Schematizing the Observer and the Epistemic Content of Theories, or Getting the Laboratory inside the Theory

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So I suggest that the principal difficulty is not that of how to leave the theory outside the laboratory door, but that of how to get the laboratory inside the theory. ¶ Well, how do we do it? For of course we do put theory and experiment in relation to one another; otherwise it would be impossible to test theories, and impossible to apply them. It would also, I should add, be impossible to understand a theory, as anything but a purely mathematical structure—impossible, that is, to understand a theory as a theory of physics—if we had no systematic way to put the theory into connection with observation (or experience).

Stein (emphases his)
"Some Reflections on the Structure
of Our Knowledge in Physics"

I am grateful to Smeenk for having introduced some of the themes I'll be focusing on today, and in particular the paper of Howard's I am most inspired by and rely on here, in his characteristically elegant, equable, measured, methodical and restrained way. I will now pick them up in a frowzy, frenetic, unrestrained, haphazard and mildly hysterical way.

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What is the theory of general relativity? Here is one way to make that question somewhat more precise: what is the more or less secure fund of scientific knowledge that constitutes the theoretical and experimental content of the theory? This formulation of the question is certainly not a standard one in philosophy of science. On that standard view, the emphasis is almost invariably on constructing formal models corresponding to something like solutions to the field equations or equations of motion of a theory, and perhaps asking when two such families of models (or other formal structure) are isomorphic in a (supposedly) relevant sense, with perhaps a vague gesture at the possible "representational capacities" of the formalism as an afterthought.

I do not like a formulation of the question that admits as an appropriate answer such a purely formal structure, along with perhaps the fixing of something like a Tarskian semantics or a sketch of representational capacities. A formulation of that kind assumes, with no argument ever given, that it is possible to make a clean separation between, on the one hand, one part of the scientific knowledge general relativity embodies, *viz.*, that encoded in the pure formalism and, on the other, the remainder of that knowledge.

The remainder of the theory's knowledge includes at a minimum what is encoded in the practice of modeling particular systems, of performing experiments, of bringing the results of theory and experiment into mutually fruitful contact—including all the *in*exact mathematical techniques that cannot be formalized, approximative and heuristic techniques motivated by loose physical arguments and principles not part of the formalism or of interpretive principles, and justified only by practical success—including, in a word, real application of the theory in actual scientific practice.

This assumption comes out most clearly in the picture of semantics that naturally and standardly accompanies the standard view of theories: semantics is fixed by ontology, and all epistemology and methodology and other practical issues and considerations are segregated into the theory's pragmatics. We should not assume such a clean separation is possible without an argument.

If one accepts my formulation of the question as fruitful and interesting, then it becomes plausible that the theory of general relativity does not—can not—in any interesting sense consist of the theory of Lorentzian 4-manifolds along with a few interpretive postulates or a family of Tarskian models or a class of possible worlds. Now, it may turn out that a compelling analysis attempting to answer my formulation will show that, in the end, the standard assumption of the clean separation of the relevant domains of knowledge—the "theoretical" and the "practical", for lack of better terms—is a good and justified one. I personally doubt it.

I will argue that the interplay between theory and practice (experimentation, observation, modeling, heuristic and approximative forms of argument, ...) is far more subtle, nuanced, complex and rich than philosophers tend to appreciate. The sum scientific knowledge constituted by the theoretical and practical content of the theory—which for lack of a better term I'll call the "epistemic content"—essentially includes knowledge we have gained, and more importantly could have gained only, through practical modeling, experimentation and observation, heuristic and approximative techniques of argument, and in particular the application of representations and models of such experiments and observations in our theory by use of heuristic and approximative techniques of argument. The epistemic content—that sum of knowledge—moreover, cannot be cleanly separated into a formal and a practical part.

This is why I think my formulation of the question based on the idea of knowledge is a fruitful sharpening and refinement, for it is exactly this epistemic content that distinguishes general relativity as a *physical* theory from a merely mathematical structure. I will further argue that a crucial component of that content consists in knowing how to—in Stein's phrase—"schematize the observer" in theoretical representations of experiments, and cannot be had without models of experiments in which the observer is schematized.

By "schematize the observer" I mean something like: in a model of an experiment, to provide a representation of something like a measuring apparatus, even if only of the simplest and most abstract form, that allows us to interpret the model *as* a model of an experiment or an observation. Such schematic representation, moreover, is not possible in a physically significant and cognitively substantive sense without recourse to all the other components of what I am calling the practical part of scientific knowledge.

This is what is needed in practice to bring theory and experiment into fruitful contact with each other, most importantly in the way that warrants our belief that the formal part of the theory represents, is about, the physical world. Interpretive principles and sketches of representational capacities on their own cannot—they themselves must be justified first.

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Because the meaning of scientific terms and propositions must rest on the knowledge we have of the physical world, and most of all on the knowledge we have gained through controlled observation and measurement, that is, through experiment, epistemic content accrues to a scientific theory in no small measure through the successful construction of representations of physical systems in the theory's terms. At bottom, then, what secure epistemic content a scientific theory has must rest on the meanings expressed in the sound articulation of experimental knowledge. This requires at a minimum that we be able, at least in principle, to construct appropriate representations of actual experiments and observations in the frameworks of our best scientific theories, that is, representations of physical systems and experimental apparatus in relation to each other as required by particular, actual experiments, not just representations of physical systems *simpliciter*, in abstraction from actual experiments.

It is characteristic of the ways that such models are constructed and applied, moreover, that they require methods and warrant—forms and aspects of knowledge—that go far beyond what can plausibly be claimed to reside in the formally theoretical sphere of a theory, no matter how many representational capacities one ascribes to it or interpretive principles one affixes to it.

Interpretive principles of the sort envisaged by the standard view can at most most fix only a relation of descriptive representation. What is primarily at issue here, however, is not the relation of representation itself, which can at most tell us "what the world would be like if the theory were true or largely true of it", but rather the *understanding* and *comprehension* the theory gives us of the world in so far as we are warranted in thinking of those models *as* representations, which goes far beyond a description of how the world would be under those conditions.

This can be seen clearly in the fact that, for essentially every physical theory we have (Navier-Stokes theory, Newtonian gravitational theory, quantum field theory, general relativity, *etc.*—*cf.* JB's remarks on how little we understand the distribution of hole-freeness), we have very little detailed knowledge of the structure of generic solutions. Usually, we know exact solutions only under conditions of perfect or near-perfect symmetry or some other unrealistic assumption (two bodies, no external perturbative influences, ...), and then argue that we can apply such solutions to real physical systems, because the approximation is adequate under appropriately controlled circumstances. That is to say: we have no real idea *at all*, in a representational sense, "what the world would be like if the theory were true or largely true of it", for any physical theory. This becomes particularly clear when one stops to think, to really think hard, about the mind-blowing complexity and richness of texture of real physical systems in the world.

Consider this glass of water: the little eddies and whorls of fluid motion that we can't see because of minuscule temperature and pressure gradients, the suspended particulate matter (dust, schmutz), the air bubbles affixed to the side of the glass by its impurities and inhomogeneities, the evaporation of molecules off the surface into the air and the adsorption of other molecules from the air, the hyper-dense, stereometrically complex network of Hydrogen bonds with extraordinarily rich characteristic symmetry patterns as web to the weft of instances of dozens of species of ionic combinations of $\mathtt{H}$ and $\mathtt{O}$ ($\mathtt{H_2O}$ being not even a majority, only a plurality), the staggering variation in structure of the electromagnetic field at every point of the fluid (the different radio channels from the same antennae playing Berg's "Lulu" and the BBC news, all the signals from all the mobile phones all over the planet, the gamma ray bursts from distant astronomical cataclysms, . . .), and on and on and on—and yet our naively, ludicrously simple models *work*—they *really work*.

I think we forget in philosophy, we overlook, lose sight of, how complex real physical systems are and what a miracle it is that our almost naively simple-minded theories can still capture them. And correlatively, we forget how much distance there is between those very pristine models we actually do know in any given theory and the real physical systems they purport to represent, and so we forget how many of those models would almost certainly do the representational tasks required, if one of them does at all—how many of those models that we have no idea of how to construct in any way graspable by the human mind, or indeed even how to identify if someone gave us one gift-wrapped.

In the end, therefore, the idea of the standard view, that the theory is characterized by the formalism in something like the sense of the family of solutions to its dynamical equations (Tarskian models, possible worlds, what have you), comes down to two options:

1. we stipulate by fiat that there are a family of actual (or possible, in some sense) physical systems, though we cannot pick them out in practice, that are the ones represented by the solutions, and those are the models
2. we stipulate that the solutions themselves (*e.g.*, vector fields on phase space), though we cannot represent them in anything like closed form or even identify one when it bites us on the ass, are the models

In both cases, we are saying that the overwhelming majority of the content of the theory is something that we do not know, are in fact nowhere near knowing, and have good reason to think we will never know in anything remotely resembling thoroughness and detail. Nonetheless, the story goes, there is a mystical relation of subsistence between the formalism and the models that univocally and unambiguously fixes them; that in turn gives rise to a magical relation of "reference" that zaps out from the formalism to the world and latches on to the salient physical systems in a physically significant way. We, in our supernal cognitive puissance, just *know* this.

We are talking here of a Fichtean faculty of pure intellectual intuition that grasps without cognitive or perceptual mediation the Kantian *Ding an sich*. Or more precisely, we are talking of nothing at all. A semantic relation that is not known and cannot be known by humans in our actual current epistemic circumstances has no possible use in actual science and has no possible bearing on a fruitful and illuminating analysis of scientific knowledge.

My rhetoric may make it sound as though only the realists are subject to my wrath, but that is not so. I intend my wrath to be shared among all philosophers beholden in any way to something like the standard view, whether of a realist or an anti-realist bent. What is this fairyland of formal models that are the semantic tools the instrumentalist uses to make his predictions? (*Cf*. Howard's remarks on making instrumentalism "sophisticated" in "Yes, but…".)

All those who hew to something like the standard view, no matter how they think of the character of the relation between a theory and the world, have the same problem: what is the family of formal structures that "characterizes" the theory? That is to say, which formal structures of the theory are even such as to be appropriate to serve as adequate representations of parts of the world, and how can we know this, given our woeful lack of actual knowledge about the details of the formal structures? *What is the theory?*

In order to address this problem, we need a mechanism or device for bringing a mathematically characterizable situation in the world of experience into substantive, physically significant contact with the mathematical structures of our theories, an intermediary that identifies the junctions where meaningful connections can be made between the two and embodies the possibility of the epistemic warrant we think we derive from such contact and connection, especially when we have access only to the most minimal, pristine and physically unrealistic of solutions to the theory's equations. The schematic representation of the observer in models of actual experiments plays that role.

[start of Stein quote]

> *In actual fact, the experimental physics is treated separately as a discipline in its own right, that is partly an art: an affair of both knowledge and manipulative and perceptual skill. But the possibility of connecting this art with the theory is closely connected with a certain possibility within the mathematical structure that is the theoretical framework: using a word I have introduced earlier, the possibility of representing experiments, and of representing the observer, "schematically." . . . I want to speak . . . of "schematizing the observer within the theory"; . . . the intention is . . . to secure empirical content—content within experience—for an abstract structure.*

[cont.]

¶ . . . [S]o far as the fundamental theory is concerned—or rather, so far as mathematically defined structures and rigorous arguments are demanded—the "schematic" representation of observers, experiments, and observations, is, I believe, as far as we know how to go.

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The standard view gets right the fact that the family of systems a theory adequately and appropriately treats is a central, essential component of the epistemic content of a theory. To identify the family of systems, we need to demarcate the regime of applicability. To do that, we need to fix error tolerances, acceptable levels of precision, and so on; to do that, we must make use of non-rigorous mathematical techniques and heuristic arguments, *etc.*, all of which cannot be represented, much less accommodated, within the standard view. To do that, indeed, we need real models of actual experimental techniques, representing the actual tools we have at our disposal for performing such measurements, for a theory's regime of applicability changes over time with advances in practical knowledge.

If demarcating the regime of applicability were not dependent on practical knowledge of the kind that changes—the current reach of experimental technique that determines fixed levels of precision and accuracy, and so on—then there would be a fixed number, once and for all—say 3.14159265359$\ldots$x10^{-6} cm—at scales smaller than which Navier-Stokes theory breaks down. But that is simply false.

Navier-Stokes theory breaks down at different places for different types of fluids under different conditions, and the increasing acuity and resolution of our measuring instruments, in conjunction with our increasingly powerful control of experimental error, both practically and experimentally, and the increasing sophistication of the models we can construct of our measuring devices and the fine details of their interactions with different Navier-Stokes fluids under different conditions, all mean that such numbers become ever smaller over time. They are not fixed *sub specie æternitatis*. (This is a simplification—one can allow, say, a fixed function of shear viscosity, bulk viscosity, thermoconductivity, pressure, shear-stress, and so on, rather than a fixed number—but the point is the same, that such a thing can be determined only experimentally, not by unchanging formal theory alone.)

Thus, neither is the family of physical systems a theory adequately and appropriately treats fixed *sub specie æternitatis*. I think this is right. A scientific theory is not a Platonic form. It does not immutably exist, independently of our actual epistemic state.

Thus, it is only by dint of the practical forms and aspects of scientific knowledge not captured by a theory's formal machinery that we can determine a theory's regime of applicability. Knowledge of that regime, however, embodies perhaps the most important part of the epistemic content of a theory—without it, we have no warrant at all for trusting our use of the theory, for understanding it *as* a theory of a particular class of physical systems, and for knowing which individual systems that seem to be of that kind, in some intuitive sense (Mercury, for Newtonian gravitational theory; this glass of water shaken enough to approach turbulence, for Navier-Stokes theory), are yet not adequately and appropriately treated by the theory (for some purposes).

There is also the contrary problem: to know what solutions and models of a theory's equations to take seriously ("it needs a lot of intuitive physical sense to know when to expect actual things to behave like the idealized models we make of them", Synge, *Relativity: The Special Theory*)—to determine which solutions and models of a theory's equations represent in an adequate and appropriate way real physical systems, and which parts of those solutions and models that do represent real systems represent real parts of them.

This is not the problem of "gauge". Consider, again, Navier-Stokes theory, a continuum theory—the solutions to its equations allow one to make, indeed they *necessarily encode*, physical predictions at arbitrarily small spatial and temporal scales; but we know that it is not appropriate for that task, and we know we can safely ignore those "predictions" of the theory in assessing its adequacy and soundness. General relativity is scaleless—one cannot distinguish a model of a Schwarzschild black hole of radius $10^{-10^{39876}}$ $\mathtt{cm}$ from one of radius 88 million $\mathtt{km}$. And yet we also know that the theory is appropriate only for treating the latter, not the former. Nothing in the theory's formalism can tell us this, without the input of knowledge that can be had only by experimentation.

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The standard view has another, related shortcoming—that, according to it, all scientific knowledge is essentially of one undifferentiated kind: true (or at least epistemically justified) propositions about states of the world, and perhaps about the global or nomic structure of the world independent of the state the world is actually in (which may, *e.g.*, be used to try to represent modal structure such as is captured by subjunctive conditionals). This comes out most clearly, again, in the picture of a theory's semantics that naturally accompanies the standard view of theories, that it is fixed by a determinate and pristine ontology.

This is not an accurate picture of scientific knowledge. Scientific knowledge has many forms and aspects that cannot be well captured with this simple picture. The structure of the knowledge that characterizes a theory as a *physical* theory of the types of physical systems it purports to treat must include *all* forms and aspects of knowledge the theory has endowed on us. A semantics that captures only knowledge—epistemic content—it is impossible in principle for humans to know is useless as a semantics on which to ground an analysis of the nature of scientific knowledge and its epistemic warrant. If the semantics does not do this, however, then what *does* it do? And why should we care about it?

A useful classification of what I have been calling the theoretical and the practical forms of scientific knowledge is suggested by the following observation of Stein ("Some Reflections...", emphases his):

> *It is hard, but possible, to learn theory by self-study from books; it is surely much harder to learn experimental techniques without a teacher to help one acquire skills; but what I suspect to be impossible is to learn the principles of experiment without* <u>*actual*</u> *experience with the relevant* <u>*instruments*</u>*.*

How one is able to learn different parts of the epistemic content of a theory may seem at first glance irrelevant to an analysis of the character of the knowledge to be learned. To the contrary, I think Stein has put his finger on something very deep about epistemic content: how it is possible to come to learn something fundamentally shapes and constrains the kinds of epistemic warrant we can have for it, and how we may bring it to bear in possible evidential relationships to other pieces of potential knowledge.

Theoretical knowledge of the sort Stein describes here in the first part of the quote has the peculiar and remarkable property of being representable by a well arranged sequence of propositions (a well written book). If it did not have that property, we could not learn it from such a sequence. Practical knowledge cannot be effectively represented so.

Sketch anecdote: during the running of the Event Horizon Telescope observations, I witnessed the following exchange between Shep and an instrumentalist at the Chilean observatory (3000 meters above sea level) who was having trouble with one of the specially designed instruments. A crystal transistor was malfunctioning. Shep said, "Oh, yeah, I've see this before. Here's what you do. Take'em down to 1000 meters [above sea level], and leave'em for a few hours; they'll settle down, and then when you take them back up, they'll work." Chilean instrumentalist: "Why does that work?" Shep: "I have no damn idea, but it does." This is a part of the practical knowledge associated with observational relativistic astronomy that I doubt anyone will ever find written in any book. Moreover, even if written, it is the sort of thing one could not *learn* from just having read the book. Of course, one could learn that particular trick—but to learn how to *play* with the instruments so as to be able acquire the feel for them necessary to figure out such tricks for solving problems—that is something that cannot be learned from a book, as Stein emphasized so beautifully in the passage I quoted.

I think this lesson generalizes: how it is possible to learn different pieces or areas of scientific knowledge is one very important criterion for determining the kind of knowledge one is dealing with—how it can be represented, among other things—and thus whether it can, *e.g.*, be captured by formalism and "coordinating principles" or "representational capacities".

The practical form of knowledge of the art of experimentation, inextricable from the epistemic content of the theory one considers, can not be formalized or even just well captured in book form; most theoretical forms of knowledge can. The latter is, to speak crudely, propositional in a way the former is not. That does not, however, *ipso facto*, imply that it is more important for a proper understanding of the character and content of the sum of knowledge that a theory embodies, that characterizes it as a *physical* theory of *that* sort of family of physical systems.

Geroch, *General Relativity from A to B*:

> *One might very well be left with the impression that the theory [of general relativity] itself is rather hollow: What are the postulates of the theory? Where are the demonstrations that all else follows from these postulates? Where is the theory proven? On what grounds, if any, should one believe the theory? I can only answer these questions with my own opinions. It seems to me that "theories of physics" have, in the main, gotten a terrible press. The view has somehow come to be rampant that such theories are precise, highly logical, ultimately "proved." In my opinion, at least, this is simply not the case—not the case for general relativity and not the case for any other theory in physics. First, theories, in my view, consist of an enormous number of ideas, arguments, hunches, vague feelings, value judgments, and so on, all arranged in a maze. These various ingredients are connected in a complicated way. It is this entire body of material that is "the theory." One's mental picture of the theory is this nebulous mass taken as a whole.*

(cont.)

In presenting the theory, however, one can hardly attempt to present a "nebulous mass taken as a whole." One is thus forced to rearrange it so that it is linear, consisting of one point after another, each connected in some more or less direct way with its predecessor. What is supposed to happen is that one who learns the theory, presented in this linear way, then proceeds to form his own "nebulous mass taken as a whole." The points are all rearranged, numerous new connections between those points are introduced, hunches and vague feelings come into play, and so on. In one's own approach to the theory, one normally makes no attempt to isolate a few of these points to be called "postulates." One makes no attempt to derive the rest of the theory from postulates. (What, indeed, could it mean to "derive" something about the physical world?) One makes no attempt to "prove" the theory, or any part of it. (I don't even know what a "proof" could mean in this context. I wouldn't recognize a "proof of a physical theory" if I saw one.)

We get misled in philosophy by a picture—we fixate, I think, on how we are *taught* the physics, but we forget how we *learned* it, and most of all how we actually know that we are warranted in believing it—believing it, that is, to be appropriately and adequately about some part of the physical world.

I also distinguish, again following Stein ("Some Reflections. . .", emphases his), among different aspects of scientific knowledge:

> *I am construing the word knowledge in a wide and ambiguous sense. The reflections I am proposing have as their object (a) our knowledge in physics as an achieved result: knowledge as the knowledge we have of X; (b) our knowledge as susceptible of justification or defense—that is, as involving a structure of "evidence" for its asserted contents; and (c) knowlege—science—as (to appropriate a word of Isaac Levi's) an enterprise: an activity aimed at increasing our knowledge in sense (a), by means appropriate to the constraints of (b).*

I would add to this list a fourth, closely related to, but distinct from, Stein's (b): knowledge as ground for epistemic warrant for other knowledge claims and for furthering the enterprise of science. Call this '(d)'. (Not all knowledge in its aspect (a) can play this role—*cf*. Smeenk's description of the understanding of FLRW models in the 1930s.)

The standard view, I claim, captures at most one of these aspects, knowledge as achieved result, and that at best only in part. The standard view's picture of the knowledge content of a theory, entirely captured by indicative propositions about the state and nature of the world (and perhaps subjunctive ones about possible states of the world), does now allow one even to ask how such claims are justified, how their justification depends on the knowledge we have in virtue of other theories, the limitations on the scope of such claims, and so on, for the practical knowledge I have been discussing informs and provides much of the body of the theory's knowledge in all these aspects, and the standard view does not allow this knowledge to be represented. Thus, it cannot explain the epistemic warrant we have for trusting and using the theory, and thinking that the understanding and comprehension it seems to give us of the world is *actually about the world*.

In another sense, however, the standard view gives too much as an answer to the question of the scope of a theory's epistemic content—for it says that every question one can pose to the theory has a determinate answer. In a purely formal sense, this may be correct. But if one wants to know whether the answer given is *relevant to the real world*—whether the answer counts as scientific knowledge in the senses, for instance, of Stein's (b) and (c), and my (d)—then one has to know, among other things, the scope of the theory's regime of applicability, *i.e.*, the spatial, temporal and energetic scales (*e.g.*) in which it is a good theory and those where it breaks down. For that, one must have recourse to knowledge that one can come by only by experimentation, and the theoretical representation of such experimentation that includes schematic representations of the observer.

In so far as this essentially represents knowledge in Stein's aspect (b), moreover, it cannot be given as physical interpretive principles in any standard sense, *i.e.*, one that encodes knowledge cleanly separable from any knowledge represented purely by the theory's formalism: our knowledge of where and how Navier-Stokes theory, *e.g.*, breaks down is in large part based on and justified by experimental results as represented in and put into meaningful contact with the mathematical formalism.

The standard view, however, is deficient, I claim, even with regard to knowledge of aspect (a) (knowledge as achieved result), in so far as it precludes fundamental understanding and comprehension we gain about parts of the world that can be had only by heuristic, informal arguments that cannot be formalized. A good example is the way that the kinematical constraint of Navier-Stokes theory that heat flux is always orthogonal to fluid flow encodes the fact that Navier-Stokes theory is valid only for fluids quite close to hydrodynamical and thermodynamical equilibrium of a certain kind.

The best arguments I know of for this are, strictly speaking, incorrect in parts, incoherent as a whole, and even inconsistent in several ways (all with respect to the formalism of the theory). Nonetheless, they serve the purpose adequately in practice, the sketching of a picture of part of the epistemic content of a theory of a genus of physical phenomena (Navier-Stokes fluids), a picture that in essence conveys the important physical features of these structures. In particular, it conveys the kind of understanding and comprehension Navier-Stokes theory gives us of the systems it treats. The nature of the arguments, moreover, are such that they manifestly could not be made or had sense given to them strictly within the confines of the formal apparatus of the theory and any epistemic content that could accrue to it from a clean semantics based on ontology alone.

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The three fundamental claims of the talk,

1. that a theory is best characterized by its epistemic content,
2. that the epistemic content consists of theoretical and practical components that are not cleanly separable,
3. and one cannot represent a theory as a theory about the world without knowing how in practice to be able to schematize the observer in theoretical models

are, for me, themselves not cleanly separable, but hang together.

The miracle of science is that theory and experiment are consonant with each other; the necessity of science is that they are inextricably so—not, however, as equals. Theory plays Boswell to the subtle and tragic clown of experiment's Johnson.