, which asserts, in essence, that all singularities of gravitational collapse are hidden within black holes. Although little progress has been made toward a general proof (or disproof) of this conjecture, there has been some notable recent progress in the study of some examples and special cases related to the conjecture. These results support the view that naked singularities cannot arise generically."
> – Amazingly enough, both those quotes came from the *same* paper, Robert M. Wald: Gravitational Collapse and Cosmic Censorship (1997).

Therefore I'd have to ask: why are cosmic censorers allowed to assume asymptotic flatness whenever it is convenient for them, but when they instead find it convenient to raise this thermodynamic objection, suddenly that's forbidden?

I also suspect anybody who cares about Penrose's cosmic censorship conjecture has assumed a universe containing an infinite number of stars at some point in their lives, with infinite total mass, which seems a rather more radical assumption than my version with only finite total mass.

Furthermore, specifying essentially *any* GR spacetime metric initial conditions respectably precisely, requires specifying an infinite set of numbers within some respectable accuracy bounds, which sounds a lot like it would consume an infinite number of bits of entropy. (Same is true about initial conditions for Navier-Stokes hydrodynamics.) So: be careful with your objections.

Regular initial data? Black holes can be created from focused gravitational waves concentrating energy into a small region, causing a black hole to form; in this way we presumably could (if desired) make the initial data be "regular" without any black holes.

We now quote **Moncrief & Eardley 1981** in their attempt to produce clear statements of cosmic censorship conjectures as *mathematical existence claims*:

> "At least for some analytic and numerical examples, space-time admits a foliation by spacelike Cauchy hypersurfaces with these properties:
>
> i. the chosen hypersurfaces Σ_t have trace(K)=0 [here K denotes *extrinsic curvature* of the hypersurface]
> ii. the surfaces foliate the entire space-time *exteriors* of the black holes (but not the entire interior)
>
> If one reconstructs such a solution from its initial data by evolving the Einstein equations in the maximum slicing gauge, then one finds a solution exists globally in a natural sense – the entire future of the black hole exterior is mapped; the time function locks onto the proper time of distant stationary observers and runs forever. The presence of the singularity does not intrude upon the evolution because of the slicing chosen and because the singularity lies hidden inside a black hole. Indeed the very possibility of such a slicing *requires* that the singularity be so hidden, i.e. implies cosmic censorship for the solution.

Such examples suggest the following informally stated global existence conjecture:
C1: Every asymptotically flat initial data set with trace(K)=0 may be evolved to arbitrarily large times t in the slicing gauge defined by trace(K(t))=0 and by the requirements that t be normalized to a standard proper time at spatial infinity.
...C1 would in essence prove the cosmic censorship conjecture for asymptotically flat space times... But C1 is actually stronger than cosmic censorship; it also asserts that the maximal slicing is always good...
What about matter? As usual there are some vexing counterexamples to C1 involving dust or null fluid. In order not to be distracted by a welter of questions about matter, we restrict C1 to vacuum."

It should be clear we've refuted C1, and that this refutation is 100% rigorous. The rigor issues concerning existence are for this purpose not issues, since nonexistence also suffices to refute C1.

Second construction: any number≥0 (up to and including an infinite number) of naked point singularities which all persist for over 1 million years.

We now construct two *long-duration* naked singularities. I'll first explain an initial construction-attempt, then explain many possible modified designs.

1. Make 2 equal Schwarzschild black holes, say M_1=50 solar masses each, orbit each other with center-separation $L_1=10^7$ km. (In contrast, the Earth-Sun distance is 1.5×10^8 km.) Each hole's Schwarzschild radius then is 147.7 km, which is much smaller than this separation. Lifetime before merger should then exceed 10 Myr. Orbital period P_1 = 15.15 hours = 909 minutes = 54540 seconds.

2. Now make each black hole have an orbiting smaller satellite black hole with mass $M_2=M_1/5000=0.01$ solar masses, and orbital-radius $L_2=L_1/10=10^6$ km. Its lifetime before merger then is about 10 Myr. Approx. Orbital period for each satellite: P_2 = 40.6 minutes = 2440 seconds.

3. Now make each satellite black hole itself have an orbiting smaller satellite ("satsat") black hole with mass $M_3=M_2/5000\approx4\times10^{24}$ kg, and orbital-radius $L_3=L_2/752\approx1330$ km. Its lifetime before merger then is about 10 Myr. Orbital period for each satsat: $P_3\approx8.37$ seconds.

4. Now make each satsat black hole itself have an orbiting smaller satellite ("satsatsat") black hole with mass $M_4=M_3/5000\approx8\times10^{20}$ kg and orbital-radius $L_4=L_3/593\approx2.24$ km. Its lifetime before merger then is about 10 Myr. Orbital period for each satsat $P_4\approx41$ milliseconds.

5. And so on. The process continues forever with (from now on) $M_{n+1}=5000^{-1}M_n$ and $L_{n+1}\approx5000^{-3/4}L_n$ where $5000^{3/4}\approx594.6$ and $P_{n+1}\approx5000^{-5/8}P_n$ where $5000^{5/8}\approx205.0$. where note that by design, each satsat...sat, no matter how deep we go into the recursion, still has lifetime before merger (due to its orbit-decay) being approximately 10 million years. (To "play it safe" to make sure all the orbit lifetimes exceed 10 Myr perhaps we actually should make L_{n+1} slightly *exceed* $5000^{-3/4}L_n$, for example $L_{n+1}=L_n/593$.) Also notice that each satellite lies well inside the "Hill radius" of its "parent planet" (and even more so if we toggle the orbit direction clockwise↔anticlockwise each recursion level to take advantage of Innanen's retrograde/prograde insight).

Version involving K: Actually, we could have used $L_{n+1}\approx5000^{-K}L_n$ whereupon $P_{n+1}\approx5000^{(1-3K)/2}P_n$ and provided our constant K obeys 0.333<1/3<K<3/4=0.75, each orbit's decay-lifetime against gravitational wave-emission would grow exponentially with n so that orbit decay would not be an issue; and each satellite would lie within the Innanen-retrograde "Hill radius" of its "parent planet." (My original choice had been K=3/4.)

The "total tide" remains bounded if K<2/3=0.666..., assuring Newton-solution **existence**.

Planet collisions are not a problem we need to worry about in the present scenario since it has only *one* "planet" per "sun." However, just for fun suppose they *were* a possible problem, e.g. in some design-variant where we had *two* planets per sun and we'd foolishly set them up in orbits making collisions possible. Then with our original choice K=3/4 our 2D crude collision-time estimate would indicate expected lifetimes for the "solar systems" n-levels deep in the recursion *decreasing* exponentially with n. That disaster could be avoided by demanding 0.333<1/3<K<3/5=0.6, causing our 2D collision time estimate at level n then to behave like $\text{time}_n\approx5000^{(3-5K)n/2}$, i.e. now *growing* exponentially with n. This would cause the expected total number of collision events during the next million years to be well below 1. (The 3D collision time estimate then would also grow exponentially.)

We now have constructed a scenario with a countable infinity of black holes whose total mass is <101 solar masses (including both rest masses and their kinetic energies), all of whose motions are well described by Newton's laws since all speeds stay below 0.01c and all separations exceed Schwarzschild radii by a factor>6000 always, and which contains two moving naked singularity points which should both persist for at least a million years. The singularities are located at the limit-points of the two infinite sequences of black hole centerpoints, but never lie inside any event horizon, i.e. are "always naked." We can, if desired, make all the black hole centerpoints stay in a *plane* of mirror symmetry (everything remains bilaterally symmetric forever), or we can make it (at least slightly) *non*planar 3D; either should work for the present application.

Modifications. Suppose you are not satisfied with having only two naked singularity points. **You want more**. No problem: We can make arbitrarily many more. E.g. if we made each hole have not one, but rather *two* satellites orbiting it, then the recursion would be an "infinite binary **tree**" instead of a "path," and there would be an *infinite* number of limit points ("tree leafs") each of which would be a moving naked point-singularity. (The sum of the hole-masses, even for the infinite-tree scenario, still is quite small and finite; i.e. that series converges.)

Lagrange-triangle idea: The "two satellites" with masses much smaller than the mass of the "sun" actually could be in the *same* circular orbit 60° apart ("Lagrange point L_4"), which is a well known stable exact solution of the Newtonian 3-body problem by J.L.Lagrange. (Which is known to be modifiable at first post-Newtonian order to still be an exact solution, and still "linearly stable," see Ahmed et al 2006.)
Regular 7-gon idea: Make each "sun" be orbited by N≥7 equal-mass "planets" co-orbiting circularly and equispaced angularly. This has the advantage of much slower orbit decays caused by gravitational-wave-emission [the demands of keeping orbit lifetimes≳10 Myr at each stage were not onerous, but if you for some reason felt they were, then that problem can be hugely diminished in this way, in fact *exponentially*(N) diminished]; and now instead of an infinite binary tree, we get an infinite N-ary tree. Or, we could make some of the "suns" not have any "planets" but have a little extra mass to weigh the same as if they had their usual full infinite tree of descendants, thus keeping the dynamics "symmetrically balanced"; then we get a tree with each node either having N or 0 children.
"Moore8" idea: Instead of Lagrange's equilateral triangle solution, we also could base things on any other stable 3-body solution. For example, each "planet" could actually be a "Moore8" (see the Moore8 appendix), each of whose three bodies is seen (when viewed at higher magnification) to itself be a smaller Moore8, etc. That's an infinite ternary tree (or tree with each node having either 3 or 0 children). What's interesting about the Moore8 is that it has angular momentum zero, thus showing we can make each recursive level of the construction, and the whole thing, have zero angular

momentum (if desired).

Also, if you wanted not two persistent naked singularities, but only *one*, then omit half the original "path" construction, i.e. the original 50-solar-mass black hole binary is replaced by only a single 50-solar-mass black hole. It now should be obvious that any desired number 0,1,2,3,... of naked point-singularities, including infinity (if that is what you desire), is attainable in this way. We have plenty of "design freedom." Also, there is nothing special about "1 million years" persistence time. I arbitrarily chose that as my design goal (overdoing it by a factor≈10 for safety), but could easily have redone it using "1 billion years" instead, or any longer desired (but it must be finite) duration. There also is nothing tremendously special about the mass ratio "5000" I used at each recursive level. Let $\mathbf{Q_n}$ be the mass ratio at recursive level n. If you prefer $Q_n=1600\pi$ instead of 5000, then fine. If you actually do not want Q_n to be constant, but rather want it to keep increasing unboundedly as a function of n, for example $Q_n=3^{1/3}40(p_n)^{13/2}$ where p_n denotes the n^{th} prime, then also fine. The point of the latter formula is to make all the Q_n be rationally-independent algebraic irrationals which increase with n ultimately unboundedly faster than n^6, but slower than n^7.

In the original construction, the trajectories followed by each of our two naked singularity points spiritually resemble the plot of the following function:

$$F_{3/4}(t) = \sum_{n\geq 1} 5000^{-3n/4} \sin(5000^{5n/8}t) \quad \text{or more generally} \quad F_K(t) = \sum_{n\geq 1} 5000^{-Kn} \sin(5000^{(3K-1)n/2}t).$$

The $F_{3/4}$ sum converges (indeed with geometric rapidity) for all real t, and so does the sum representing its first derivative $F'_{3/4}(t)$. However, if $1/2<K\leq 3/4$, the sum representing the *second* derivative $F''_K(t)$ *diverges* exponentially. The point of this is that the trajectories followed by each of our two naked point-singularities are **non-analytic** curves. They are everywhere differentiable, i.e. each naked singularity always has well defined position and velocity. But these curves are nowhere second-differentiable, i.e. *never* have any well-defined acceleration. Indeed, this $F_{3/4}(t)$ is exactly the indefinite integral of a Weierstrass nowhere-differentiable continuous function.

If $3/7<K<1/2$ then F_K has a well-defined acceleration but not "jerk." By making $K>1/3$ *approach* 1/3 we can make the naked singularity's location have an *arbitrarily large* **but finite** number of well-defined time-derivatives.

If we were allowed to employ K=1/3 (the smallest K possibly permitted by Innanen's retrograde Hill region enclosure demand) then it would be infinitely-differentiable. However, I doubt the construction will work with K=1/3.

For almost the same reason, the Newtonian potential experienced by any particular "body" in our infinite collection – or at any particular point of $\mathbb{R}^3$ – will for any fixed K>1/3 have only a finite number of time-derivatives, causing the **motion of each body to be *nowhere analytic***. That contrasts drastically with the Cauchy-Kovalevskaya theorem's assurance that the motion of any Newtonian N-body system with N *finite* is analytic *everywhere* except at singularities where the potential |energy| of at least one body is infinite.

This also contrasts with some people's fond **fantasy** that general relativity outputs analytic solutions. With my persistent naked-singularity GR solutions, that fantasy is false everywhere always.

As far as I know, all previous claimed constructions of cosmic censorship counterexamples were instantaneous. I.e, I am unaware of any previous claimed construction of a naked singularity lasting any positive time duration. Also, none of them worked in pure vacuum – they all required some postulated kind of matter and/or radiation (often not a physically-available kind) and thus pertained to a physical theory necessarily larger than, and hence making more assumptions than, pure Einstein gravity. Also, my construction here is "more elementary" than any previous one I know of, i.e. can be comprehended without knowing much physics and math, no need to solve any partial differential equations, and without any need for, or dependence on, a computer.

Stability? A big possible objection, and in fact the only objection that causes me any worry, to the above 1 Myr "proof," is that we need all the orbits of the planets and satsats and satsatsats, etc. to stay stable-enough for 1 Myr. (We don't want it to self-destruct after only 3000 years.) The above argument implicitly assumed that happened. But does it?

Well, first of all, this construction is what astronomers call "hierarchical" and it is a well-accepted empirical finding in astronomy that hierarchical N-body systems are stable, at least for any *finite* N. (I am not claiming this remark is a "proof," but nevertheless it should convince most astronomers.)

If (fictionally) each planet at each recursive level were regarded as interacting *only* with its immediate "parent" and "children," then we by design could make its orbit be exactly Poincare-periodic with a permanently linearly-stable orbit (BHHH type), which indeed could be made (and *generically* this would automatically happen with probability=1, without any effort from us) KAM stable and d-Nekhoroshev stable for some finite d. Indeed, if the constant "5000" were large enough (and as designers, we are allowed to make it as large as we want, including nonconstant Q_n growing with n) – specifically instead of "5000" use $Q_n=3^{1/3}40(p_n)^{13/2}$, then this would be true even if we extended this to grandparents and grandchildren (or *any fixed finite number* ℓ of levels up or down in the recursion). That is because the grandchildren have far tinier masses (in the original construction 5000^2 times tinier, and the grandgrandchildren 5000^3 times tinier, etc, so they can be regarded as small perturbations, and hence KAM theory assures us everything can still be made fully stable. And similarly but going in the other direction, the parents, grandparents, etc may be treated by working in a rotating reference frame with slight perturbations representing Coriolis pseudoforces, etc. E.g, the local orbit of Pluto and Charon round each other, does not terribly care about the nonlocal fact that the huge Sun is out there making them both orbit it, or that the even huger Milky Way galaxy is out there making the whole solar system orbit *it*. The Pluto-Charon system regards itself, at first approximation, as falling freely. It regards the Sun and Galaxy (and galaxy-cluster, if we went up another level) merely as tiny and tinier (and tinier) perturbations of that model.

The trouble with all that is: our whole construction really has *infinitely* many recursive levels. To make this rigorous we really need some sort of infinite-dimensional KAM and/or Nekhoroshev theory. Finite-dimensional is not good enough.

However, let me argue nonrigorously that it *is* good enough: Observe that if C is a suitable constant (assumed for the moment to be independent of ℓ) the 2ℓ-Nekhoroshev escape-time guarantees $t_{esc}>\exp(C\varepsilon^{-1/(2\ell)})$ (here quoted in the planar case with ℓ+1 bodies, fixed center of mass and fixed total energy, so 2ℓ degrees of freedom) all seem boundable below by a positive constant (e.g. 1 Myr) *no matter how large ℓ gets* since the perturbation size ε is by design exponentially(ℓ) small. This strongly suggests to me that the whole thing *can* be made stable enough for long enough. Also, no matter how large ℓ gets, KAM guarantees *existence* of an infinite number of *exactly*-periodic solutions staying close to the design, any one of which would suffice to reach ℓ recursive levels deep (for *any* finite ℓ) with *infinite* Newtonian persistence time.

And if Nekhoroshev's C is *not* independent of ℓ, e.g. shrinks with ℓ (imperiling the above argument?), that is ok because we can simply increase the Q_n above 5000 by whatever amounts we need to compensate for that, making Nekhoroshev's ε effectively as small as we want at each level of the recursion. The fact that

"K" obeys the bounds 1/3<K<3/5 keeps the orbit wave-decay times, Hill region-inclusion, total-tide existence criterion, and crude collision time estimates (if we cared about them) ok; for that purpose it does not matter how rapidly we make Q_n grow.

Finally, an **additional objection** is that Newton's laws are *not* exactly the Einsteinian laws governing our motions. Indeed, the whole system is not really a "Hamiltonian system" at all, in the sense that it loses energy by radiating gravitational waves and thus exhibits "friction." That is why the whole construction has been designed to make Newton's errors get relatively exponentially smaller as a function of recursion level $n\to\infty$, and to keep the cumulative gravitational wave energy losses (by design) smaller than the orbit energies for 1 Myr, and indeed if we want we can make them stay exponentially(n) smaller. Thus the construction *asymptotically behaves like* a Newtonian Hamiltonian system, to within exponentially small error, i.e. the "friction" becomes exponentially small.

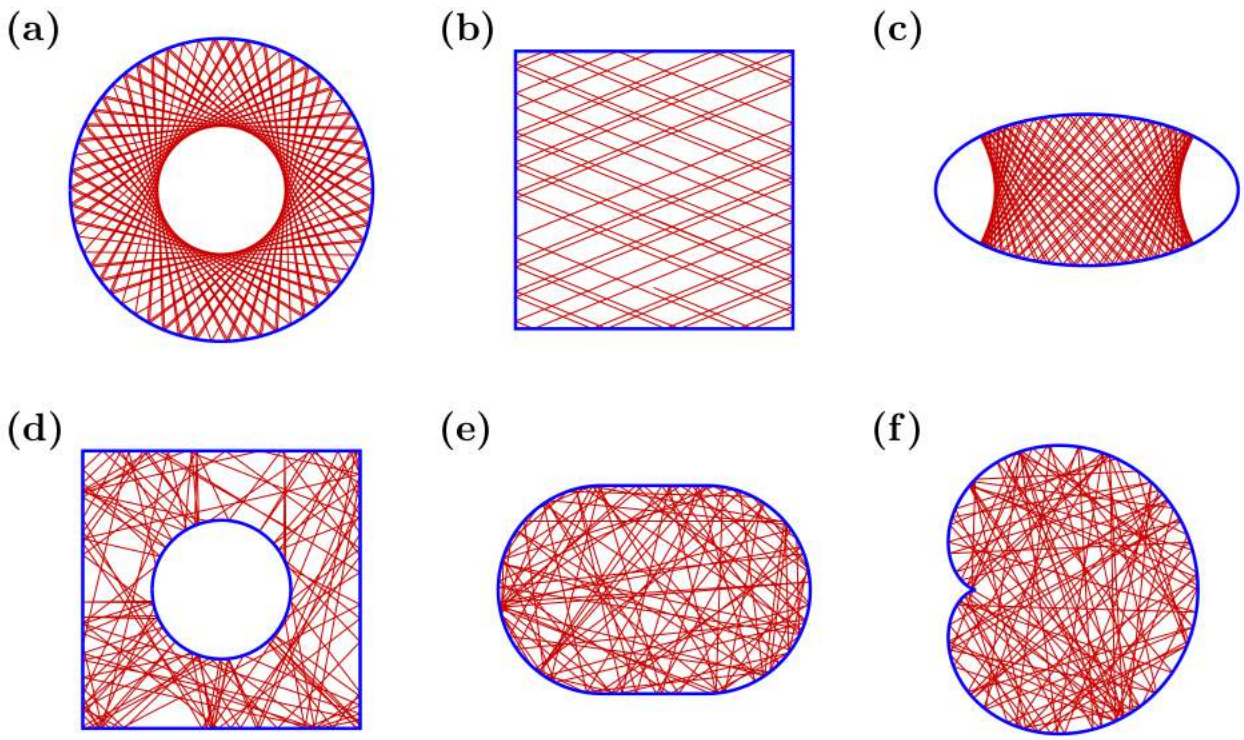
Conclusion. I believe my 1 Myr argument was *not* a completely rigorous math proof and will not be at least until suitable infinite-dimensional KAM theorems become available. But I also believe that, for the present, this has been convincing enough for physicists. (Or lawyers: "proof beyond a reasonable doubt.") I simply see no objection that seems "reasonable." Penrose's cosmic censorship conjecture now should be regarded by theoretical physicists as a mathematical assertion about the Einstein vacuum field equations that simply is false: Naked singularities inside Einstein's vacuum equations can persist a million years if the Einstein equations genuinely are evolvable in scenarios of this kind. (And if they are *not* thus-evolvable, that represents a far-huger failure of Einstein gravity as a physics theory, and also ought to produce a CCC counterexample anyhow since known Cauchy existence theorems show that whatever stops the time-evolution in the horizon-external region must be a naked singularity. I.e, Einstein is known to be evolvable for some positive timespan if the initial data is well behaved and smooth enough.)

And my weaker first proof (providing a naked singularity for only 1 instant, without asking for 1 Myr persistence time), I consider 100% rigorous.

5. Proof sketch for Main Theorem: Turing-Unsimulability of Einstein vacuum

Before beginning, let me point out (since some people are confused about this): Chaos by itself is *not* necessarily an obstacle to Turing simulability. If the Turing machine has access to arbitrarily precise initial data, and carries enough decimals, then it can handle exponential error growth with only "polynomial slowdown" by just carrying a linearly-growing number of decimal places and using numerical integration techniques that provide accurate-enough error guarantees. This was demonstrated in my Newton-N-body/Church paper for integrating "improved-Newton laws" of motion.

> Analogous problem suitable for students: consider frictionless planar "**billiards**" with a single mass-point "billiard ball" bouncing off the edges of a **polygonal "billiard table."** The laws of motion are time-reversible, and the motion is readily predicted by computer and never chaotic (Boldrighini et al. 1978), causing it to be easy to back-deduce initial conditions to high accuracy by "running it backward." For almost all polygons billiard trajectories are ergodic (Kerckhoff et al. 1986), but for polygons with all angles rational (in degrees), e.g. a rectangle or any regular N-gon, the motion is never ergodic because only a finite set of directions is accessible (also true in any triangle, even if its angles can be irrational). But if we replace some polygon edges by **circular arcs** (Bunimovich 1979, Sinai 1970), then the motion usually becomes chaotic and a computer using single-precision arithmetic to simulate forward in time, quickly becomes unable to deduce accurate initial conditions via a backward run. [Pictures taken from Habilitation thesis of Arnd Bäcker at TU Dresden. All billiard table shapes conserve energy. In case (a) angular momentum also is preserved; in case (b) both |components| of linear momentum; and in case (c) the product of the angular momenta about the two ellipse-foci. The circle-in-square case (d), square-pasted-to-two-semicircles case (e), and cardioid (f) all yield full chaos.] However, that obstacle can be overcome by using *multi*precision arithmetic carrying enough decimal places (the number of decimal places needed grows proportionally to the timespan simulated). Although such multiprecision runs are much slower, the slowdown factor is "polynomial," i.e. not large enough to hurt the extended Church-Turing thesis.



For different billiards 100 successive reflections of one orbit are shown. The regular dynamics for the billiard in the (a) circle, (b) square and (c) ellipse is in contrast to chaotic dynamics for the (d) Sinai billiard, (e) stadium billiard and (f) cardioid billiard.

What can, and usually will, prevent Turing simulability is **something *worse* than mere chaotic *exponential(t)* error growth: factor-*infinity* growth of infinitesimal errors in *finite* time**. My proof will argue that exactly that pathology can be made to happen in a scenario resembling our above "persistent naked singularity" recursive construction, but adding more bells and whistles, with the goal of "speeding up" chaos an infinite number of times.

Plan of the proof: A recursive setup like the preceding section's, but at each recursive level we, by design, set up something *chaotic* with some moderate finite number N of extra black holes, resembling our solar system. In the preceding section, each recursive level's N-body orbit-scenario (for some small finite N) had intentionally been designed to be *stable*, e.g. so we could try to use Nekhoroshev estimates to assure long survival for the whole persistent naked singularity construction. We now instead intentionally are making each recursive level's N-body orbit scenario be *chaotic*. Ideally, it would be "stably eternally-confined chaos." Unfortunately for us, whether that exists is presently an open mathematical question. I suspect it does exist and have putative examples; if they are valid our proof gets better and simpler. Fortunately, we shall not need eternal, nor even very long, survival times: for the present section's purposes, estimates like "Lecar's law" suffice, and even much weaker claims would suffice.

> If the "eternally confined chaos" conjecture is correct, life seems easy. If not, then we need the chaos to stay confined for *long enough to do its job* – i.e. merely assuring survival for a large-enough **constant×log(Q_n) number of Lyapunov times** at the n^{th} level of the recursion would suffice, in the 1 Myr naked singularity proof structure above, most simply with Q_n a large-enough constant. For example we would have Q_n≈19410 if we wanted to make each level resemble Sun+Neptune+Pluto, in view of the Sun/Neptune≈19410 and Neptune/Pluto≈7000 mass ratios, in which case we merely need survival of the chaos for a **constant number of Lyapunov times**. Since Lecar's empirical law estimating survival time for Gladman's putative-eternally-confined-chaos 3-body scenarios gives us *far* more than merely that (plus Lecar himself claims/admitted his law was erroneously too small in these scenarios), I do not think it is reasonable to dispute this. (After it has done its job of surviving at least that many Lyapunov e-folding times, the recursive subsystem can blow itself to hell with expulsions and/or collisions and I will not care, since even if its total mass-energy $E=Mc^2$ were somehow entirely dedicated to ruining a recursive level nearer to the tree-root, we can, by choosing our constants right, assure that would not be enough.)

Then at the next level, we have a (scaled down) miniaturized solar system. Then at the next level, an even smaller one, and so on forever. In the original naked-singularity construction each "solar system" had only N=1 "planet," but we now need N≥2 to have chaos – and N=2 should suffice, albeit I have no objection to moderately larger N, e.g. N=7, if that yields better performance. Each successive "solar system" runs faster than its parent, i.e. over 200× shorter orbital period in the original naked singularity construction, and we also can make it have 200× shorter Lyapunov times. I.e. we can make each solar system's Lyapunov time be upper-bounded by a constant number O(Q_n) of its orbital periods. (Vrbik 2013's analysis backs this up.) Each child system perturbs its parent by acting as a small chaotic "noise source" perturbing its parent's dynamics, which then amplify such noise exponentially with time (because that is what chaos does) until it causes large effects (approaching full scale). Since this perturbation is only a constant factor smaller than the whole parent amplitude, after only a constant number of parent orbits, the parent configuration will get fully randomized versus what would have happened without any noise-feed coming from the child system. And then this happens for the parent's parent too, etc, all the way up the infinite chain of recursions, in a total timespan which (by summing a convergent geometric series) amounts to only a constant number of orbit-periods for the topmost largest and slowest system.

Then the behavior of the outermost system (e.g. made of 50-solar-mass black holes) is continually affected in a major way by the tiniest grand-grand-...-grandchild system, which consists of infinitesimally small black holes – and this all happens in only finite total time, comparable to a constant number of orbital periods of the outermost system. *Every* subsystem, no matter how far down the recursion chain it is, affects the macroscopic top level in a major way, so the simulator cannot afford to disregard (or not know enough about) any of them.

So what we have here, is an ***infinite* simulation task**, to simulate our physical system's time-evolution for only a *finite* timespan. Furthermore, we presumably can make all this happen in a *plane* of bilateral-symmetry, so that trajectory *topology* has meaning (i.e. if a planet passes to somebody's left, that is topologically different from it passing to their right). If so, we then can arrange for an infinite set of possible *topologically-distinct* trajectory time-evolutions to happen, in only finite total time. That would also demonstrate unsimulability but in a different way (now based on *output* size).

Now maybe you suspect this simulation was not really an "infinite task," because some amazing simulation algorithm nobody thought of yet can handle it absurdly faster than any obvious kind of simulation algorithm. Wrong! Because *any* simulation must **read the input**; my argument makes it clear that step is not optional; and just the input-reading portion of the algorithm – forget the rest – from any finite number of input tapes (or even countably-infinite number of input tapes, provided we can only read from a bounded-cardinality subset of them each Turing machine timestep) using any finite alphabet (I don't care what encoding scheme you use) necessarily will take infinite time to read enough to guarantee a sufficiently good finite outermost-system accuracy spec in a simulation of a finite-duration time-evolution.

Like the previous construction, the present construction would still work – *if* we could ignore the possibility of "planet collisions" – if arbitrary sufficiently-small perturbations are made to its initial data, where here "data" means the black-hole masses, positions and velocities; and "sufficiently small" for *masses* means relative error; for *positions* means relative to the minimum of the distances from that black hole centerpoint to (1) the naked singularity point and to (2) its parent and (3) children; and for *velocities* means relative error as regarded by the parent's reference frame.

However, we now probably *do* **object to "planet collisions,"** both actual and near-miss. That *contrasts* to our prior naked singularity ">construction, where since N=1 each stage collisions are not a concern. (Or even if we had wanted N>1 then we could set it up right to make it obvious that collisions could not occur, since in that argument there was no need for chaos or large trajectory-topology counts, so just use obviously-stable "solar systems" each recursive stage.) And unfortunately our above crude collision-time estimates (both in the same plane, and in 3-space) indicate exponentially-increasing rates of collision as we go down the recursive chain to smaller and smaller "solar systems." Therefore the set of initial data that works, will be much smaller, of near-zero measure. Nevertheless it should be nonempty, as may be seen by the same sort of sensitivity analyses in my old Newton/Church paper. (The exponential amplification property of chaos actually *helps* you to show *existence* of a suitable initial dataset via "fine tuning.") I'll try to give at least some hint of what I mean by that in the subsection about "proof without chaos."

But we can dodge that whole worry via the different, simpler, **"lazy man's approach to collision-avoidance"**: instead of making $L_{n+1} \approx L_n/593$, make L_n shrink considerably more slowly than 593^{-n}, for example like 50^{-n}; and then instead of $P_{n+1} \approx P_n/205$, we would have P_n shrinking like 5^{-n}. Since 5×50=250<5000 (as opposed to 593×205=121565>5000), the crude within-plane collision-time estimates now are exponentially *increasing* with n so we expect *well below one* collisions in a million years, and happily should not need any careful fine-tuning of the initial data – pretty much any initial data usually should work to yield an unsimulable and collision-free Einstein problem. (Furthermore, even if a few black-hole collisions *did* happen during the million years, then I don't see why that hurts us: if we simply demand a constant factor more outermost-level accuracy from our Einstein-simulator, then we still ought to have an unsimulable task.)

This "lazy man's approach" suggests to me that not only is Einstein GR unsimulable for worst case input, in fact it *typically* is unsimulable, at least for random input scenarios of this construction's general ilk.

Now suppose you do not like black holes. You want **initial data without any black holes**. That desire should be satisfiable because it is known that focused gravitational wave pulses can concentrate enough energy into a small-enough region to create a black hole from pure vacuum. We begin with smooth bilaterally-symmetric initial data representing suitable incoming gravitational wave pulses (they come in from above and below the symmetry-plane to create holes centered on, and thereafter permanently moving only on, that plane) with no event horizons anywhere, and indeed no time-dilation more severe than a constant anywhere. These pulses start at distances of order M away from the plane, when generating a mass-M hole. For that reason, the limit-point of the hole-center locations should still be a naked singularity even before any holes actually form. (Well, considering holes form within time<ε after simulation-start, valid for *every* ε>0, my word "before" was misleading. The point I was trying to make was: the initial dataset we here are proposing has no black holes, but does have one naked point-singularity.) But aside from the one naked one, there is no singularity anywhere in the initial data; and it has no event horizon anywhere.

Objections to the mathematical **rigor** of the above constructions may be handled by my usual "4-case cheat" trick. Also, it seems to me that more important than producing "a fully-complete proof," is: producing an argument that is convincing enough to make physicists believe it. I think I have achieved *that*.

My proof sketch for Turing-unsimulability is now complete.

Can we prove unsimulability without relying on any claims about Newton-N-Body "chaos"? Design each recursive "solar system" to follow one of a small finite number (≥2) of topologically-different possible histories, depending both on initial data, and on which one its smaller-predecessor followed. The summed time for all systems to do their things is upper bounded by the finite sum of a geometric series. The net result is an **uncountably infinite** number of topologically distinct trajectories followed by the black holes during a finite timespan, which clearly is more than can be simulated by a Turing machine in any finite amount of time (if the Turing machine were demanded to understand the topology). Each successively-smaller system's design-parameters are constrained by the specs its larger parent system needs into some small, but positive-measure, set. We need to prove existence of suitable parameters by following that chain forever. Initial data corresponding to such a set then is Turing-unsimulable with the outermost (largest) solar-system's behavior after a finite timespan being unpredictable by any Turing machine in any finite amount of simulation time.

6. Open problems

I. Prove or disprove: the Newtonian N-body problem for some or all N≥3, can exhibit permanently-confined chaos arising from a set of initial conditons with positive measure. Proving this also will refute the last remaining possible remnant of the ergodicity conjecture and Fermi 1923's false "quasi-ergodicity theorem." Ideally, you'd also prove all pairwise distances permanently stayed bounded between two positive constants. If you cannot prove permanent chaos, then at least prove something resembling the "Lecar law" empirical lower-estimate for 3-body chaos survival time.

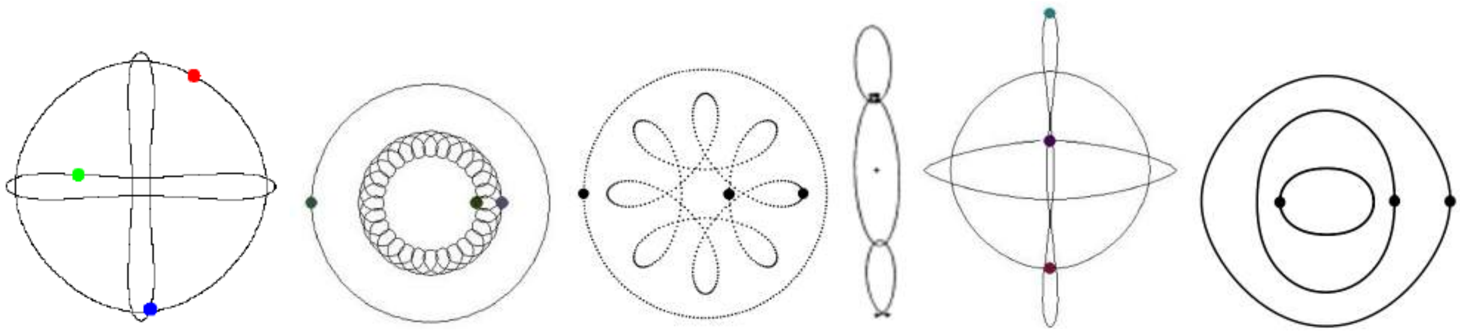
II. Usefully extend KAM theory to countably-infinite-dimensional Hamiltonian systems of ODEs.

III. The Kepler-Newton 2-body problem is almost entirely soluble just by taking advantage of the known constants of the motion. But that is not the case for any N-body problem with N≥3 in either the plane or 3-space, because there are too many degrees of freedom versus too few constants of the motion. This suggested to writers beginning with Poincare that the 3-body problem could not be solved in closed form (and some statements of that ilk have been proven). However, there are rigid-rotator N-body, and Lagrange's elliptic 3-body family, of solutions expressible in closed form for N≥3, because the rigid-rotation or Lagrange fixed-shape demand reduces the number of degrees of freedom drastically. Prove or disprove: The "Moore8" 3-body choreographic solution, and/or some of the noncircular Sitnikov solutions, can be expressed in some reasonably-closed form.

IV. *Prove* nonexistence of 12:7 resonant orbits (and/or nonexistence for other such ratios) in the circular planar (or elliptic?) restricted 3-body problem, as asserted with only numerical pseudo-proof by Tsiganis, Varvoglis, Hadjidemetriou 2002. The methods of Mather 1986 might enable such proofs, but nobody ever did it.

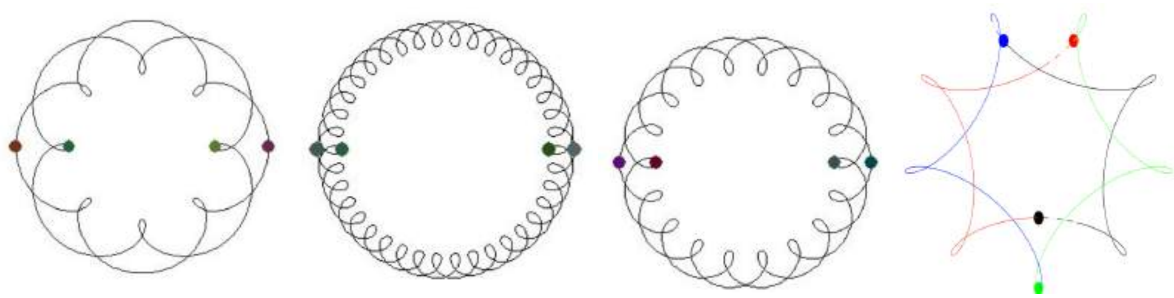
V. As I pointed out, my persistent CCC counterexamples and unsimulability constructions yield spacetime pseudometrics which are *non-analytic* almost everywhere. So suppose we added to the axioms of Einstein general relativity, the additional axiom that the spacetime metric must be **analytic** everywhere (except perhaps on a countable set). Would some such additional axiom suffice to prevent persistent CCC counterexamples and restore Turing simulability? Would it yield a logical contradiction?

7. Appendix about BHHH planar periodic (or Poincare-periodic) 3-body (and some 4-body) orbits (includes own references)



Above: Six examples of BHHH 3-body periodic orbits. The first three are "retrograde" and the 4th is prograde. The first four involve 3 equal masses and are stable. I don't want to give you the utterly false impression that it is important that all the masses be equal. That was merely for simplicity of description. Small arbitrary perturbations of these masses will yield similar-looking periodic orbits; and indeed achieving stability seems to be easier if the three masses are, say, 1, 30, 900 versus if they are near-equal. The last two pictures involve all-unequal masses. Of those, the first is stable, but the stability of the second (masses 10,5,1) has not been assessed. All these figures were drawn by other people including Moore, Henon, Ouyang/Xie, Vanderbei, and Chen.

Below: The first 3 examples are 4-body "double double-star" planar orbits from the "always parallelogram" class. The last three are 4-body choreographies. All four involve 4 equal masses and are stable and periodic.



By BHHH I refer to Roger A. Broucke, John D. Hadjidemitriou, Michel Henon, and (about 100 years earlier) George W. Hill, all of whom, working largely independently, wrote a large number of works about the class of 3-body planar periodic (or Poincare-periodic) Newtonian solutions that may be described informally as either "tight binary star plus faraway planet that orbits their center of mass" or "planet and moon which orbit a faraway star"; and with these two 2-body orbits either prograde or retrograde. I.e. the 3-body motion approximately "factors" into two, usually nearly-noninteracting, 2-body motions, in the latter case earth-moon and sun-EM, where "EM" is a fictitious body located at the center of mass of the earth and moon, with their summed mass. Initial conditions for this kind of motion may always be written in a form in which the center of mass remains fixed at (0,0), all 3 initial locations lie on the X-axis, and all three initial velocities are orthogonal to it (two point up and one down). BHHH proved the existence of continuum-infinities of these solutions, described by analytically-smooth parameterized families, which for some 1-parameter families include arcs eith non-empty interior containing only linearly-stable orbits. If we permit Poincare-periodic solutions, then we can get families with at least one additional continuously-variable parameter, which in some cases can be proved "linearized stable."

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8. Appendix about Moore's "Eight," Marchal-Nauenberg "P_{12} family," and the related "Ouyang-Xie pentagram" (includes own references)

Lagrange 1772's rigidly-rotating equilateral triangle 3-equal-mass 3-body solution is a special case of a more general kind of periodic motion called a "**choreography**," in which all the bodies move along *one* fixed closed curve. Cris Moore discovered the first post-Lagrange example in 1993: the "**figure-8** choreography" (animation by Moore).

Moore found it approximately with computer aid, but did not prove it really *existed* and was *collision-free*; that was done by Chenciner & Montgomery 2000. Moore's three equal-mass bodies move along a particular fixed figure-8 shaped curve in the plane, never colliding, and equispaced in time. Their summed angular momentum equals zero. Simo 2002 computed Moore8 initial conditions and period to very high accuracy. Everybody conjectures, but nobody has proved, the *uniqueness* (up to scalings and rotations) of the Moore8. Kapela & Zgliczynski 2003 with rigorous computer-aided numerics proved it "**locally unique**": within a ball of radius 10^{-6} of initial-conditions, the only one that works yields a unique Moore8 orbit. I suspect that stronger uniqueness claims could be proved in a similar way but with larger computations. Montgomery 2005 was able to prove uniqueness for the three-body problem with an attractive r^{-2} potential (not Newton's r^{-1}) modulo symmetries. That happened in a remarkable way: the dynamics of Montgomery's bounded zero-angular-momentum solutions is equivalent to a geodesic flow on the thrice-punctured "shape sphere." The punctures are the binary collisions. The metric generating the geodesics is the "Jacobi-Maupertuis metric." When the three masses are equal this metric has negative curvature everywhere except at two points corresponding to Lagrange-style equilateral-triangle solutions. (This negativity only happens with three *equal* masses.) A corollary (corr.2.2) of this negativity is the *uniqueness* of the r^{-2} figure-eight up to rotations and scaling, among the zero-angular-momentum equal-mass solutions in its homotopy class. (This solution also happens to enjoy *constant* moment of inertia.) Montgomery also finds a complete symbolic dynamics for encoding the collision-free solutions, and proves that collision solutions are dense within the bound solutions. (Furthermore, every other 3-equal-mass zero-angular-momentum choreography, if any, also is unique in its homotopy class.) Can the dynamics of *Newton*-gravity 3-body motions *also* be regarded as some geodesic flow for some metric? According to McCord, Meyer, Offin 2003, the answer is "no" for Newtonian 3-body motions in 3-space, but "yes" in the plane *provided* the total energy is positive and the angular momentum zero, otherwise "no." (Since the Moore8 is a bound configuration, its total energy is *negative*, so "no.") Fujiwara & Montgomery 2003 gave an analytic proof that the Eight's two loops each are *convex*. They also pointed out on p.272 that the Eight's symmetry group has order 12 and is isomorphic to the dihedral group of the regular hexagon. No closed form expression is known for the figure-8 shaped curve, albeit if it does have one then I suspect it involves elliptic functions. And indeed Fujiwara, Fukuda, Ozaki 2003 did find a closed form involving elliptic functions for the choreographic figure-8-shaped zero-angular-momentum motion of 3 unit-mass bodies in the XY plane:

$$X(t) = sn(t) / (1+cn(t)^2), \quad Y(t) = sn(t)cn(t) / (1+cn(t)^2); \quad (X^2+Y^2)^2=X^2-Y^2$$

where here the "modulus" k of the sn and cn Jacobian elliptic functions obeys $k^2=(2+\sqrt{3})/4\approx0.93301$, the period of the motion is $4K(k)\approx11.07225258$ where K(k) denotes the complete elliptic integral of the first kind, and this curve is a "Lemniscate of Bernoulli" with arc-length=$(2\pi)^{-1/2}\Gamma(1/4)^2\approx5.2441151$, enclosed area=1, width=2, and height=$2^{-1/2}\approx0.7071$. This curve is the locus of points with product of distances to the two "foci" equalling 1/2, where these foci are at Y=0, $X^2=1/2$. Its two lobes each are convex. However, FF&O's lemniscate is *not* the Moore8, because FF&O's motion is under the pair-potential $V_{FFO}(r)=\ln(r)/2-r^2 3^{1/2}/24$, which is *not* $V_{Newton}=-k/r$. Their $V_{FFO}(r)$ actually increases from $-\infty$ at r=0 to a maximum value of $[\ln(12)-2]/8\approx0.069613$ at $r=12^{1/4}\approx1.86121$, then falls. However, this potential-maximum cannot affect the dynamics since plainly the maximum pairwise distance between any two of the bodies is upper bounded by $5.2441151/3\approx1.74804$. Within the domain that matters, namely roughly $0.68\le r\le1.2$, the approximation $V_{FFO}(r)\approx0.259-0.33/r$ is accurate to within ±5% of full range. It is unknown whether the Moore8 is an algebraic or transcendental curve, but probably the latter because C.Simo claims to have shown by computer fit-attempts that it is not of the form P(X,Y)=0 for any polynomial P with deg(P)≤12. The Moore8 seems stable against small-enough perturbations in the initial conditions (including slight changes in the three masses, away from all-equal) in the senses that similar-looking orbit behavior empirically results.

Otto F. Dziobek's planarity theorem: Any 3-body solution featuring zero total angular momentum, necessarily is planar. (Page 63 of Dziobek 1892.)

Toshiaki Fujiwara's three-tangents theorem: For any 3-body solution featuring zero total linear and angular momentum, the three instantaneous tangent lines to the three trajectories are coincident, i.e. all three intersect in the same (time-dependent) point or are parallel.

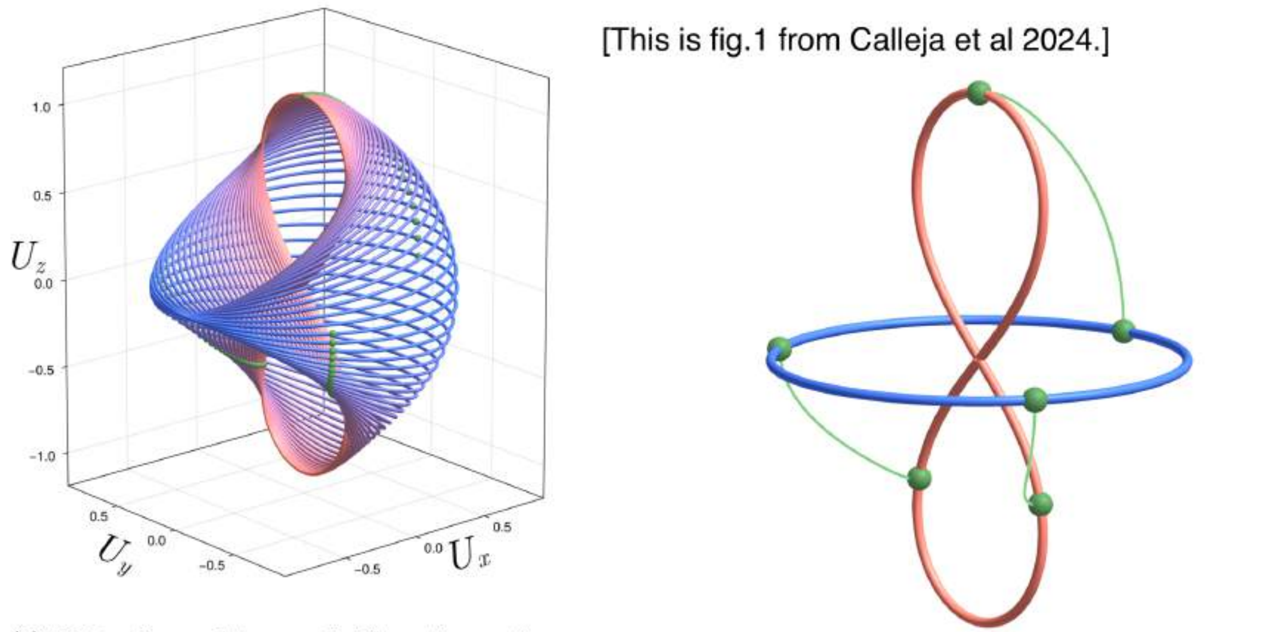
The Moore8 and the Lagrange triangle are the two simplest 3-body choreographies, and the only two known in 1993. This led to the question: are Moore's and Lagrange's choreographies somehow connected? The answer: Yes!

We quote Calleja et al 2024: "Shortly after the announcement of the Eight by Chenciner & Montgomery, Marchal 2000 published a paper investigating the discrete symmetries of 3-body choreographies. In particular, recalling that non-circular periodic solutions of the equal-mass three-body problem have at most 12 space-time symmetries, he studied the properties of the most symmetric family of relative periodic orbits bifurcating from Lagrange's equilateral triangle by continuation with respect to the angular frequency Ω of a rotating frame. By a 'relative periodic orbit,' we mean a solution which is periodic in an appropriate co-rotating frame. Marchal referred to this most symmetric continuation class as the P_{12} family, and in the same reference showed that:

1. The P_{12} family is *non-empty*: there exists a family of relative periodic orbits bifurcating from the Lagrange triangle and having the maximal allowed 12 symmetries.
2. The P_{12} family comprises **relative choreographies**: in a suitable *rotating coordinate system*, each periodic orbit in the P_{12} family can be viewed as a *single* closed curve in $\mathbb{R}^3$. The three bodies follow one another around this curve, spaced by a time shift of ⅓-period.
3. Variational properties: the P_{12} family *minimizes the action* between appropriate terminal conditions.

Next, Marchal observed that Moore's three-body figure-8 evinces all 12 symmetries of the P_{12} family. Inspired by this, he conjectured that P_{12} actually *contains* the Eight. We prove this conjecture."

[This is fig.1 from Calleja et al 2024.]



(a) Thirty-three orbits sampled from the continuous branch of solutions proven to exist by Calleja et al 2024. Each curve is a relative periodic choreography of the three body problem with equal masses.

(b) The Lagrange triangle (blue) and the figure eight (red). Each of these choreographies is planar, and the P_{12} family – comprised of relative choreographies – is transverse to both planes.

Marchal's P12 relative choreographies connecting the Lagrange triangle to the figure eight. A dense set of the relative choreographies in the family corresponds to choreographies in inertial frame.

The resulting (according to Calleja et al 2024) **"P_{12} theorem"** is: The P_{12} family of relative-choreography orbit solutions of the 3D Newtonian 3-body (equal masses) problem, at angular frequency Ω=1 is a Lagrange equilaterial triangle 3-equal-masses choreography, and at Ω=0 is the Moore8, and when 0<Ω<1 is some hybrid of the two. Each orbit depends *analytically* on time t (modulo 2π) and the angular frequency Ω. The period *in the rotating frame* remains fixed as angular frequency Ω is varied from 0 to 1.

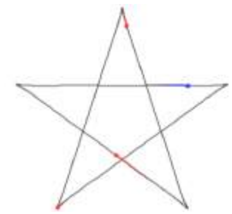
But in fact, as I pointed out to Calleja et al by April 2025 email, they were unable to prove the uniqueness of the Moore8. Therefore the claim in their abstract that the P_{12} endpoint *was* "the" Moore8, was in fact never shown. But I also pointed out to them that the Kapela-Zgliczynski local uniqueness theorem could probably be used, if they did some additional work, to justify Calleja's abstract's claim in the sense that the P_{12} endpoint could be shown to be identical to Kapela & Zgliczynski's figure-8 orbit.

Over the course of one period, the Moore8 has Lyapunov factors exp(iθ) for the two twist angles θ∈{107.313310441, 3.03218} degrees. According to linear stability analyses by Chenciner, Fejoz, Montgomery 2005, P_{12} orbits become unstable (in the sense that exponentially growing infinitesimal perturbations exist) for *all* Ω>0. However, §IV of Nauenberg 2007 claims that small finite perturbations empirically stop growing, stay small and bounded forever, and yield a permanent "complicated quasiperiodic stable state" (whatever that meant – he never defined any of those words) for all P_{12} orbits with 0<Ω≦0.1. If Nauenberg is right about that, then this provides another example of 3-body "confined chaos." More generally, both C-F-M and Nauenberg 2007 investigated what happens to the Moore8 when spun about *any* of its three axes of symmetry. The other two axes also yield (presumably analytically-smooth) continuum families Γ_2 and Γ_3 of periodic 3-body solutions in $\mathbb{R}^3$. Both C-F-M and Nauenberg 2007 agree that the Γ_2 family orbits are stable for 0≤Ω≦0.0920, while the Γ_3 family orbits are stable for 0≤Ω≤0.44, the latter (for linear stability) being proven by Kapela & Simo 2011, see their fig.7.

Meanwhile, Doedel et al 2003 (with computer aid in an apparently non-rigorous, but pretty convincing, manner) considered the Moore8, featuring three equal masses, say $M_1=M_2=M_3=1$. They then **(a)** continuously varied the mass M_1 while preserving the sum $M_1+M_2+M_3=3$ until reaching a *restricted* 3-body solution, i.e. with $M_1 \to 0^+$, in such a way that **(b)** the 3-body solution *stayed periodic*. I.e, apparently there exists a continuous path (in mass 3-tuple space) of periodic solutions from the figure-8 orbit to a periodic solution of the restricted 3-body problem. Some such path (if it exists) must be analytically smooth. Doedel et al did not say whether all the members of this path lacked exponentially-growing infinitesimal perturbations.

Roberts 2007 and Kapela & Simo 2007 independently proved the "linear stability" of the Moore8 with perturbations restricted to the plane, i.e. it has no exponentially-growing class of infinitesimal perturbations.

Kapela & Simo 2011 prove KAM stability of *both* the Moore8 regarded as a 2-degree-of-freedom Hamiltonian system [in plane with center of mass fixed at (0,0), and total energy fixed too, and angular momentum=0] *and* two selected orbits of so-called "rotating Eights" (Broucke et al 2006; these also are planar, but now have fixed *non*zero angular momenta) using computer-assisted rigorous numerical methods to compute the "twists." (Kapela & Simo also give rigorous bound-windows for initial conditions for the Eight on p.14.) This KAM stability also is true even for *three*-dimensional perturbations (provided they preserve the energy and the zero angular momentum) of the Moore8, because of Dziobek's planarity theorem. However, the linear-instability of P_{12} orbits for all angular velocities $\Omega>0$, no matter how small, proves the Moore8 is *not* KAM stable in $\mathbb{R}^3$ if we permit perturbations causing *non*zero angular momentum.



Computations by Imai et al 2007 indicated that a Moore8-like choreographic solution still exists even with relativistically-corrected versions of Newton's laws, in particular the Einstein-Infeld-Hoffman laws. Imai et al's point was: If we are in a near-Newtonian regime, then by slightly perturbing the Newtonian Moore8 initial conditions we can restore the exact periodicity, with post-Newtonian laws of motion, of what otherwise would have been a "close miss" yielding, in general, a non-periodic orbit.

Post-Moore, hundreds of N-body "choreographies" in which all N bodies (N≥3) move along one fixed closed curve, were found by other authors (e.g. Vanderbei 2004). But the vast majority are unstable to arbitrarily-small exponentially-growing perturbations and hence cannot be expected to occur in Nature. Exceptions include some linearly and empirically stable 4-body choreographies found by Ouyang & Xie 2015, such as their remarkable "**pentagram**" shown at right. Its edges are nearly, but not exactly, straight; the 4 masses are equal and travel along the same path equispaced in time; the masses may be regarded as two "binary stars" with each binary-star always consisting of one lying on edge n and the other on edge n±2 (numbered mod 5) of the 5 successive "edges" of the pentagram. Ouyang & Xie 2018 showed the pentagram is merely one member of an analytically-parameterized family (as masses vary) of 4-body orbits, of which all the members whose parameters are close enough to the Pentagram's are linearly stable.

Such linear- and KAM-stability claims make it possible that the Moore8 or Ouyang-Xie pentagram might occur in Nature. But computer experiments by Douglas C. Heggie and Piet Hut in 2000 suggest 1-100 probably exist in the observable part of the universe, i.e. probably nobody will ever observe one.

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Appendix: Survey of Billiards

"Mathematical billiards" is the study of the trajectories (aka "orbits") of a particle bouncing elastically in the plane off fixed rigid bounding curves that define the "billiard table." This obeys Newton's laws of motion with zero gravity – and Newton's equal-angle law of elastic reflection: if the bounding curve is differentiable at the location of a bounce, then angle of incidence equals angle of reflection. By convention, if the particle hits a non-differentiable point, e.g. corner, of a boundary, its trajectory ends. We demand the bounding curves be differentiable everywhere except perhaps at a finite or countable set of points, so that trajectory-stoppage is a zero-measure phenomenon. **Phase space** for planar billiards is (since speed by definition is constant) the set of *angular directions* (ℝ mod 2π) cartesian-producted with the set of positions (i.e. the interior of the billiard table). Often one focuses just some 1-dimensional subset of positions which keep getting revisited an infinite number of times (e.g. distance along the *midline* of the billard table) in which case that reduced phase space is 2-dimensional, allowing nice pictures to be drawn.

Our purpose here is to survey known results and open questions in this area. Although billiards is not directly useful for this paper's purposes, it is simple enough that people can prove things about it, thus gaining insight into the nature of chaos, ergodicity, and Hamiltonian dynamical systems. In particular: while the existence of regions of "confined chaos" is an open question for the Newtonian N-body problem, it is a solved question – it exists – for simply-connected compact planar billiard tables.

Extensions: One could also consider (≥3)-dimensional billiards, nonEuclidean geometries, geodesics on a 2-dimensional Riemannian manifold, and/or adding gravity or other forces. Examples:

1. A particle bouncing losslessly inside a *circle* in the plane in the presence of a "flat Earth" constant gravity field (parabolic trajectories between bounces) empirically exhibits "chaos"; but if bouncing inside an upward-open *parabola* seems non-chaotic.
2. In 1898, Jacques Hadamard published an influential study of the chaotic motion of a free particle gliding frictionlessly on a manifold of constant negative curvature, called "Hadamard's billiards." Hadamard was able to show that *all* geodesics are unstable, i.e. all particle trajectories diverge exponentially from one another, with a positive Lyapunov exponent. This is, in fact, true provided the Riemann curvature scalar is everywhere bounded below a negative constant.
3. The path of the tiny-mass asteroid in the planar restricted circular 3-body problem, in the "stationary Sun & Jupiter" rotating coordinate system, can *approximate* a billiards problem, with "bouncing" off Hill's algebraic curves of zero-velocity. Near-polygonal-trajectory examples: Hilda-asteroid orbits (3:2 resonance with Jupiter) resemble triangles. Asteroids in 4:3 resonance with Jupiter (the only prominent example is 279 Thule) have a near-trapezoidal orbit, for a certain squarelike trapezoid. Asteroids in 5:4 resonance with Jupiter (no examples known, but theoretically possible) have an orbit resembling a regular pentagon.
4. The trajectory of a particle bouncing back and forth along a diameter of a sphere, will be *stable* in any number (including ∞) of dimensions, in the sense that the Lyapunov distance (product of billiard velocity and Lyapunov time) is infinite: small perturbations grow at most *linearly* with time, while zero-angular-momentum perturbations stay *bounded*.
5. Presumably some version of Einstein GR *enhanced* by adding magic billiard-boundaries that black holes bounce off (or billiards enhanced by adding gravitional interations between billiard balls) would, rather trivially, be unsimulable.

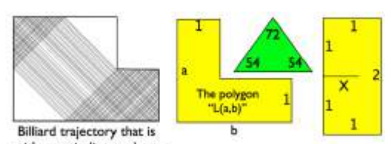
Polygonal billiard tables. If all the angles of the polygon are rational (measured in degrees) then we call it a "rational-angled polygon." Generically, however, polygons are not rational. A triangle is of "type (a,b,c)" if its angles are proportional to (a,b,c). Example: type (1,1,1) is equilateral.

A trajectory is **periodic** if closed and never hits a corner. It is **infinite** if not closed and never hits a corner. An infinite trajectory is **dense** if it approaches each point of the billiard table arbitrarily closely. A dense trajectory is **uniformly distributed** if the fraction of time (for the 0<time<T fragment of the trajectory) it spends inside any triangle that is a subset of the billard table, is (in the limit T→∞) proportional to that triangle's area.

For the following rational-angled polygons, *every* corner-avoiding trajectory is *either* periodic or uniformly distributed:

1. Triangles of type (1,1,n), (2,n,n) and (2,n,n+2) for n≥1; (1,2,n) for odd integer n≥5; (2,3,4), (3,4,5), (3,5,7), and (1,4,7). That list is exhaustive for non-obtuse and isoceles triangles. [C.T.McMullen.] Therefore any non-obtuse or isoceles triangle *not* on this list, has at least one corner-avoiding trajectory which is neither periodic nor uniformly distributed.
2. Any regular polygon. [W.A.Veech.]
3. Any polygon that can be tiled by congruent squares (polygon vertices must be a subset of the square-tile vertices). [E.Gutkin.]
4. Any polygon that can be tiled by congruent triangles of type (3,2,1) or (2,1,1); polygon vertices must be a subset of the triangle-tile vertices.
5. L-shaped polygons of type L(a,b) where a=x+z√d and b=y+z√d for rational x,y,z with x+y=1 and integer d≥0. That list is the exhaustive set of (a,b). [C.T.McMullen.]

For the following rational-angled polygons, *every* corner-avoiding trajectory is *either* periodic or dense:



Billiard trajectory that is neither periodic nor dense

1. The 2×1 rectangle with a line-segment barrier X long extending inward from the midpoint of one of the long sides. If X is rational this is a square-tileable polygon, see above theorem. If X is irrational and not approximated absurdly well by rationals (e.g. if X is any algebraic irrational) then there exist a continuum-infinite, but measure=0, set of dense but *not* uniformly distributed trajectories. [Cheung 2003.] These arise from a set of directions with Hausdorff dimension 1/2.
2. For the 54-54-72 isoceles triangle [Cheung, Hubert, Masur 2008; type (3,3,4)]:
    - For each direction, either all billiard trajectories are periodic or all are dense,
    - There exist directions in which billiard trajectories are dense but *not* uniformly distributed.

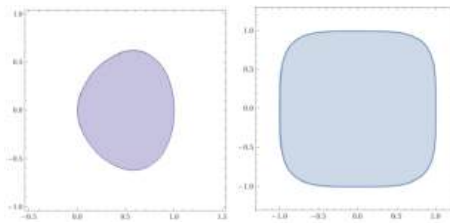
Periodic orbits? Open question: does every polygonal billiard table have at least one periodic orbit? (If it does, then that orbit automatically is non-chaotic, since chaotic trajectories do not exist on polygonal billiards tables.) The answer presumably is "yes," but this question remains open even for triangles. The answer was proven to be "yes" for the following subclasses of polygons:

1. Rational-angled polygons (Boshernitzian et al 1988); indeed, each rational-angled polygon has a *dense set* of periodic billiard paths.
2. Acute triangles (Fagnano 1775).
3. Right triangles (Holt 1993).
4. Triangles with angles A,B,C (in degrees) obeying max(A,B,C)≤100 (Schwartz 2009), improved to max(A,B,C)≤112.3 by Tokarsky, Garber, Marinov, Moore 2018.
5. Any isoceles triangle, and any small-enough angle-perturbation of an isoceles triangle (Hooper & Schwartz 2009).

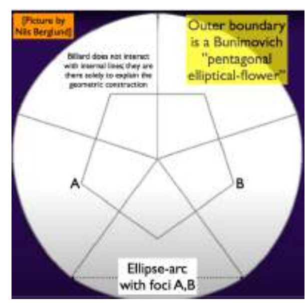
More about periods. The "Ivrii conjecture" states that periodic orbits on a smooth convex billiard table have measure=0. That has been proven for 2-bounce periods, 3-bounce periods, allegedly also 4-bounce periods, real-analytic billiard tables, and generic smooth convex tables.

Birkhoff's Theorem: C2-smooth convex-curve billiard tables always have at least two N-bounce periodic orbits for each N≥2. (Proof is variational involving maximizing or minimizing path-length.)

Chaos? A trajectory is "chaotic" if suitably infinitesimally perturbing its initial conditions yields eternal exponential growth of that perturbation with time. Its "Lyapunov distance" is the billiard-travel-distance needed to cause factor-e≈2.71828 perturbation growth.



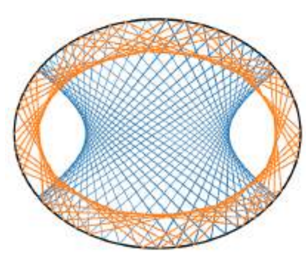
1. No trajectory in a polygon can be chaotic. (Gutkin & Haydn 1997.)
2. Any periodic billiard trajectory whose mirror-reflections are all either flat or concave (at least one concave) automatically is KAM stable (Arnold-Kozlov-Neistadt 2006) and if we are speaking of 2-dimensional billiards therefore automatically fully stable.
3. Trajectories in a circle preserve angular momentum relative to the circle-center, and in an ellipse preserve the product of the angular momenta relative to the two foci; neither are never chaotic. The Birkhoff-Poritsky conjecture asserts that ellipses are the *only* C2-smooth convex curves which entirely lack chaotic trajectories – every non-ellipse such curve has a positive-measure set of initial conditions yielding chaos. Bialy & Mironov 2008 proved that conjecture for *centrally-symmetric* C2-smooth strictly convex planar billiards tables.
4. Open Problem: Construct a *strictly*-convex C1-smooth billiard table in which almost all starting conditions yield chaos.
5. Open Problem: Construct a convex C2-smooth billiard table in which almost all starting conditions yield chaos. Bunimovich suspects C2 billiards tables that are C3 except at a finite set of exceptional points always have KAM-stable periodic orbits, and therefore could not be quasi-ergodic and also could not be chaotic from a full-measure set of initial conditions.
6. Two presumably "typical" smooth examples (pictured at right): The billaird table $x^4+y^4≤1$ (bounded by a real-analytic algebraic curve) empirically has a positive-measure set of chaotic trajectories, but also a positive-measure set of non-chaotic ones e.g. arising from KAM tori surrounding KAM-stable periodic orbits, which prevent quasi-ergodicity. See Jeng & Knill 1996. That same two-faced behavior appears also to occur for the finite component (with x≥0) of billiard table $y^2≤x-x^3$ (bounded by an elliptic curve). Both these curves are drawn in figures at right. Based on those sorts of experiences, it has been conjectured that almost all curved billiard table shapes have chaotic orbits.
7. All **strictly convex sufficiently smooth** (C7 suffices, and according to a 1982 Univ. of Paris PhD thesis by R.Douady, C6 suffices, and he conjectures C4 suffices) billiards tables contain "caustics" (i.e. curve to which a trajectory is repeatedly tangent) and hence are not quasi-ergodic; and there exist KAM-stable (and hence in view of 2-dimensionality fully stable) periodic orbits (Lazutkin 1973) and hence it is not possible for a full-measure subset of initial conditions to yield chaos.
8. By connecting "Bunimovich mushroom" billiard-table shapes one can obtain dynamics featuring an arbitrary (finite or infinite) number of "KAM islands" coexisting with an arbitrary (finite or infinite) number of chaotic components, all with positive measures in phase space (Bunimovich 2001).
9. For N∈{5,6,7}, the N-gonal "elliptic flower" is a simply-connected planar billard table with the same symmetry group as a regular N-gon, whose boundary consists of N identical inward-curving ellipse-arcs (joined at corners), with the foci of each arc being two non-adjacent vertices of a regular N-gon (separated by not one, but rather two, polygon edges), Bunimovich 2022. There is exactly one continuously adjustable parameter for this kind of elliptic flower: the size of the flower relative to the size of the N-gon. Elliptic flowers contain 3 different kinds of trajectories:



    (+) trajectories which permanently travel anticlockwise around the N-gon.
    (-) trajectories which permanently travel clockwise around the N-gon.
    (0) trajectories which pass through the N-gon.

    By appropiate choice of the parameter, these trajectories can be made each to have positive measures and be either chaotic or non-chaotic. In particular whenever the flower is large-enough compared to the N-gon (in the limit the flower approaches circular shape), all three trajectory types will be chaotic positive-measure sets; and since they cannot interconvert, we get three regions of "confined chaos" (and therefore at least 3 "ergodic components"). Prior constructions also exhibiting such confinement regions: Bunimovich & DelMagno 2009, Bunimovich 2001, and appendix C of Wojtkowski 1986.

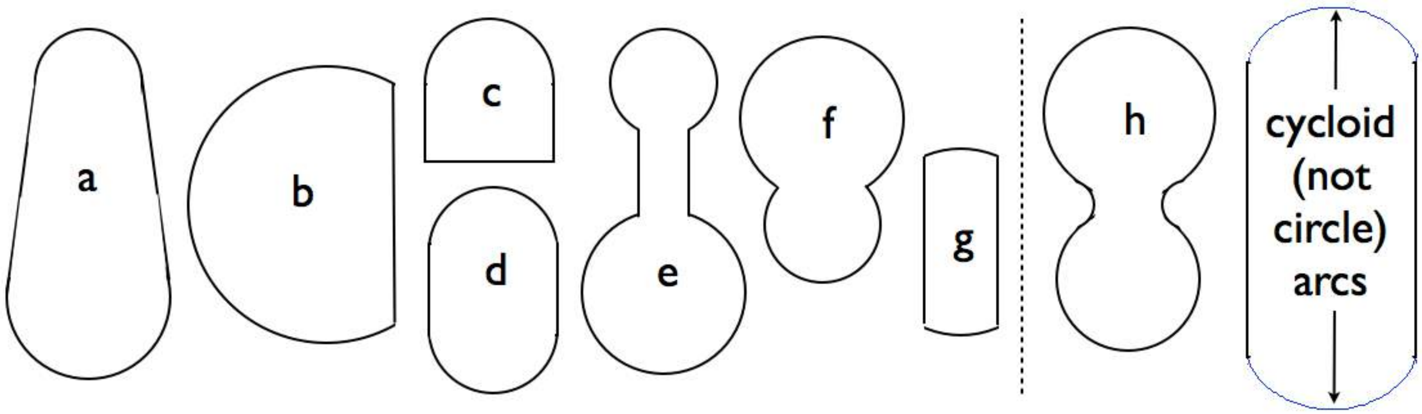
Ergodicity? If almost every initial condition (position & velocity) for the particle yields a trajectory which passes arbitrarily near every point of the billiard table, and with direction arbitrarily near any desired direction, then we call that billiard table "quasi-ergodic." If the amount of time the trajectory spends within area A in directions between angles α and β is proportional to |β-α|A, then it is "ergodic."



1. Rational-angled polygons are never quasi-ergodic because any trajectory moves in only a finite set of directions. Nevertheless, almost every initial condition yields a dense trajectory (Boldrighini et al 1978).
2. Ellipses are never quasi-ergodic because any trajectory always leaves one or two positive-measure connected open subsets of the ellipse unexplored. (There are two distinct unconvertible types of trajectory within an ellipse, neither chaotic, and both leaving chunks of the ellipse unexplored; exemplars are shown in different colors in the picture at right.)
3. Generically, polygonal billiard tables are ergodic. If the polygon's angles (in degrees) are irrational but possess a certain super-exponentially fast-converging infinite family of simultaneous rational approximants, then that polygon is ergodic (Vorobets 1997), which enables constructing explicit examples.
4. Every convex planar curve of constant width (e.g. the Reuleaux triangle and Rabinowitz 1997's algebraic curve) has a rotational invariant curve of 2-periodic orbits and hence is never quasi-ergodic.
5. Any billiard table such that *almost all* (i.e. all but a measure-zero set of) trajectories are chaotic, automatically has a finite

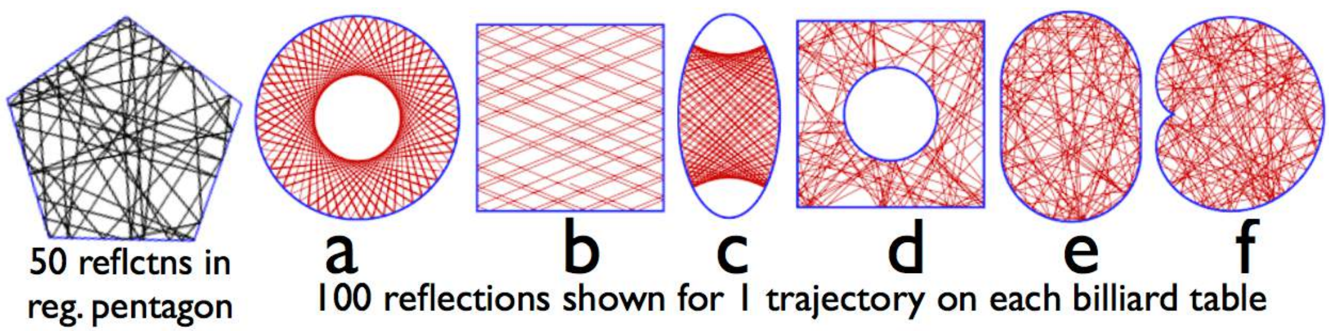
or countably-infinite number of "ergodic components" each with positive measures. For an ergodic table, the number of ergodic components equals 1, and all trajectories have *equal* Lyapunov distances.

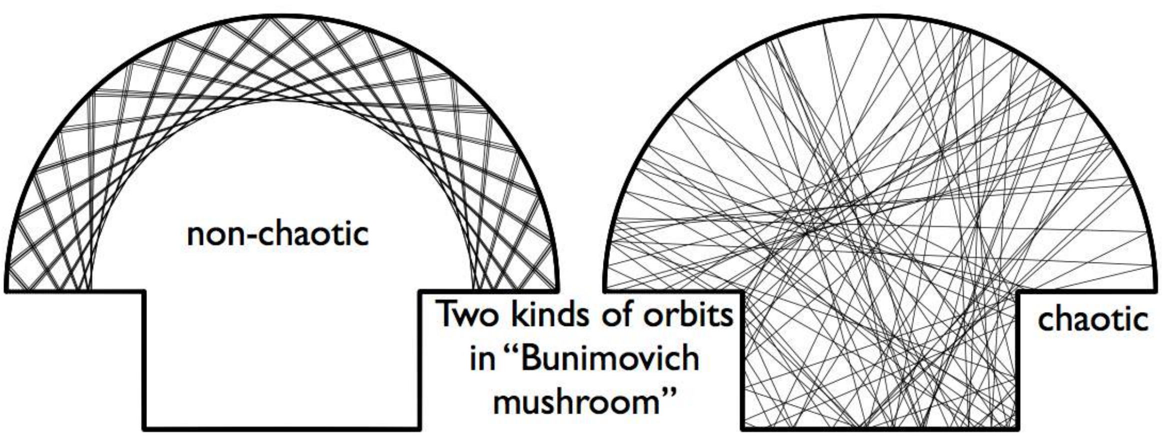
6. Many specific kinds of billiards tables are provably both ergodic and chaotic, including "Bunimovich stadia" (the noncircular convex hull of two circles, Bunimovich 1979) and all the kinds of shapes a-g shown below. (All shapes made from circular arcs and line segments only, except the rightmost which is two cycloids glued to a rectangle. Shapes bcefg have corners, rest are C1-smooth. Shapes efh nonconvex, rest convex. Shape g is a circle cut by two chords. The two shapes to the right of the dashed line were proven by Wojtkowski 1986 to be chaotic; I presume they also are ergodic, but Wojtkowski did not prove that.)



Note that convex Bunimovich stadiums that are arbitrarily small perturbations of a circle, are chaotic and ergodic, even though the circle itself has *zero* chaotic or dense trajectories. Also ergodic: "Sinai billiards" (regions bounded by line-segments and curves that always bend outward; e.g. a circular hole inside a square, or the astroid region $x^{2/3}+y^{2/3}\le 1$; Sinai 1970). Also chaotic: cardioids (Wojtkowski 1986). Bunimovich's dumbbell shapes (e in fig. above; Wojtkowski 1986 has more general dumbbells, e.g. h without any corners) rather resemble some Earth-Moon "Hill regions."

In the figure below (a-f taken from Habilitation thesis of Arnd Bäcker at TU Dresden), shapes abc and the regular pentagon contain no chaotic trajectories, but both the square and pentagon are ergodic for almost all trajectories. Almost all trajectories in shapes def are chaotic, with d and e also known to be ergodic. The "Bunimovich mushroom" contains *both* a positive-measure set of non-chaotic trajectories, and a positive-measure set of chaotic trajectories.





Rough numerics and near-"optimal" parameters for some chaotic billiard tables. Based on Dahlqvist 1997 and Datseris, Hupe, Fleischmann 2019, for the Sinai billiard (d in fig. above) the Lyapunov distance seems to be a *decreasing* function of the hole-circle's radius. In units causing the billiard table to have area=1, the Lyapunov distance seems to assume its minimum value (approximately 0.5) when the circle diameter equals ≈80% of a square-side.

For the Bunimovich mushroom with semicircle diameter=2, stem height h>0, and stem width 2w for $0<w\le 1$, the non-chaotic fraction of phase space is

$$F_{\text{non-chaotic}} = 2[\arccos(w)-(1-w^2)w] / [hw+\pi].$$

If 0<w<1, then note that this F is strictly between 0 and 1 and is positive even in the limit h→0+, even though when h=0 we get the semicircle which has no chaotic orbits at all – a discontinuity. The Lyapunov distance of the chaos (expressed in units where the mushroom has unit area) seems to assume its minimum value (approximately 1.1) when h≈0.6 and w=1, i.e. when the mushroom becomes a half-stadium. If h≈0.6 and w=1/2 then the Lyapunov distance is ≈1.4.

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