Simulacra, Saturnalia, and Wild Extrapolation
— or —
Black Hole Thermodynamics and Semi-Classical Gravity As a Way of Life

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I dedicate this book, with respect, admiration and affection, to David Malament and Howard Stein, who helped me learn to think like a physicist, and to Bob Geroch and Bob Wald, who helped me learn to think like a philosopher.

Contents

Chapter VI

Black Holes as Thermodynamical Systems: The Central Problem

BOOTCAMP READERS:

1. this is a *rough* draft; the argumentative structure is not as clean and well formed as I want; there is some needless repetition in a few places, but not onerously so, and many infelicities; parts of the chapter are given only as an outline of the claims and arguments, albeit a detailed and thorough outline, and, even though not fully fleshed out, they are, I think, nonetheless both legible and intelligible; *caveat lector*

2. there are many exegetical passages, not part of the body of the chapter, but rather notes to myself or to the reader indicating where there are questions or problems I need to think more about, where I am dissatisfied with what is there, where I indicate further things I need or want to discuss, where I am reminding myself to do something, *etc.*; they are syntactically marked by being surrounded by asterisks enclosed in braces, *e.g.*, '[*** foo ***]'; the ones beginning "BOOTCAMP READER" I want you to read; the others you may read or not as you will; some of them I think you should, some are unnecessary for following the career of the argument, and a few will be unintelligible to you; *lector ad libitum*

When you're lost in the rain in Juarez
And it's Eastertime too
And your gravity fails
And negativity don't pull you through
Don't put on any airs
When you're down on Rue Morgue Avenue
They got some hungry women there
And they really make a mess outta you

Bob Dylan
"Just Like Tom Thumb's Blues"

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[*** BOOTCAMP READER: the following, up until the next exegetical comment of this form explicitly marking the end of the passage, will not appear in the final version of the chapter; I put it here to orient you[1] with regard to the general philosophical approach I am taking in the book as a whole; it will appear in the first chapter of the book, "Reveille" ***]

In today's world of philosophy of physics, standard practice is to require one of the following in order for one to feel that one has at least the grounds for understanding a theory:

1. a fixing of the fundamental ontology of the theory;
2. or a fixing of the semantics of the theory using some variant of the semantic view *à la* van Fraassen or (more popular these days) a possible-worlds semantics *à la* Lewis;
3. or whatever is demanded by any of a number of more niche views about how to fix the empirical content of a theory, which nonetheless have ardent backers, like the use of category theory to characterize the models of a theory in such a way as (the sometimes implicit claim goes) to allow for a unique articulation of their physical significance.

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[1] *NOT* to 'orientate' you, as the British barbarism has it.

Most often, proponents of such a thing think that that is *all* one needs in order to do all the work of understanding a theory. I call such a thing a *monolithic interpretation*: the idea that a single, formal description of the theory suffices to capture all of its epistemically and empirically relevant content. Even empiricists and instrumentalists such as Carnap and van Fraassen subscribe to Tarskian-like semantics to give empirical content to the mathematical formalism of theories. It is in that sense that I claim that they (and almost all other empiricists and instrumentalists I know of today) take the meaning of mathematical formalism to be determined by designative relations with respect to a fixed ontology, even if they are not realists about the ontology.

Even the work of philosophers of physics I feel sympathy with and have admiration for, who seem to want to resist the idea as well, like Weatherall's work on representation, and Ruetsche's spectacular book on QFT, succumb in the end to the urge to provide such a thing. I find it particularly surprising in work such as Ruetsche's—she explicitly argues that there cannot be a single "pristine interpretation" (to use her term) of QFT apt for all its uses, but she nonetheless thinks that a uniquely fixed possible-worlds semantics will suffice for characterizing all the ways to understand the theory in its different uses. And even though Weatherall wants to emphasize the freedom of a theory's "representational capacities", at the end of the day he still seems to think it can be fully characterized by something like a simple iteration of interpretive or coordinating principles.

What I rather would like to have, what I feel to be more appropriate given my antipathy to traditional metaphysics and my beliefs about the inadequacy of standard approaches to characterizing and capturing all of a theory's epistemically and empirically relevant content (Curiel 2022), is the freedom to pick and choose what philosophical tools may be fruitful in any given investigative context, without the prior expectation that one approach or set of tools will work everywhere. Sometimes, sketching a bare set of abstract interpretive principles does all the work one wants (such as in Malament's work on different ways an observer may characterize rotation in GR); other times, one won't be able to make any philosophical or foundational progress without getting down and dirty with the fine details of theoretical calculations and of possible experimental design without any pretense that a possible-world semantics or abstract interpretive principles will contribute anything of substance to our understanding of the situation (as I believe to to be the case for current gravitational wave astronomy, and for much of black hole thermodynamics in particular and semi-classical gravity more generally).

It is, therefore, *not* my aim to construct a monolothic "interpretation of SCG or BHT", as is the standard practice today in the philosophical treatment of physical theories in particular and fields of physics more generally. For that reason, many of the chapters of the book will consist of an accounting of all the interesting problems I find, along with examination of possible ways to address them, without aiming from the start to find a single, unified way of tackling them all. Part of my aim *is* to fight against the ontological tide in recent philosophy of physics, that a theoretical semantics, and so empirical content of theoretical propositions derivatively, is fixed by ontology.

Some of the situations we consider will be so epistemically unusual that near-globally shared philophical principles—so widely held that they are almost always never even explicitly recognized as being in play—may need to be denied, such as the idea that the mathematics of a theory stands in a relationship of designative, depictive or verisimilous representation to the world. Instead of fixing an ontology from the start and having it determine a monolithic interpretation, therefore, I rather will investigate whether we can develop a unified methodological and epistemological approach to the field. Such a thing would, at a minimum, provide some epistemically principled means for trying to figure out what tools or approaches will be useful in any given investigative context targeting a particular kind of system or situation or phenomenon.

The goal, thus, is to fix epistemic principles and have them guide interpretive activity—for interpretation is not a *terminus ad quem* but a *terminus a quo*. But this does not mean that nothing is ever fixed or determined, only that it is always so, when so, provisionally. As Stein (1992, p. 292) says, in articulating some of the virtues of Carnap's ideas over Quine's,

> [W]here Quine in principle leaves all open to the flow of experience – and of that part of experience, in particular, that constitutes the evolution of science – Carnap, having eventually come to recognize that science develops in ways that entail revisions even of 'categorial concepts', wishes to make at least local stands in the midst of this Heraclitean flux, and endorses constructions designed to achieve the maximum possible clarity both in what we say (and our understanding of what we say), and in the basis for the decisions we make.

I stand here with Carnap.

Thus:

The Methodology

> Treat matters as epistemological so far as possible, moving into metaphysics only so far as necessary, and then always grounding it in and constraining it by the knowledge and understanding one has acquired in conformity to the epistemological principles one works with.

It will, therefore, in the end, itself be a matter for investigation and critical judgment whether a monolithic, all-encompassing, single "interpretation" of SCG (or even just of BHT) is feasible, or even merely a sigle unified approach fruitful for addressing all issues. If promising answers to many problems turn out to hang together and mutually support each other in a compelling way, so much the better. If that does not appear to be the case, there will be important lessons we can learn from that as well.[2]

This, I believe, is how physics and metaphysics can bear fruitfully on each other, by the way of an enlightened and sophisticated pragmatism—in the rich sense of Peirce, as regimented by James, brought down to Earth by Dewey, informed with the rarefied spirit of logic by Carnap, opened up to possibility by Putnam, made wise and contemplative by Stein, in continual conversation with physics—that's the ticket. *Pragmatism* because what matters most in scientific knowledge, in the enterprise of knowledge and understanding, is having a rich and sophisticated enough understanding of all the possible relations between formal theory and practical work, how one *embodies that understanding in one's practice* of bringing the two into contact (Stein 2021). I want to chart a path between a naive and Polly-Anna-ish realism, a tepid and pessimistic instrumentalism, and a

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[2] I thank David Malament for pushing me most mercilessly and fruitfully on what I mean by a "unified approach to a theory" that is not a monolithic interpretation.

cynical, world-weary anti-realism: a sophisticated, wise and equable pragmatism.

[*** BOOTCAMP READERS: here endeth the orienting exegetical passage ***]

Almost all of the topics addressed in this book have at their root the idea that it makes sense to attribute in a physically significant way thermodynamical properties to black holes, which I call the *Central Problem* of the field, ramifying directly into or otherwise bearing on almost every other problem. The claim that it makes sense to do so—the *Central Claim*—is, both implicitly and explicitly, ubiquitous in the entire field of research. It is formulated in various ways by physicists and philosophers working on radically different problems, taking entirely different approaches in wildly varying frameworks—see, *e.g.*, Sciama (1976), Sciama, Candelas, and Deutsch (1981), Unruh and Wald (1982b), 't Hooft (1985), Wald (1994), Rovelli (1996), Strominger and Vafa (1996), Maldacena (2000), Wald (2001b), Jacobson (2003), Padmanabhan (2005), Page (2005), Hayward et al. (2009), Carlip (2014), Curiel (2014), Mann (2015), Harlow (2016), Oriti, Pranzetti, and Sindoni (2016), Wall (2018), Wallace (2018b), Lüst and Vleeshouwers (2019), Prunkl and Timpson (2019), Wallace (2019), and Almheiri et al. (2021).[3] [*** make bibtex entries, add references, for:

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[3] This list could go on, if not *ad infinitum*, then certainly *ad nauseum*. I tried to choose exemplars that cover a broad spectrum of times, problems, approaches and frameworks, but I have no doubt that some personal biases entered into its construction.

Interestingly enough, in his seminal paper Hawking (1975) makes no such claim. He rather describes black holes only "as if" they were thermodynamical systems, saying, *e.g.*, (p. 199),

> quantum mechanical effects cause black holes to create and emit particles as if they were hot bodies with temperature $\frac{\hbar\kappa}{2\pi k}$ . . .

and (p. 201),

> the gravitational field of a black hole will create particles and emit them to infinity at just the rate that one would expect if the black hole were an ordinary body with a temperature in geometric units of $\frac{\kappa}{2\pi}$ . . .

and (p. 203),

> There are independent, thermodynamic, grounds for regarding some multiple of the surface gravity as having a close relation to temperature.

I suspect that Hawking at the time already believed that black holes, in some sense, really were thermodynamical systems, but was perhaps unwilling to make the explicit claim in the paper due to concerns about editorial refereeing or general opprobrium from the wider community. Remember that, at the time, it was only Bekenstein who had claimed that black holes ought to have real thermodynamical properties attributed to them, suffering skepticism verging on obloquy from the rest of the community, such as publicly expressed by Israel (1973), and (more mildly) by Hawking himself only a year earlier (Bardeen, Carter, and Hawking 1973). Indeed, even Hawking's own initial, adumbrative announcement of his results (Hawking 1974) was met with immediate and severe skepticism by leading figures in the field. Davies and Taylor (1974), for instance, concluded,

> In our view it is a rather too literal interpretation of the concept of the 'temperature' of a black hole to apply it to emission processes in this way. . .

It is, however, noteworthy that, very soon after, proposals were made (Chapline 1975) for possible cosmological observations of primordial black holes that might preferentially support the conclusions of Hawking (1974) over those of Davies and Taylor (1974).

In Hawking (1976), even though he now uses phrases such as "black hole entropy" and "the temperature of the black hole", in the end he is still careful not to make the strong claim (p. 197):

> The conclusions of this paper are that there is an intimate connection between holes (black or white) and thermodynamics. . .

Davies 1978 "Thermodynamics of BHs"; some exemplificatory stuff between 1985–1996; . . . ; just how nauseated do I want to make the reader? Very. Very nauseated. ***] (I discuss several of these in detail below.) Until recently, however, there has been no sustained or detailed attention either by philosophers or physicists given to the question of what it may mean to conceive of black holes as thermodynamical objects, certainly not in any philosophically systematic way (Curiel 2014). In this chapter, I will initiate such an investigation.[4]

In particular, I will argue that substantive empirical content accrues to the relevant theoretical claims if and only if one can show that the theoretical models of the purportedly thermodynamical behavior of black holes are adequate for demonstrating that they enter into appropriate interactions with external systems of a manifestly thermodynamical character, such as ordinary gases and thermal blackbody radiation. (That biconditional does not imply that those interactions and their direct consequences *exhausts* all empirical content accruing to the theoretical claims.) This will be the epistemic principle guiding an attempt to find a unified approach to interpreting BHT and SCG. That proposed answer, and the assumptions and methods I use to argue for it, will ground the analysis of many of the rest of the problems I investigate in the book.

In this chapter, I will, for the most part, only pose problems and questions and suggest possible ways to begin to address them. Throughout the following chapters, I will return to these suggestions, testing them against the more detailed issues to be examined, and so clarifying them, emending them or discarding them, as the case may be, and possibly developing new ones as the career of the investigations demand. It will not be until the final chapter, "The Epistemic Content of It All", that we will be in a position to determine how well those attempts fare as possible ways

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Even two years after the seminal paper, Gibbons and Hawking (1977) still made only guarded claims such as (p. 2739),

> the close relationship between event horizons, gravitational fields, and thermodynamics that was found for black holes has a wider validity.

In Wald's (1975) immediate, clarificatory expansion of Hawking's original argument, he also remains guarded, saying (p. 10, his italics),

> The main result of the analysis is the following: *the density matrix for emission of particles to infinity at late times by spontaneous particle creation resulting from spherical gravitational collapse to a black hole is identical in all aspects to that of black body thermal emission at temperature* $kT = \hbar\kappa/2\pi$.

Indeed, he concludes (p. 31),

> Many fundamental questions remain for future investigations: is there indeed a deep connection between entropy and black hole area, or is the above analogy [the role of area in Bekenstein's GSL] merely an accidental quirk?

The earliest claim I can find to the effect that black holes truly are thermodynamical systems, and that in particular surface gravity and area are, respectively, real thermodynamical temperature and entropy, is Sciama (1976), and he goes out of his way to explain and defend the novelty of the claim. I discuss his arguments below in §vi.1. I thank Ted Jacobson for drawing my attention to it.

[4] There have also been compelling attempts to attribute thermodynamical properties and behaviors to more general causal horizons (Jacobson and Parentani 2003), not just those associated with black holes, such as Rindler horizons (Unruh 1976), cosmological horizons (Gibbons and Hawking 1977), and even more exotic objects such as quantum extremal surfaces (Engelhardt and Wall 2019). I focus in this chapter only on the putative thermodynamical properties and behaviors of black holes. I examine the possible thermodynamics of other and more general types of horizons and their peculiar problems in, *inter alia*, chs. ix, xiii and xvii, and in particular in §§vii.15 and xvi.9, below.

to construct a unified approach to—but, I expect, not a monolithic interpretation of—BHT and SCG.

As will be my practice for the subsequent chapters of the book, I will postpone and segregate to a Postscript (§VI.9) detailed discussion of the literature on purely philosophical issues not immediately about the foundational problems of the physics, so as not to distract those readers without the background or interest to engage with it. [*** BOOTCAMP READER: does this device work? I want to make the book accessible to physicists who have never, *e.g.*, read Peirce, but I also want to discuss Peirce, so this seems like a possible compromise. ***]

VI.1 The Meaning Problem

As we have seen in §V.9.1, the formal analogy between the classical laws of black hole mechanics and those of ordinary thermodynamics is near perfect, and, in the purely classical domain, most physicists and philosophers are content to leave it as such, *viz.*, formal. As I then sketched in §V.9.2, most physicists and philosophers, when quantum effects are taken into account, want to move beyond the formal analogy and claim that black holes are truly thermodynamical systems. We arrive thus at the first—and central—problem of the book.

The Meaning Problem

> What does it mean to conceive of black holes as thermodynamical systems?

What one counts as a good answer to this question will depend sensitively on one's views on how theoretical terms in physical theories acquire meaning, and what forms that meaning can take. (See, *e.g.*, (Carnap 1956; da Costa and French 2003; van Fraassen 2008; Curiel 2017c, 2022).) I postpone detailed discussion of the philosophy literature to §VI.9 below.

Every two systems treated by formally similar kinematical constraints, dynamical equations, laws, principles, *etc.*, will of necessity be similar in many ways *as systems*—*e.g.*, everything and its mother is an SHO, and the kinematical constraint constituting the dependence of period on ratios of kinematic quantities for all classical SHOs—but, for some reason, we don't think that all SHOs are "physically similar" or "equivalent" or what have you, much less "identical" or even "the same" in some weaker but still physically significant sense [*** break this up into 3 sentences, with the clause between dashes giving two concrete examples of two different kinds of SHO and their periods ***]. We don't speak of a system as being "truly an SHO", except, perhaps, where we mean to indicate, *e.g.*, that possibly dissipative mechanisms or effects can be ignored in the regime of interest—but we never make such a claim in the same way as physicists and philosophers seem to when they claim that black holes are "truly thermodynamical systems".

Indeed, this seems to be the first instance of this problem in the history of physics; if that is correct, it is a remarkable fact, a truly novel problem in theoretical physics, of a depth not seen since the development of GR and QT. [*** But what about static magnetic and electric fields in the 18th Century? did they think a static magnetic field obeying Coulomb's law was possibly an electric field? did this happen in early particle physics, with the proton and neutron, alpha rays, beta rays, x-rays? perhaps those are similar cases, but none of them show all the relevant features: how theoretically close black holes are to ordinary thermodynamical systems, how theoretical foray after theoretical foray uncovers new facets of black holes displaying similarity or even conformity with ordinary thermodynamical systems, with the dissimilarities (*e.g.*, non-extensivity of entropy) admitting of possible, plausible explanations (in this case, entanglement entropy, emergence of area laws for entropy in other quantum thermodynamical systems, . . . [*** references ***]), and still after decades all of it with no empirical access; I will leave detailed consideration of the possible historical examples to more capable hands. ***]

I'll call the abstract kinematical constraints and equations of motion of systems such as an SHO a *schema*, and each particular species of physical system whose equations of motion are of that form an *instance* of the schema (and so, by extension, I will also refer to the systems themselves as 'instances', and so on). One such species is the simple pendulum; another is the vibrating string.[5] When there is a system of equations of motion or field equations that uniquely characterize a true species of physical system—a natural kind, if you will—and so does not act as a schema, such as Maxwell's equations do for Maxwell fields, then I will refer to a given concrete system of that kind as an *individual* of the species, as, say, a particular solution to Maxwell's equations determined by concrete initial data. In what way, then, are individuals of a species similar or the same that instances of a schema are not?

[*** intermezzo: philosophical accounts of similarity, what they may or may not be able to provide of use to the issue:

1. to get straight on what one means by "the same" (different forms of energy, *e.g.*, or, more to the point, of entropy, and thermodynamical character in general) requires consideration of several different relevantly admissible, meaningful notions of "related", each physically significant in its own context, and of how that may all change when pushed into a new regime, for new phenomena
2. start by distinguishing different ways one might advance, *viz.*, from the linguistic side, as it were ("meaning of terms"), or from the cognitive side ("meaning of concepts"), or from the physical or metaphysical side ("sameness of (natural) kind")
3. dismiss the first, for we are interested in physics here, but postpone discussion of concepts to §VI.3; the issue is not whether 'thermodynamical' as a qualifier of the properties and behavior of boxes of gas and of black holes has the same meaning in both cases, but rather whether the two kinds of systems are physically the same in some relevantly substantive sense—which may entail sameness of meaning of the differently circumstantiated qualifier, but that sameness of meaning will be a consequence of a good analysis, not a ground for it.

end of intermezzo ***]

[*** BOOTCAMP READER: the remainder of the section is given as a detailed outline; for the remainder of the chapter, when parts are given only in outline form, I will no long explicitly remark on the fact, the list form of the text serving as sufficient indicator ***]

1. with the intermezzo and discussion of the SHO and Maxwell fields as motivation, provide a somewhat precise definition of "simulacrum" ; I don't want to beg questions by defining from the start a simulacrum as something that obeys all or most equations, but doesn't interact with other systems in the right way, since that is a position I want to argue for

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[5] I will not define the notion of "species" here. I trust its rough sense is clear enough. I define the notion and give detailed discussion of it and closely related ideas in Curiel (2021, 2022).

2. thus, define simulacrum: "a simulacrum of a given species of physical system is one whose quantities, qualitative behavior, kinematical constraints, dynamical equations, laws, principles are significantly similar to or even formally identical with those of the species, and yet do not fall within the ambit of the theory treating the species" (where "same theory" may mean "extension of original theory so as to account for new types of systems, new regimes, *etc.*")

3. reframe the question, then: are black holes mere simulacra of thermodynamical systems, or are they The Real Deal, *viz.*, individuals of the species?

4. extending ordinary thermodynamics to cover BHT: not translatability or analogy, but rather *being part of the same theory*, extended so as to treat new kinds or classes of physical systems, possibly in new regimes; as an example, we now take the treatment of blackbody radiation to be *part of* thermodynamics (otherwise it is difficult to see how it could serve as part of the definition of temperature[6]); that's what physicists want today, as physicists at the end of the 19th century struggled to do with blackbody radiation itself, and the electromagnetic field more generally; crib discussion from Curiel (2014)

5. remark on how this is different from joining two seemingly disparate types of physical systems into a single theory by unification, *e.g.*, Maxwell's realization that electric fields and magnetic fields were not the same thing, and yet not separate, but rather different aspects of the same thing

6. there are several different ways to go: identity (in relevant sense) of quantities, of qualitative behavior, of kinematical constraints, of dynamical equations, of possible interactions with other (types of) systems, of principles, of laws; I do not pretend to have a clear and precise characterization of each of those clearly distinguishing each from all the rest; they certainly bleed into each other; but I trust the sense is clear enough to be getting on with for the moment

7. most physicists (and philosophers) seem to focus on laws:

   a. Wald (2001b) makes the claim in exemplary fashion:

   > [I]t appears that certain laws of black hole mechanics are, in fact, simply the ordinary laws of thermodynamics applied to a system containing a black hole.

   b. Wall (2018, p. 2) explicitly uses the weaker 'similar':

   > [Black holes] obey laws of thermodynamics similar to ordinary matter systems, when viewed from the perspective of an observer outside the horizon, so long as we attribute to the horizon an entropy S proportional to its area A, a

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[6] See, *e.g.*, Benedict (1969) for detailed exposition of the way the Planckian power spectrum of blackbody radiation is used to define that part of the temperature scale above 1063.0 °C. It is not the most up-to-date reference with regard to the international agreement on defining the standard methods for the determination of temperature (for which see, *e.g.*, Rumble 2022), but I have found no better guide to the nuts and bolts of thermometry when it comes to developing in a physically illuminating way how the mathematical theory and the experimental techniques bear on each other.

temperature T proportional to its surface gravity κ, and of course an energy E proportional to its mass M.

   c. discuss also Carlip (2014), Mann (2015), and Lüst and Vleeshouwers (2019), and some algebraic QFT on curved spacetime folks, and some LQG and some holography folks

   d. Dougherty and Callender (2016) never give a definitive statement on the matter, but base their arguments primarily on quantities and principles

   e. Wallace (2018b) is interesting case: he likewise never gives a definitive statement on the matter, but he does invoke and rely on in different parts of the career of his argument relevant similarity or identity (he is not always clear on the distinction) with regard to all of: quantities; qualitative behavior; interactions; laws; and natural kinds

   f. Prunkl and Timpson (2019) likewise never give a definitive statement on the matter, but base their arguments primarily on quantities, interactions and principles

8. I will rely on my recent and ongoing work on the semantics and the empirical content of physical theories, the fundamentally pragmatic nature of the structure of our knowledge in physics and the nature of inter-theoretic relations to formulate an answer to address this question (Curiel 2017b, 2017c, 2021, 2022).

9. In particular, I will argue that substantive empirical content accrues to the relevant theoretical claims only if one can show that the theoretical models of the purportedly thermodynamical behavior of black holes satisfy identical kinematical constraints as ordinary thermodynamical systems do, and are adequate for demonstrating that black holes enter into appropriate interactions with external systems of a manifestly thermodynamical character (Curiel 2014).

10. I prefer kinematical constraints and interactions because they capture all those ideas at once, in so far as their proper definition, representation and use in investigations relies on all the rest (quantities, qualitative behavior, dynamical equations, laws, principles), and at the same time captures something of indubitable physical significance that, moreover, lends itself to direct empirical investigation (at least, if we had empirical access to the relevant regimes); all the rest—quantities, qualitative behavior, dynamical equations, principles and laws—are important and play essential roles, but, I claim, they can all be understood as grounded in an adequate comprehension of kinematical constraints and interactions

11. If, say, there were some physical system that obeyed the free Maxwell equations but did not couple to, interact with electric charge the way that Maxwell fields do, that would be a *simulacrum* of a Maxwell field. [*** three possibilities to consider: they couple to some other charge by way of an equation formally the same as the second Maxwell equation; they couple to some other charge by another equation of another form; they couple to electric charge by an equation of some other form ***].

12. I have qualms about introducing yet another Ism into philosophy, but, for the sake of brevity of expression, I shall: I will call this idea 'interactionalism'.[7]

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[7] I thank Alex Mathie, in an unpublished manuscript, for coming up with the moniker.

13. Before giving a more detailed exposition of interactionalism and supporting arguments for it, however, I will first consider what seems to be the main argument in both the physics and the philosophy literature in favor of taking black holes to be true thermodynamical systems, *viz.*, the invocation of the Hawking effect.

14. Wald (1999b, p. A182) makes the claim in exemplary fashion:[8]

    > The fact that $\kappa/2\pi$ truly represents the physical temperature of a black hole [as a consequence of the Hawking effect] provides extremely strong evidence that the laws of black hole mechanics are not merely mathematical analogues of the laws of thermodynamics, but rather that they in fact are the ordinary laws of thermodynamics applied to black holes.

    See also, *e.g.*, Page (2005), Wallace (2018b), [*** cite others ***]. Indeed, Unruh and Wald (1982b, p. 944) make the even stronger claim that

    > the existence of acceleration radiation [outside the event horizon, a consequence of the Hawking effect] is vital for the self-consistency of black-hole thermodynamics.

    How does this relate to the claim that we ought to use similarity or sameness of laws to address the Central Problem? Emission of blackbody radiation *is* a law in the relevant sense.

15. "Hawking radiation seems crucial for a statistical mechanics of black holes": one must distinguish between what is needed for a *thermodynamics* of black holes and what for a *statistical mechanics* of them; one may well believe Hawking radiation is needed for the former but not the latter; if, *e.g.*, the micro-degrees of freedom of black holes do not couple to external quantum fields in the right way to produce the analogue of blackbody radiation—as the jiggling electrons in hot iron couple to the quantum Maxwell field to produce blackbody electromagnetic radiation—then one might well have a statistical mechanics without a thermodynamics (since the black hole will then not couple in the standard ways with ordinary thermodynamical systems); the treatment of black holes as "condensates" of underlying GFT states in Oriti, Pranzetti, and Sindoni (2018), for example, provides a statistical mechanics of black holes completely independent of Hawking radiation; discuss Wallace (2019)

16. so, putting aside for a moment the issue of providing a statistical-mechanical underpinning for the putative thermodynamical properties and behavior of black holes—I will return to it again and again below, primarily in chs. X, XI, XIII and XVI—how in detail can the Hawking effect provide grounds for attributing meaning to the Central Claim?

17. [*** after addressing that, move on to: ***] What, besides Hawking radiation, are the quantum field-theoretic effects in semiclassical gravity that may be relevant to the conceiving of black holes as thermodynamical systems? Other possibilities that come to mind are: the singularity structure of the 2-point functions of the vacuum states of quantum fields in curved spacetime [*** cite ***]; the behavior of $\langle \hat{T}_{ab} \rangle$ near the event horizon and asymptotically far away from it [*** cite ***]; and the dependence of black hole entropy on area and the validity

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[8] Similar remarks appear in Wald (2001b).

of the GSL (Wall 2009). Are these necessary for thinking of black holes as thermodynamical? Certainly the last, probably the first, and possibly the second.

18. Briefly discuss fact that we seemingly need to attribute entropy to black holes in order to save the ordinary Second Law by extending its formulation to the GSL. Although this may superficially seem to support the standard claim that it is similarity of laws that provides the means for addressing the Central Problem, in fact it rather supports my view, for the putative violations of the ordinary Second Law occur only when black holes interact in the right sorts of way with ordinary thermodynamical systems. I discuss the GSL at length in ch. XI, and in particular in §VII.1 the way it grounds the attribution of entropy to black holes.

19. Another possibility: putting the Hawking effect aside, the provision of a a statistical-mechanical grounding for black hole thermodynamics would *prima facie* lend meaning to the Central Claim

20. I find this unsatisfying, because it begs a serious epistemic question: in essentially all studies of statistical-mechanical systems, it is their thermodynamical behavior that provides evidence for the correctness of the statistical-mechanical treatment. If we do not have independent access to a system's relevant micro-degrees of freedom and their dynamics, do not, in other words, have some independent warrant for believing *both* that those are the relevant micro-degrees of freedom and concomitant dynamics that give rise to what we believe to be thermodynamical properties and behavior *and* that we have their statistical treatment adequately under control for the derivation and study of that thermodynamics, then we must *already know* that a system is appropriately thermodynamical in order to use the results of the study of its thermodynamics as evidence for the correctness of a statistical-mechanical treatment. To use a putative statistical-mechanical treatment as the ground for conceiving of black holes as thermodynamical systems, without independent reasons for thinking that statistical mechanics (micro-degrees of freedom, dynamics, statistical treatment) is on the right track, is to get the epistemic order of the subject wrong.

21. That is, however, the situation we find ourselves in with regard to a possible statistical-mechanical treatment of black holes. Classical general relativity provides no resources for defining micro-degrees of freedom of purely gravitational systems such as black holes, much less a dynamics for them. Such a statistical-mechanical treatment, therefore, would have to take the micro-degrees of freedom as the deliverance of something like a theory of quantum gravity, even if only a manifestly effective theory, such as the low energy, perturbative effective field theory Burgess (2004) develops and defends. ([*** Wallace, "Quantum Gravity at Low Energies", 2021, http://philsci-archive.pitt.edu/20049/, also defends the framework ***].) Even in this seemingly safe, benign framework, however, which seems to be well understood and which we seem to have under theoretical control across a substantive range of physical regimes, for an interesting class of physical systems, I claim we have too little epistemic warrant to use the results of arguments and calculations in such frameworks as evidence for further, contentious theoretical claims. To try to do so would, again, get the epistemic order of the situation wrong.

22. this idea, of getting the epistemic order right in scientific investigations, will recur again and again; I treat it to some depth in §VI.7 below, where I will also discuss the kind of frameworks Burgess (2004) develops and [*** Wallace 2021 ***] defends, arguing in some detail for my claims here.

23. Of less concern, but still worth considering, is the fact that any such introduction of micro-degrees of freedom for purely gravitational phenomena goes beyond the scope of SCG itself as a framework, which assumes a classical spacetime geometry. Indeed, this is of a piece with the previous considerations. As epistemically parlous as the use of SCG may be, in so far as it is grounded in the classical spacetime geometry of general relativity, for which we do have strong empirical warrant, one can only worsen the epistemic situation by introducing theoretical contrivances to attempt to capture the behavior of "gravitational" degrees of freedom more finely grained than general relativity admits.

24. returning now to my preferred approach (although not exclusive of calling on the resources of those other possibilities, and perhaps ones not yet considered that will arise during the course of the book's larger career)[9], interactionalism is the epistemic principle on and around which a unified approach—not a monolithic interpretation—to BHT/SCG may be built, although it is likely not to suffice on its own for the job

25. for interactionalism also allows us to begin to try to address the problem of epistemic warrant, and possibility even the problem of epistemic order, in a principled way, to some degree: in ordinary physics, one wants of a new model or theory, in the best of cases, that it predict unexpected, novel phenomena, which one can then search for experimentally; we have not that luxury here; so what we can rather do is require that, when a theoretical framework has been constructed from other frameworks, themselves already having empirical support, in such a way that the way they are combined and extended is guided by the attempt to ensure that the posited new phenomena/kinds of systems interact with known kinds of systems "in the right way" (which must be specified and argued for), then some epistemic warrant may accrue to the new framework if one then discovers that it allows one to treat other new phenomena/kinds that interact with *other, different* known kinds of systems "in the right way"

26. *N.b.*: this is nothing like the non-empirical confirmation of Richard David on string theory, for here the idea is that what little empirical warrant may accrue comes from the construction, combination, extension directly from theoretical frameworks that themselves do have empirical warrant, and then showing that unexpected predictions are made about the relation to other theoretical frameworks about which we also have empirical warrant; string theory was built from the ground up on mathematical principles alone, in the sense that nothing fundamental about it was constructed directly from combinations and direct extensions of

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[9] [*** Klaas Landsman, in conversation, raised the possibility of trying to employ the theory of thermodynamics developed for chaotic dynamical systems as a basis for understanding black holes as thermodynamical systems. (See, *e.g.*, David Ruelle, *Thermodynamic Formalism*; Rufus Brown, *Equilibrium States and Ergodic Theory of Anosov Flows*; Collet and Eckmann, *Concepts and Results in Chaotic Dynamics*; I thank Landsman also for those references.) It is beyond the scope of the present work to consider that possibility, although I find it intriguing, and undoubtedly worth pursuing. To an unknown, eager PhD student: a marvelous dissertation topic! ***].

known frameworks, but rather the attempt was made to connect it with known frameworks only after the fundamental structures were already decided upon

27. discuss Sciama (1976), the first instance I can find in the literature of someone arguing in favor of the Central Claim based (in part) on the idea that they interact with ordinary thermodynamical systems in the right way. See especially pp. 392*ff.* [*** but I think early Bekenstein did something similar—go back and read those papers again ***].

28. discuss now in detail Wallace (2018a), who argues vigorously that we should treat black holes as thermodynamical objects and Dougherty and Callender (2016) argue against it, but neither gives a clear criterion for when it is appropriate or not to treat any physical system as a thermodynamical one.

29. discuss now in detail Prunkl and Timpson (2019), who pick up and use the criterion that I proposed in Curiel (2014).

VI.2 The Criterion Problem

My answer to the Meaning Problem leads directly to the Criterion Problem: what kinds of other systems should be conceived of as "manifestly thermodynamical", and what are the kinds of interactions black holes should have with them in order to support the claim that black holes themselves are thermodynamical? Physicists have not provided clear criteria for "manifestly thermodynamical systems". A few philosophers have touched on the issue, with Dougherty and Callender (2016) arguing against orthodoxy, that we should be skeptical of the claims that black holes are truly thermodynamical objects in any substantive sense, and Wallace (2018a) arguing in favor of the orthodoxy. One issue they disagree on, but which is perhaps the most important one to settle in attempting to address the Meaning Problem, is the principled choice of criteria for determining what is and is not to count as a "thermodynamical system", and why.

The Criterion Problem

> What are good criteria for deciding when a given kind of physical system is "thermodynamical", and why?

As I intimated in §VI.1, the way that physicists in the 19th Century convinced themselves that electromagnetic radiation is thermodynamical provides a useful template for doing the same with black holes: the surface gravity and area must play the same role in the new theory with regard to modeling processes and interactions (such as mutual equilibration and heat flow) as, respectively, temperature and entropy do in the original theory (classical thermodynamics) with regard to the analogous interactions and processes there for the systems that classical thermodynamics appropriately and adequately treats (*e.g.*, ordinary gases and fluids).

I take the concepts "work" and "heat" and the appropriate relations between them to lie at the heart of thermodynamics, in particular the Clausius and Kelvin Postulates, and so to provide the grounds for a satisfactory criterion for what counts as "truly thermodynamical". [*** explain why they are fundamental, briefly discuss Maxwell (1891), Boltzmann (1964), Planck (1926), Sommerfeld (1964), Fermi (1937), and Uffink (2007); briefly explain why I find axiomatic treatments, *e.g.*, Carathéodory (1909) and Lieb and Yngvason (1999), based on relations such as adiabatic accessibility, to be unsatisfying, as such concepts are meaningless until one has a grip on heat and work ***]. The criterion, roughly speaking: "interaction" in relevant sense must include exchanges of work and heat satisfying Clausius and Kelvin Postulates.

The claim that what we conceive of as ordinary thermodynamical systems in the ordinary course of things and the kinds of interactions among them we take to be characteristic of thermodynamic behavior—the kinds of systems studied in undergraduate and graduate courses, *e.g.*, and the particular behaviors there focused on—ought to be taken to be "really thermodynamical" is not a metaphysical claim. It is, in particular, not a claim about natural kinds (metaphysically construed) or essential structures or anything like that. It is an epistemological and methodological claim. Those are the kinds of system that the framework of thermodynamics was first worked out to deal with; that framework handles the modeling and investigation of those systems superbly; and we know by strongly supported experience how to extend that framework so as to treat new kinds of thermodynamical systems.

VI.3 The Extension Problem

The Central Problem raises as well further questions of immediate relevance to longstanding and important debates in more general philosophy of science. The one of most relevance here is about the nature of physical concepts and how they are modifed and evolve through theory development and theory change:

The Extension Problem

> How can the concepts and relations of one theory, in this case classical thermodynamics, be extended and modified so as to be applicable in the context of a radically different one, the quantum treatment of black holes in general relativity? And how can this be done, moreover, so that the extended and modified concepts and relations have, in some important sense, the same physical significance as the original ones, or are appropriate successors thereof, with the original ones visible in them, as it were, when restricted to the relevant regimes and systems?

To address this problem, and the related more general philosophical problems of the referential stability of physical concepts over time and in different physical contexts, and their possible incommensurability across different theories, I will rely on:

1. Peirce (1877a), Peirce (1991), and Peirce (1894, 1992a) on nature of signs and concepts
2. Nersessian (2002, 2008) on development and modification of concepts
3. look at `concept-extension.tex`, `concepts-in-play.tex`, `explication.tex`, `concept-formation.tex`, `concepts-theory-wants.tex`, and `investigative-frameworks.tex`
4. my work on the confirmability of theoretical frameworks (Curiel 2021), in conjunction with my work on the semantics and empirical content of theories (Curiel 2017b, 2017c, 2022): When the concrete models of physical systems constructed by structuring data gathered from experimentation and observation can be appropriately identified with individual theoretical models across multiple theories, then one has an appropriate translation that preserves physical significance.

VI.4 The Problem of Too Much Rigor

The first three laws of black hole mechanics are rigorous theorems in differential geometry that follow only from the fundamental mathematics of relativistic spacetimes, and do not depend in any essential way on the particulars of relativistic dynamics as encapsulated in the Einstein field equation (Curiel 2017a). This is in strict opposition to the laws of classical thermodynamics, particularly the Second Law, which stand as more or less phenomenological principles derived by empirical generalization. If we are to take seriously the idea that black holes are truly thermodynamical objects, as the overwhelming majority of the physics and philosophy communities do, this raises another problem, a natural sequela to the Meaning Problem, no matter what else may be said in favor of the idea:

The Problem of Too Much Rigor

> How can the laws of black hole mechanics, which are theorems of pure differential geometry, ground physical content that is appropriately the same as the laws of classical thermodynamics, which are phenemonological generalizations?

How can the formal, mathematical structure of classical general relativity on its own, without even the input of physically substantive principles, "know" about the thermodynamical nature of black holes? One possible answer: energy conditions are assumed in the proofs of the laws of black hole mechanics, and those *do* have the character of the laws of ordinary thermodynamics, *viz.*, empirical generalizations not themselves admitting of proof (Curiel 2017a).

1. *N.b.*: energy conditions themselves are just formal constraints on geometrical quantities used to prove a mathematical theorem, gaining empirical content only after the theorem is proved, by invocation of the EFE (Curiel 2018)
2. mention proofs of ANEC using *quantum* methods, and assumptions about entropy (advert to ch. XIV)—more support for the idea that black hole thermodynamics needs quantum theory?
3. any role here for interactionalism?

However one attempts to address it, this problem will complicate any attempt to provide an answer to the Extension Problem (Section VI.3).

[*** This segues naturally into classicality problem, *à la* Jacobson's plaintive query, "How did classical GR know..." ***]

VI.5 The Classicality Problem

> The four laws of black hole mechanics, which are analogous to those of thermodynamics, were originally derived from the classical Einstein equation. With the discovery of the quantum Hawking radiation, it became clear that the analogy is, in fact, an identity. How did classical general relativity know that the horizon area would turn out to be a form of entropy, and that surface gravity is a temperature?
>
> Ted Jacobson (1995)
> "Thermodynamics of Spacetime: The Einstein Equation of State"

It is still orthodoxy today in the physics community that there is no consistent theory of thermodynamics for purely classical black holes (Unruh and Wald 1982a; Wald 1999a, 2001a), *i.e.*, when quantum effects are not taken into account, for two reasons. First, it seems that they must be assigned a temperature of absolute zero, if any at all, and that itself for two reasons. The first is that, classically, black holes are perfect absorbers and emit nothing, and so by the law of blackbody radiation they must have temperature absolute zero. The most infamous example in this entire field of study, Geroch's thought experiment of slowly lowering towards a black hole a box filled with thermal matter, provides another argument that a classical black hole must have temperature absolute zero, for its claimed consequence is that one can use them to construct a thermal engine with perfect efficiency.[10] The second reason that orthodoxy holds there is no consistent thermodynamics of classical black holes is that they seem to violate the Clausius Postulate, in so far as, were a temperature proportional to their surface gravity to be attibuted to them, then heat could spontaneously flow from a colder to a hotter body by putting a black hole in a thermal bath at a temperature lower than its own attributed temperature: the black hole would absorb the cold thermal radiation and give nothing back.

One may well wonder, however, whether these standard arguments in favor of orthodoxy are correct. One observation that should spur skepticism is that all the arguments appear to depend on exchange of the wrong sort of energy: if we want to try to construct a thermodynamics for classical black holes, which are purely gravitational systems, then we should take account of the possibility of characterizing purely gravitational forms of heat and work and investigate what role they play in the mechanisms grounding the arguments. The only mechanism for heat exchange with black holes the arguments allow is by way of Hawking radiation and the heat of ordinary material systems. They therefore ignore the possibility of defining "gravitational heat" by way of exchange of gravitational energy (*e.g.*, in the form of gravitational radiation). Had 19th Century physicists ignored exchange of electromagnetic heat and work in their investigations of blackbody radiation, they too would have concluded that there was no consistent thermodynamics of blackbody radiation to be had.

One may well also question whether the definition of temperature used in many of the arguments, based on the Planckian power spectrum of blackbody radiation, is the appropriate one to use in the context of a purely classical description of black holes, for the electromagnetically radiative thermal equilibrium of systems immersed in a bath of thermal radiation is essentially a *quantum* and *statistical* phenomenon, by which I mean one that can be correctly modeled only by using the hypothesis that radiative thermal energy is exchanged in discrete quanta and then computed correctly only with the use of statistical methods. To use that characterization of temperature to argue that we must use quantum mechanics in order to take surface gravity seriously as a physical temperature, therefore, may beg the question.

Thus:

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[10] For an exposition and critical discussion of the thought experiment, see Curiel (2014). According to Jakob Bekenstein (private correspondence) and Robert Wald (conversation), Geroch first proposed the example during a colloquium he gave at Princeton in December, 1971. Bekenstein remarked that he considered it the first attempt to attribute a temperature to a black hole.

The Classicality Problem

> Is it possible to construct a consistent thermodynamical theory of purely classical black holes, when quantum effects are ignored?

If the answer is yes, this would strongly suggest that spacetime geometry on its own, with no input from quantum field theory, is already an intrinsically thermodynamical structure, with possibly far-reaching consequences for what it may mean to "quantize gravity" (Curiel 2018). If the answer is no, then one must ask what it is about purely gravitational systems that, in contradistinction to ordinary matter, blocks them from admitting a classical thermodynamical description. Ordinary gases, liquids and solids, after all, have a well defined thermodynamics without accounting for quantum effects. Why should large black holes be different? And the question that Ted Jacobson poignantly and plaintively poses in the epigraph to this section would become even yet more mysterious.

I argue in Curiel (2014), contrary to orthodoxy, that there is a consistent way of treating purely classical black holes as real thermodynamical systems, that they should be assigned a thermodynamical temperature proportional to their surface gravity and a thermodynamical entropy proportional to their area, and, in fact, that not to do so leads to the same kinds of inconsistencies as occur if one does not do so for black holes emitting Hawking radiation (Unruh and Wald 1982a). Continuing the theme that interactionalism may play the role of grounding a unified approach to black hole thermodynamics, the arguments rely essentially on the analysis of interactions between classical black holes and ordinary thermodynamical systems. I will only summarize the form of the arguments and the claimed results here.

1. definition of gravitational free energy and gravitational heat for classical black holes based on irreducible mass (Christodoulou November 1970), and characterization of their thermal coupling to ordinary thermodynamical systems based on them

2. construction of a modified form of Carnot cycle, which I call a 'Carnot-Geroch cycle', as it is based on the mechanism at the heart of Geroch's infamous though experiment, which kills three birds with one stone: not only does it show that κ can be characterized as the absolute temperature of the black hole using the same arguments as classical thermodynamics uses to introduce the absolute temperature scale by way of efficiency considerations about Carnot cycles; it does so by showing that in the coupling of black holes with ordinary thermodynamical systems, κ does in fact play the physical role of temperature and area that of entropy; and, remarkably, it has as a natural corollary the existence of a universal constant that renders the proper physical dimensions to surface gravity as a measure of temperature and area as a measure of entropy

3. the argument based on the Carnot-Geroch cycle makes three non-trivial assumptions: first, that it makes sense to attribute a physical temperature and entropy to a black hole (though we do not yet know what they are); second, that the entropy of ordinary thermodynamical systems and the entropy of the black hole are jointly additive; and third, that ordinary thermodynamical systems and classical black holes mutually equilibrate when their respective temperatures are numerically the same

4. if one is suspicious of these assumptions, one ought to recall that they are *exactly the same* assumptions one needs to argue that blackbody electromagnetic radiation has a thermodynamical character consistent with that of ordinary thermodynamical systems

5. moreover, further justification for the Carnot-Geroch cycle comes from the fact that it allows one to formulate and sketch plausible arguments supporting the validity of appropriate versions of the Clausius and Kelvin Postulates extended so as to include classical black holes; if one is swayed by the arguments I adduced in defending my approach to the Extension Problem (§VI.3), then this provides further, powerful evidence in favor of a consistent thermodynamics for classical black holes

If my arguments are correct, a possible consequence might be that one should rather think of the Einstein field equation as a thermodynamical "equation of state" rather than a fundamental field equation (Jacobson 1995b). If that were correct, it might be a category mistake to attempt to "quantize general relativity", just as it would be to try to do so for the ideal gas law. [*** but, as Rovelli once remarked to me, one does "quantize", *e.g.*, fluid mechanics—phonons—as a semi-classical treatment, an effective field theory, which is perhaps what semi-classical gravity itself may best be thought of as, so that is really just grist for my mill—no one believes that the theory of phonons is a candidate for being fundamental, and with good reason ***]

[*** the signature of quantum gravity, in particular the traces of whatever statistical quantities it may give us for making traditional sense of the thermodynamical phenomena I discuss here, show up already in purely classical, non-statistical theory, thus giving us clues to what kind of statistical theory we may need underlying it, by looking at differences between black-hole thermodynamics and ordinary thermodynamics ***]

VI.6 The Problem of Too Many Definitions

The very idea of a black hole is not itself univocal (Curiel 2019). The many different definitions used in different fields, and even in different projects and problems in the same fields, are often inconsistent with each other. How will this compound and complicate all the other problems?

1. summarize Curiel (2019), give a few examples, compare usage of concept of "black hole" in BHT/SCG with those in other subfields

2. again, attempt to address this using interactionalism: what all the different characterizations do have in common, or at least ought to have, if they are to be used in investigations that support or rely on the Central Claim, if my arguments are correct, is the capacity to ground a treatment of appropriate interactions between black holes and ordinary thermodynamical systems

VI.7 The Problem of Epistemic Warrant (Black Hole Thermodynamics)

> String theorists are happily studying universes that don't exist, particle physicists are busy inventing particles that no one ever measures, and theorists mass-produce "solutions" to the black hole information loss problem that no one will ever be able to test.

> All these people get paid well for their remarkable contributions to human knowledge. If that makes you laugh, it's the absurdity of the situation, not my blog, that's funny.
>
> Sabine Hossenfelder
> Back Re(Action) blog, 15 Oct 2018
> http://backreaction.blogspot.com/2018/10/dear-dr-b-what-do-you-actually-live-from.html

Radin Dardashti gave a very interesting talk entitled "The Rise and Fall of Scientific Problems" as part of the Warsaw Spacetime Colloquium Series (11 Dec 2020, https://www.youtube.com/watch?v=S6L8bFLy0g4), in which he compellingly argued that scientific problems, among many other tasks, provide the motivation for theory development and shape how theory actually does develop. When one looks at the kind of problems that have driven theory in the last 50 years—the Higgs naturalness problem, the hierarchy problem, the strong-CP problem, the Information-Loss Paradox, the fine-tuning problems of cosmology, and so on—one comes to realize that none of them arise directly from a mismatch of theory and data, or from a lack of data that foreseeable technological development may mitigate. When one then considers the possible solutions they have driven us towards—supersymmetry, now effectively ruled out; string theory; multiverse inflation—one perhaps cannot but feel that such problems should *not* be allowed to play the dominant, primary role in motivating and shaping theory development that they have in those parts of contemporary theoretical work at the frontier of high-energy particle physics and gravitational physics. When one thinks of the cumulative hundreds of human millenia (like a man-hour, only gender neutral and longer) spent on supersymmetry over the last 50 years, and on BHT and QG, one cannot but feel that 80% of theoretical particle physicists should have gone into condensed matter, 90% of BHT and SCG physicists should have gone into classical GR or astrophysics, and 98% of QG physicists should have gone into cosmology. We paid a lot of money expecting some physics, but all we got was almost math.

These fields all raise profound problems of methodology. In the case of our particular focus now, black hole thermodynamics and results concerning quantum fields in the presence of strong gravitational fields more generally are without a doubt the most widely accepted, most deeply trusted set of conclusions in theoretical physics in which general relativity and quantum theory work together in seemingly fruitful harmony. This is especially remarkable when one reflects on the fact that we have absolutely no experimental or observational evidence for any of it. Indeed we do not have the remotest empirical access to the regimes where we expect such effects to appreciably manifest themselves. Those results, moreover, come from taking two theories (general relativity and quantum field theory), each of which is in manifest conceptual and physical tension with the other in a variety of respects, and each of which is more or less well understood and supported in its own physical regime radically separated from that of the other, and attempting to combine them in novel ways guided by little more than physical intuition (which differs radically from physicist to physicist) and then to extend that combination into regimes we have no hard empirical knowledge of whatsoever. This all gives rise to a nexus of related questions:

The Problem of Epistemic Warrant (BHT and SCG)

> How did a framework (SCG) derived by combining seemingly incompatible theories in a novel way so as to extend their reach into regimes that we have no way of testing in the foreseeable future, and its pampered progeny BHT, become the most important touchstones for testing novel ideas at the frontier of the theoretical physics of gravitation? Can they play that role? What epistemic warrant do we or can we have for them in our current epistemic state, and for the foreseeable future?

I believe that we should be much more skeptical of the conclusions than the physics community tends to be today. I do not recommend that we reject them, rather only that we should be more cautious in attributing confirmation to them in granting them our enthusiastic acceptance, in the face of a lack of empirical data directly relevant to them.

One can try to redescribe this situation more charitably. We are using well understood, empirically well entrenched principles in each framework in a way that seems to avoid the manifest tensions and possible outright contradictions between the two frameworks of general relativity and quantum field theory and combining them in seemingly principled ways. Thus, we can have some confidence that the new framework gets something right when extended into those new regimes.[11] Indeed, some have argued that we do in fact have some empirical access to phenomena we expect to be appropriately and adequately treated by SCG, and are even now using observations to put it to the empirical test.

1. Sudarsky and collaborators claim that their approach to inflation provides empirical support (they use SCEFE to calculate anisotropies of CMBR):

   a. "Towards a formal description of the collapse approach to the inflationary origin of the seeds of cosmic structure", Alberto Diez-Tejedor, Daniel Sudarsky, JCAP 07(2012)045, doi:10.1088/1475-7516/2012/07/045, arXiv:1108.4928 [gr-qc]

   b. "On the quantum origin of the seeds of cosmic structure", Alejandro Perez, Hanno Sahlmann and Daniel Sudarsky, Classical and Quantum Gravity, Volume 23, Number 7, doi:10.1088/0264-9381/23/7/008

   This requires, however, assuming a particular interpretation/formulation of quantum mechanics, *viz.*, dynamic collapse, so it cannot be thought of as giving straightforward empirical support to SCG

2. More generally, the apparent recovery of the anisotropies by the use of generic quantum field theory on curved spacetime in the framework of inflation may be thought to provide possible empirical support to the framework. See, *e.g.*: "Inflationary paradigm after Planck 2013", Alan H. Guth, David I. Kaiser, Yasunori Nomura, Physics Letters B Volume 733, 2 June 2014, 112–119, 10.1016/j.physletb.2014.03.020; "Semiclassical predictions regarding a pre-inflationary era and its effects on the power spectrum" Paul R. Anderson, Eric D. Carlson, Taylor M. Ordines, Bradley Hicks, Phys. Rev. D 102, 063528 (2020, 6, 15 September), arXiv:2005.12370 [gr-qc], 10.1103/PhysRevD.102.063528; "Backreaction in

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[11] In ch. xii, I shall argue that this is *too* charitable—many of those manifest tensions and possible contradictions *do* make themselves felt on closer examination, in potentially insalubrious ways.

Cosmology", S. Schander, T. Thiemann, Front. Astron. Space Sci. 8(23 July):692198, 2021, arXiv:2106.06043 [gr-qc], 10.3389/fspas.2021.692198/full

3. I shall argue in §§xvii.5–xvii.6 that the technical and conceptual problems of the attempts to apply quantum field theory on curved spacetime and SCG to inflation, and in cosmology more generally, are so severe as to call into question how much support current cosmological observations can really yield to the frameworks. In any event, even if they do provide some empirical support, they cannot provide empirical *guidance* for further theory development.
4. see also, for further grounds for skepticism, recent work by Smeenk, and, *e.g.*: "Inflationary paradigm in trouble after Planck2013", Anna Ijjas, Paul J. Steinhardt, Abraham Loeb, Physics Letters B Volume 723, Issues 4–5, 25 June 2013, 261–266, 10.1016/j.physletb.2013.05.023; "Emergence of classical behavior in the early Universe", Abhay Ashtekar, Alejandro Corichi, and Aruna Kesavan Phys. Rev. D 2020, 102(2, 15 July), 023512, 10.1103/PhysRevD.102.023512
5. possible observational support for black hole mechanics:
    a. "Testing the black-hole area law with GW150914", Maximiliano Isi, Will M. Farr, Matthew Giesler, Mark A. Scheel, Saul A. Teukolsky, arXiv:2012.04486 [gr-qc]
6. see also some possibly relevant work on trying to coax observations relevant to QG
    a. Sabina Scully, "Semiclassical Gravity: A Testable Theory of Quantum Gravity", in ed Sabine Hossenfelder, Experimental Search for Quantum Gravity, Cham, Switzerland:Springer, FIAS Interdisciplinary Science Series (FIAS), 2018, 10.1007/978-3-319-64537-7, pp 69–76, 10.1007/978-3-319-64537-7_11
    b. "table-top QG": Bose et al. (2017), Marletto and Vedral (2017), and Christodoulou and Rovelli (2019)

    Note that table-top QG, which may be thought at first glance to bear at least superficially on these matters, in fact cannot do so, for "superposed classical null-cone structures" are incompatible with SCG, relying as it does on a strictly classical spacetime geometry

In any event, the real problem is more subtle and more severe: the issue is not so much that we have no empirical data that indubitably supports any of these frameworks; rather, there is no otherwise inexplicable data to *constrain* and *guide* our theorizing. In all previous periods in the history of physics when, for whatever reason, a pressing need was felt to develop new theories and new frameworks, as happened with Newton's construction of his framework of classical physics generally and in particular with the development of his theory of gravity, with Maxwell's unification of electric and magnetic theories into his theory of the electromagnetic field, with Einstein's development of special and general relativity, with Planck's, Einstein's, Bohr's, Heisenberg's, Schrödinger's, Dirac's, *et al.*, development of QT—in all these historical episodes, development of novel theory was driven, constrained and guided by compelling experimental data that then-current theories could not accommodate. We simply do not have that today.[12]

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[12] See Curiel (2001) for detailed discussion, and [*** bibtex: Lee Smolin, The Trouble with Physics, Boston: Houghton Mifflin Harcourt, 2006 ***] which picks up and expands on several themes from that paper.

[*** Finally: discuss in more detail the problem of epistemic order that I raised in §VI.1 ***]

VI.8 Bringing It All Back Home

[*** summary ***]

VI.9 Concluding Scientific Philosophical Postscript

Discuss highlights of philosophical literature, how they bear on the arguments of this chapter, concerning:

1. interpretation, semantics of physical theory:
    a. Curiel (2009, 2017c), and stuff discussed therein (Carnap, Suppes, Putnam, Lewis, da Costa and French, van Fraassen, Stein)
    b. . . .
2. empirical content of theoretical terms
    a. Maxwell (1869, 1870, 1871, 1876a, 1876b, 1879)
    b. Curiel (2022)
    c. . . .
3. natural kinds, species of systems:
    a. Curiel (2016, 2017b)
    b. . . .
4. relevant notions of similarity, in particular Goodman (1972) on similarity (it's just too delicious not to), and for linguistic/semantic analysis:
    a. Frege, Russell
    b. Goodman's "Likeness of Meaning", Analysis, Vol. 10, No. 1 (Oct., 1949), pp. 1–7
    c. Carnap
    d. Putnam's 1975 '`same`$_{\mathtt{L}}$', for instance, will not do (Curiel 2016)
    e. some more recent philosophy of language
5. concept development and extension:
    a. Hertz (1899, "Introduction")
    b. Peirce
    c. Nersessian (2002, 2008)
6. inter-theory relations:
    a. Butterfield (2011)

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