Synopsis and Discussion

Underdetermination in Science

Saturday-Sunday, 21-22 March 2009
Center for Philosophy of Science
University of Pittsburgh
Pittsburgh , PA USA

"Getting Real: Underdetermination and the Hypothesis of Organic Fossil Origins"
Kyle Stanford, Dept of Logic and Philosophy of Science, University of California, Irvine

"Resisting Underdetermination: Caught Between the Data and the Deep Blue Sea"
David Harker , Dept of Philosophy and Humanities , East Tennessee State University

"Underdetermination, Methodological Practices, and the Case of John Snow"
Dana Tulodziecki , Dept of Philosophy , University of Missouri , Kansas City

"Must Evidence Underdetermine Theory?"
John D. Norton, Dept of HPS and Center for Philosophy of Science, University of Pittsburgh

"What is a 'Physically Reasonable' Spacetime?"
John Manchak, Dept of Logic and Philosophy of Science, University of California, Irvine

"The Identical Rivals Response to Underdetermination"
P. D. Magnus, Dept of Philosophy, State University of New York, Albany

"On the Roots and Consequences of Underdetermination"
Nicholas Rescher, Dept of Philosophy, University of Pittsburgh

Open discussion. Editorial response to Kyle Stanford "The Underdetermination of Scientific Theory"
Greg Frost-Arnold, Dept of Philosophy, University of Nevada, Las Vegas; J. Brian Pitts, University of Notre Dame; and the speakers.

Program Committee
John D. Norton and Kyle Stanford

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Posted March 27, 2009

Underdetermination Conference Center for Philosophy of Science, March 2009

Greg Frost-Arnold

The following comments are not intended to serve as a summary of last weekend's proceedings. My remarks reflect only my idiosyncratic interests, and I have only commented on the first four papers.

1. *Kyle Stanford*

Kyle Stanford offered a criterion to distinguish scientific theories that are susceptible to the Problem of Unconceived Alternatives (PUA) from those that are not. This is important, because he does not want to be an anti-realist about all scientific hypotheses *tout court*. One such hypothesis is that fossils have organic origins. Kyle suggested that we should not be anti-realists about hypotheses for which we have *projective inductive* evidence; however, if we have merely abductive evidence for a hypothesis, then we should be anti-realists about it, since it is vulnerable to PUA.

A question that Kyle heard (from others and me) is: what is the evidence that even projective induction is safe from the PUA? Here is my form of the worry. When we make a projective induction, we have to assume that our limited past evidence is *relevantly similar* to the whole population under investigation. But we often don't know whether our sample is relevantly similar to the entire population—we don't know which variables are relevant, and furthermore we may not even know that certain relevant properties exist (for example, if such properties had not yet been discovered). So it seems that even projective induction may not be safe from the PUA. Kyle offered a response in his closing remarks: safety from the PUA may not be a binary matter—perhaps there is a continuum, in which projective inductions are more safe than mere abductions, but not immune *simpliciter*.

2. *David Harker*

David Harker presented a way to limit the damage purportedly done by strong underdetermination claims. He focused on defending the realist claim that theories *progress*, and bracketed the question of whether theories are approximately true.

The part of Harker's proposal most interesting to me was the claim that his version of realism makes a prediction that cannot be had without assuming his version of realism: the parts of past theories responsible for empirical progress are preserved across subsequent episodes of theory change. This was of particular interest to me, because I have argued that the no-miracles argument is, despite realists' claims, an inference to a *bad* explanation, in part because adding (certain) realist theses to our current scientific theories generates no new predictions.[1] Some audience members wondered whether this prediction really is borne out by the data (viz. the history of science). I have another question: couldn't/ wouldn't the constructive empiricist make exactly the same prediction? That is, why wouldn't the constructive empiricist say that the parts of theories preserved across change are the very parts responsible for empirical success? And if

[1] "The No Miracles Argument for Realism: Inference to an Unacceptable Explanation," *Philosophy of Science* (forthcoming); draft: http://faculty.unlv.edu/frostarn/RealismLimitsOfExplanation.rtf

realism and anti-realism make the same prediction, then of course the fact that realism makes that prediction is no evidence for it over anti-realism.

3. *Dana Tulodziecki*
Dana Tulodziecki presented a nice case study on John Snow showing that scientists admit other rules and standards of theory-choice besides 'save the phenomena,' and that when these other rules and/or standards are considered, then it will be much more difficult for anti-realists to construct *equally good* theories.

Looking back, I have two questions:
(a) How does this proposal differ from or extend upon Laudan's in "Demystifying Underdetermination," where he suggests that if we allow ourselves canons of ampliative inference as well as deductive inference, then the apparent rivals to current theories will be eliminated?
(b) For the sake of the argument, imagine that there is no underdetermination: we somehow manage to settle on some set of practices and standards for scientific reasoning such that for any body of evidence, there is one and only one extant theory that is most supported by it (put otherwise, imagine that there is a function from data sets to current theories that always picks out the most justified theory). My question is: would this be any help against Stanford's New Induction?

4. *John Norton*
I am also curious how John Norton would answer the above question (3.b) as well.

Also, I wanted to register one difference of intuitions between John and me—though the fact that John has an intuition counter to my own makes me weight my own less. He suggested that, in observational astronomy (c. 1543), Ptolemaic and Copernican models are merely notational variants of one another, or at least, all the differences between the two models are *surplus structure*. I do not share this intuition. Why? I can see why gauge quantities, absolute position, absolute time, and absolute velocity can be viewed as surplus structure: there is an independently motivated picture of the natural world in which these entities do not appear. There is not (c. 1543) for the Ptolemaic and Copernican models. I recognize and understand that, for the purposes of capturing the astronomical data within acceptable observational error in the 16th Century, the answer to the question 'Is the Earth moving or not?' can be either yes or no. But that, an anti-realist should say, is *exactly* what we should expect in a paradigmatic case of underdetermination: the evidence available to us does not favor one hypothesis of the Earth's motion over another.

(This is in part why, during the Q&A, I asked for an independent characterization of surplus structure. If Norton's response to almost all examples of apparently equally well-confirmed rivals is 'It feels like surplus structure to me,' then the proponent of underdetermination will likely accuse Norton of defending his thesis from potential counter-evidence in an unmotivated, *ad hoc* way—analogous to anti-evolutionists denying transitional forms by declaring any apparently disconfirming instance, e.g. *Archaeopteryx*, to be a non-transitional form, e.g. a bird or a reptile.)

Posted March 27, 2009

J. Brian Pitts
Invited Discussion of
Underdetermination in Science, 21-22 March 2009
Center for Philosophy of Science, University of Pittsburgh

This is a personal summary of and response to the underdetermination workshop. Given limits of space and my competence, I will not be able to comment on every talk. The comments by Frost-Arnold remove that deficiency; I thank him for clarifying comments and questions also.

Some speakers, including Norton[2] and Magnus,[3] are skeptical about any global underdetermination problem in science. Norton's willingness to dismiss as frivolous such candidates as might be developed by gratuitous mutilation presupposes that we are rationally entitled to rather strong principles of inductive inference. He finds that our shared judgments of particular warranted inductive inferences apparently irreparably outstrip those that can be justified through serious arguments, however.[4] A plausible response to this situation might be to adopt a (partly) externalist epistemology, so that the lack of explicitly available justification does not imply a lack of justification. Rescher's invocation of C. S. Pierce's account in which we automatically discount some hypotheses due to mental processes tied to our origins might be an example, as is Magnus's invocation of Thomas Reid. A constraint on an account of these inductive inferences is that it not be self-refuting.

Even if there is no global problem, there might still be important local problems of underdetermination. Fundamental physics had flourishing cases of underdetermination into the early 1970s.[5] [6] These cases involve, for example, a rivalry between a theory with massless photons (the quantization of Maxwell's theory) and a one-parameter family of theories with massive photons (attributed to Proca), the parameter being the photon mass. Physicists, who individuate theories less finely, would see this as a rivalry between massless photons and massive photons, with the further question of ascertaining (or bounding) the photon mass. The rivalry, which is immune to trivialization by Norton's identical rivals strategy, is also permanent, because no experiment can show that the photon mass is zero rather than just suitably small. The massive theories are not gauge theories, so there is much at stake conceptually in the rivalry. Electro-weak unification seems to favor the massless theory, however.

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[2] John D. Norton, "Must Evidence Underdetermine Theory?" in Martin Carrier, Don Howard and Janet Kourany, editors, *The Challenge of the Social and the Pressure of Practice: Science and Values Revisited*, pp. 17-44, University of Pittsburgh Press, Pittsburgh (2008).
[3] P. D. Magnus, *Underdetermination and the Claims of Science*, dissertation, University of California at San Diego (2002), http://dspace.sunyconnect.suny.edu/bitstream/1951/42590/1/dissertation-lulu.pdf
[4] John D. Norton, "The Inductive Significance of Observationally Indistinguishable Spacetimes," PhilSci.
[5] J. Brian Pitts, "Permanent Underdetermination from Approximate Empirical Equivalence in Field Theory: Massless and Massive Electromagnetic, Yang-Mills and Gravitational Theories," under review.
[6] David G. Boulware and Stanley Deser, "Can Gravitation Have a Finite Range?" *Physical Review D* **6** (1972), p. 3368.

Other sciences might have different sorts of handicaps. Whereas Stanford argues from the fossils case that historical sciences might have a special advantage, Derek Turner[7] has argued that they have special liabilities. These claims, if not hastily generalized, need not conflict. In any case the importance of distinguishing historical sciences from experimental sciences is clear. It was suggested that politically relevant sciences, such as climatology, might also pose special challenges. Perhaps the urge to immediately apply the science is the cause. Controversy seems not to arise in most engineering sciences in the way that it does for fundamental physics and various ideologically significant sciences. If we should not accept a global underdetermination thesis, neither should we rush into a global *determination* thesis based on the safest cases.

Matters of politics and historical science both arise in the issue of creationism, which came up repeatedly in Norton's papers and in informal discussion. It is routine to dismiss as radical skepticism any proposal that conflicts with induction, as Stanford does. However, it was long routine to accept that some part of Noah's Flood was physically impossible, but happened anyway, because the Bible is true, so God must have performed a miracle;[8] this view was challenged only when progressive Protestants such as Thomas Burnet in the late 17th century strove harder to make sense of the Flood without miracles.[9] [10] (They failed.) The Bible is full of cases where it is deemed appropriate to make *local* exceptions to induction in favor of trusting divine revelation. Non-Whiggish history of science therefore requires noting that what counts as a merely skeptical rival can be historically mutable. While Norton's response to the supposed novelty of Goodman's new riddle of induction might well suffice,[11] Hume's old problem of induction is untouched. Norton takes Bayesian accounts of confirmation to be the least friendly to underdetermination.[12] But prior probabilities are infamously subjective. In polemical contexts involving creationism, there might be little or nothing scientific to draw upon (if empirical agreement with mainstream science is built in) other than different priors (widely shared but of unclear origin) to favor ordinary geology over a Gosse-style young Earth created with fossils around 4004 B.C. or a scenario (perhaps never proposed) with miraculous fossil-sorting in the Flood. Bayesian confirmation theory is then reduced to an ornate receptacle for the intuitions that such creationist strategies are implausible; thus Norton has difficulty in *arguing* for induction in the form of requiring space-times to be maximally extended---that is, that the history be as long as possible.[13] (The problem of irrelevant conjunction or "tacking" is also relevant here: the relevant hypothesis is the empirical adequacy of standard geology, the irrelevant one

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[7] Derek D. Turner, *Making Prehistory: Historical Science and the Scientific Realism Debate*, Cambridge University Press, Cambridge (2007).
[8] Walter Charleton, *The Darknes of Atheism Dispelled by the Light of Nature* (1652).
[9] Don Cameron Allen, *The Legend of Noah: Renaissance Rationalism in Art, Science and Letters*, University of Illinois, Urbana (1963).
[10] John Keill, *An Examination of Dr. Burnet's Theory of the Earth: together with some remarks on Mr. Whiston's New Theory of the Earth* (1698).
[11] John D. Norton, "How the Formal Equivalence of Grue and Green Defeats What Is New in the New Riddle of Induction," *Synthese* **150** (2006), p. 185.
[12] John D. Norton, "Must Evidence Underdetermine Theory?"
[13] John D. Norton, "The Inductive Significance of Observationally Indistinguishable Spacetimes."

the world's actually being only *c*. 6000 years old. The irrelevant hypothesis does *not* get confirmed by the fossil record even if the conjunction does, at the expense of the claim that the world is *and looks* young.) Gerhard Schurz's project of providing a non-question-begging defense of exceptionless induction against "esoteric worldviews"[14] to address creationism is therefore noteworthy. His argument, however, requires the premise that exceptionless induction has outperformed all esoteric worldviews in the past.

Manchak argues using global methods in General Relativity that data can never exclude exotic global space-time properties. He also argues that attempts to exclude them by *fiat* run into difficulties, so we should seriously entertain exotic global space-time properties. I suggest that, even granting Einstein's equations, it is difficult to know what global space-time properties are physically possible. Particle physicists in the 1930s-70s managed to derive Einstein's equations as a classical field theory in Minkowski space-time basically just by rejecting negative energy instabilities;[15] the result is that gravity is a Poincaré-type universal force. That fact suggests that particle physicists also have a claim on what is possible globally given Einstein's equations, though that question has not been pursued in much detail. Such derivations in effect extend and complete Einstein's *Entwurf* strategy, with essential use of energy-momentum conservation, though as a lemma rather than a postulate.[16] [17] Recent work on Einstein's notebooks has led some historians to conclude that Einstein actually found his field equations substantially using the physical leg of his double strategy, not the mathematical leg which he credited with success.[18] Rather than try to settle who owns Einstein's equations and gets to decide what further conditions can be imposed, I suggest splitting the difference. Manchak's exotic global features likely are possible given the mathematical standpoint, but might well be impossible given the particle physics approach. Tame solutions of Einstein's equations, those that would fit with any particle physicists' sensibilities as well as the geometrical standpoint and hence definitely are possible according to our best theory, we might regard as *a priori* somewhat more probable than the more exotic solutions possible only given the geometrical view. While the facts perhaps still could not force just one space-time upon us, we might have a rationally preferred candidate.

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[14] Gerhard Schurz, "The Meta-inductivist's Winning Strategy in the Prediction Games: A New Approach to Hume's Problem," *Philosophy of Science* **75** (2008), p. 278.

[15] Peter van Nieuwenhuizen, "On Ghost-free Tensor Lagrangians and Linearized Gravitation," *Nuclear Physics B* **60** (1973), p. 478.

[16] Albert Einstein and Marcel Grossmann, "Outline of a Generalized Theory of Relativity and of a Theory of Gravitation," translated by Anna Beck with Don Howard, *The Collected Papers of Albert Einstein, Volume 4, The Swiss Years: Writings, 1912-1914, English Translation*, The Hebrew University of Jerusalem and Princeton University, Princeton (1996); translated from *Entwurf einer verallgemeinerten Relativitätstheorie und einer Theorie der Gravitation*, Teubner, Leipzig (1913).

[17] J. Brian Pitts and William C. Schieve, "Slightly Bimetric Gravitation," *General Relativity and Gravitation* **33** (2001), p. 1319, gr-qc/0101058v3.

[18] Michel Janssen and Jürgen Renn, "Untying the Knot: How Einstein Found His Way Back to Field Equations Discarded in the Zurich Notebook," in Jürgen Renn, editor, *The Genesis of General Relativity, Volume 2: Einstein's Zurich Notebook: Commentary and Essays*, pp. 839--925, Springer, Dordrecht (2007),

Magnus and Frost-Arnold suggest that surplus structure can sometimes be heuristically useful, so this can be a reason not to apply the identical rivals strategy. I applaud their nuanced case-by-case approach. Treating gravity as a universal force could be useful in quantization, for example, by providing resources for causality to be well-defined, whereas traditionally it is not very clear why canonical quantum gravity can introduce equal-time commutation relations in the absence of a prior notion of equal times. There are also costs to the universal force view, such as the fact that the background metric, surprisingly, does not totally individuate points, on pain of indeterminism *via* the hole argument. These issues are rich with open questions.

To conclude, even if there is no global underdetermination problem in science, there might be interesting local problems. Such issues can inspire fruitful and important lines of research, whether in methodology, history, or science itself.

Posted March 29, 2009

Ubiquity of Possible Massless *vs.* Massive Cases and the Value of Pursuit

J. Brian Pitts
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I thank John Norton for his response to some of my comments on induction and on underdetermination in electromagnetism. He and I have some common ground regarding matters of induction. I am more worried than he is by the idea of the non-existence of a one true logic of induction, however, perhaps because I am less sure that we have non-question-begging access to the facts in particular cases and so desire the one true logic to help to avoid arbitrary conclusions.

While I don't see John as defending a determination thesis, I think that he underestimates the likelihood of significant cases of underdetermination in some contexts in contemporary physics as well as the value of taking them seriously as a strategy for perhaps resolving them. It is helpful that John has outlined some of the features of massive Proca electromagnetism that I did not mention. There are several differences between John's and my views of the significance of the Proca electromagnetic case, however. In particular, in my comments I did not (partly for brevity) make clear some points that seem important, so I will try to do so now.

First of all, the electromagnetic case is representative of a phenomenon that one should, defeasibly, expect to be a generic possibility in particle physics. For any field/particle that isn't obviously massive (by virtue of having a short range or requiring high energies to produce in accelerators), the question *prima facie* arises whether the field/particle is massless or massive. Those are the two great ways that fields/particles can be in relativistic field theory, corresponding to lacking or having in the Lagrangian density a term quadratic in the potential, respectively. As far as I can tell, particle physicists don't view one as *a priori* much more likely than the other, though experience with certain kinds of fields generates expectations that aren't so *a priori*. The defeasibility condition reminds us that technical details can and often do make a difference. John writes as if the Proca electromagnetic case were the only situation where one had to entertain the question of massless *vs.* massive underdetermination. But this sort of question routinely arises. A proto-instance is Newtonian gravity, where Neumann and Seeliger entertained, in effect, massive scalar gravity in the non-relativistic limit in the 1890s. Somehow a relativistic massive scalar gravity was not invented until 1968,[19] to my knowledge, though adding such a term to Nordström's theory would have been easy in the 1910s. (I have found infinitely many such theories.) If the bending of light had not been observed so soon, then there should have been a massless *vs.* massive underdetermination issue for scalar gravity. Massive scalar gravity should have taught us much about space-time theory long ago, as I hope to explain elsewhere. The massless *vs.* massive worry also should have been entertained for neutrinos. In fact most people assumed neutrinos to be massless, but some entertained the massive possibility, and they were proven right eventually. By taking the underdetermination issue seriously, we strengthen our ability to perform eliminative induction, entertaining what might otherwise be

[19] Peter G. O. Freund and Yoichiro Nambu, "Scalar Fields Coupled to the Trace of the Energy-Momentum Tensor," *Physical Review* **174** (1968), p. 1741.

unconceived or practically ignored alternatives. Either we avoid unjustified optimism by suspending judgment or we are able to eliminate some possibilities and earn greater certainty.

One had to entertain massless *vs*. massive underdetermination for Yang-Mills theories, if the world had any examples of Yang-Mills theories, and the question was debated in the 1960s and early '70s. Empirically it was evident that an *effective* mass term (or some other way of avoiding long-range effects) existed for the weak and strong nuclear forces. Spontaneous symmetry breaking turned out to offer an effective mass without an ordinary mass term for a Yang-Mills theory of the weak force, while an ordinary mass term turned out to behave badly. (Non-perturbative effects are important for the strong force.) One had to entertain massless *vs*. massive underdetermination for gravity, at least if one wanted to avoid suffering from unconceived alternatives, and particle physicists did so from the 1940s to the early 1970s. If supergravity (invented in the mid-1970s) had existed before massive gravity was generally set aside in 1970-72, then massless *vs*. massive underdetermination for (the spin 2 part of) supergravity would also have arisen. For massive Yang-Mills and massive gravity, technical problems arise in quantum field theory, if not sooner, when a mass term is present. That every stone has been turned in trying to address those problems seems unlikely, however. If more particle physicists took it as their task to probe cases of apparent underdetermination thoroughly, or if philosophers of science often were also particle physicists, then the questions would get the attention that they deserve. Partly due to the apparent accelerated expansion of the universe, physicists are increasingly willing to work on massive gravity once more, so there might be progress on this front.

Second, it isn't clear to me what John means in regarding current bounds on the photon mass as implying that it is at most "very slight." I also do not understand what would be a "miniscule" photon mass, especially such that the massive case might "just [seem] much less credible than the zero mass of the standard theory." Perhaps "just seems" is intended literally, in which case the subjectivity is worrisome. No doubt the current bounds on the photon mass are very small compared to the mass of a planet, or a horse, or a pea, or even an electron. But mass is a dimensionful entity, so a value[20] of 10^{-49} *grams* is not small or almost zero in any obvious objective sense, in the way that the number 10^{-49} is small (whether or not it is almost zero). If one could produce a particle physics argument that a photon mass in this range is unlikely, or produce a probability distribution for photon masses that escapes Bertrand paradoxes in choosing between mass and squared mass for the distribution or displays invariance while achieving normalizability, *etc*.,[21] [22] then I could understand John's concern and share it. As matters stand, it's difficult to know whether we have already shown the probability of a nonzero photon mass to be small, or we are just barely started in excluding massive candidates, or somewhere in between. Further experimentation would either vindicate a Proca theory of some nonzero photon mass or refute the heaviest photon theories, allowing one to update the probability distribution by Bayesian conditionalization and hence raising the probability of a vanishing photon mass,

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[20] L. C. Tu, J. Luo and G. T. Gillies, "The Mass of the Photon," *Reports on Progress in Physics* **68** (2005) p. 77.
[21] John D. Norton, "Ignorance and Indifference," *Philosophy of Science* **75** (2008), p. 45.
[22] Rodney Holder, *God, the Multiverse, and Everything: Modern Cosmology and the Argument from Design.* Ashgate, Burlington, Vermont (2004).

basically as John indicates. But it is not at all clear how far along in this process we are already. There might also be relevant cosmological constraints and perhaps the energy-time uncertainty relation to consider as bounds to what could possibly be achieved inductively; it has been suggested that there is a limit around 10^{-66} grams below which we cannot clearly measure the photon mass.[23] But is that almost zero? Much of the interest of a photon mass, such as the annihilation of gauge freedom, the presence of three polarizations rather than two, *etc*., persists for any nonzero value, no matter how small.

Third, John seems to have it both ways regarding philosophers' fine-grained theory individuation *vs*. physicists' coarse-grained individuation. Physicists don't naturally think of different photon masses as picking out different theories; they think of massive electromagnetism as one theory as having an adjustable knob for the photon mass. This difference is relevant given the rhetorical tendency of John's phrasing such as "pulling another version of the theory from the hat," which (while formally matching physicists' usage in "version of the theory") suggests, with the magician's hat imagery, that by some tawdry philosopher's trick something novel is gratuitously sneaked in to replace something old that has been slain by heroic facts. On the contrary, one of the virtues of the Proca case is that it is natural (arising in real science), rather than cultured or artificial (in terms of Norton's classification[24]) and hence not contrived. A number of physicists have thought it questionable to set at zero a parameter than might well merely be small.[25] [26] Especially when conceptual issues are at stake, such as gauge freedom, such vigilance is appropriate. While Norton's point here seems to rely on philosopher's sharp individuation of theories of massive photons to marginalize lower-mass varieties by emphasizing their distinction from higher-mass ones, his reading of the relevance of electroweak unification seems to require a rather coarse individuation of theories with massless photons. Electroweak unification does not merely add new claims or new fields to electromagnetic theory. It produces a new theory inconsistent with the old one; the physical photon field is not even the field entering the original Lagrangian in the electromagnetic way.[27] Thus ordinary electromagnetism (Maxwell's or standard quantum electrodynamics), not just the massive theories, was abandoned with electroweak unification, though the successor resembles the massless theories more closely than the massive theories, to be sure.

Fourth, John and I differ in where we see signal and where we see noise. As noted above, I see signal in the generic *prima facie* possibility of a mass term for fields/particles that aren't obviously heavy. John seems not to have the generic situation in mind. I see signal in the case of massless *vs*. massive scalar gravity, though the fact that gravity bends light implies that scalar gravity (massless or massive) is false. I see signal in the case of neutrinos, where the mass term was vindicated after being quite widely ignored. John sees signal in the fact that

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[23] L. C. Tu, J. Luo and G. T. Gillies, "The Mass of the Photon."
[24] John D. Norton, "Must Evidence Underdetermine Theory?"
[25] L. Bass and E. Schrödinger, "Must the Photon Mass be Zero?" *Proceedings of the Royal Society of London A* **232** (1955), p. 1.
[26] Peter G. O. Freund, Amar Maheshwari and Edmond Schonberg, "Finite-Range Gravitation," *Astrophysical Journal* **157** (1969), p. 857.
[27] Michio Kaku, *Quantum Field Theory: A Modern Introduction*. Oxford University, New York (1993), p. 337.

electroweak unification was taken as a good reason to prefer massless over massive electromagnetic theories. To me that looks perhaps like noise, a piece of good luck for the opponent of underdetermination, something that one could not have regarded as *likely* in advance on methodological grounds (though the idea of the instability of empirical equivalence under change of auxiliaries might call attention to the bare possibility). The technical issues regarding massive Yang-Mills and massive gravity theories look to me like more good luck for the opponent of underdetermination (in addition to the possibility that further exploration could perhaps alter the outcomes). No insights into underdetermination in general could make such problems likely; only insights into technical matters of particle physics could reveal the problems. One cannot rationally expect to be lucky. One can, perhaps, expect such "luck" if one believes Hegel's Absolute Spirit to be watching over scientific history by making things easy to ensure progress toward the truth. Otherwise we have to consider the significance of a generic possibility that is sometimes vindicated, and sometimes at least provisionally overcome by a string of accidents on some important occasions. Not being a Hegelian, I am more confident in the methodological significance of the generic possibility which sometimes comes true than in that of lucky apparent exceptions.

While I am not sure that there is ultimately underdetermination in cases of this sort in particle physics, it looks like a live possibility. Moreover, one does better science by seriously entertaining underdetermination and trying to shut the door to some options through research than by assuming, as Pollyanna and Pangloss might be tempted, that there is no problem and resting comfortably with what seems most probable at the moment. In the case of gravity, the warrant for General Relativity is much greater due to particle physicists' showing that nothing else is possible (to oversimplify a bit),[28] rather than resting comfortably in the irreversible progress already made by Einstein in the 1910s.[29] The task of applying confirmation theory to cases of apparent underdetermination also suggests interesting methodological questions.

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[28] David G. Boulware and Stanley Deser, "Classical General Relativity Derived from Quantum Gravity," *Annals of Physics* **89** (1975), p. 193.

[29] Jürgen Ehlers, "The Nature and Structure of Spacetime," in *The Physicist's Conception of Nature*, editor Jagdish Mehra. D. Reidel, Dordrecht, (1973), p. 71.

Reply to John Manchak on Particle Physics and Global Space-time Properties

John Manchak's response to my proposal suggests that some discussion of just what the particle physics approach to cosmology is, or ought to be, might be of use. He takes a geometric approach to Einstein's equations for granted. I suspect that he reasons something like this:

1. Einstein's theory involves his equations as a key aspect of local differential geometry.
2. Local differential geometry leaves a great deal unspecified about global differential geometry.
3. Einstein's theory fixes nothing more than local differential geometry.
4. Therefore Einstein's theory leaves a great deal unspecified about global differential geometry.

Manchak's results provide a vivid reminder of the lack of specification of global geometry by local geometry. They also indicate that making global geometry more specific by adding the sorts of postulates that seem natural in global General Relativity tends either to admit solutions that one might not have wanted or to exclude solutions that one might have wanted.

A core assumption of particle physicists' approach to gravity (often largely implicit, perhaps in calculations) is that gravity is just another force like the electromagnetic and other fields; gravity differs from the other forces in (generally more difficult) technical details, but not conceptually, except insofar as the technical details require.[30] [31] [32] [33] [34] The other forces are assumed by default to live in Minkowski space-time, so it is natural to assume (defeasibly) that gravity, a self-interacting spin 2 field, lives there also. Thus the analog of premise 3 above fails. Another common feature of particle physicists' treatments of gravitation is a rather pragmatic disregard for conceptual issues, so we should not be surprised if important conceptual issues have lain dormant for considerable lengths of time. In what follows I aim to explore particle physicists' idea that gravity is just another force, while attending to conceptual issues and aiming for logical consistency.[35] That aim is helped by an old paper by Roger Penrose,[36] who strove to

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[30] Suraj N. Gupta, "Gravitation and Electromagnetism," *Physical Review* **96** (1954), p. 1683.
[31] Peter van Nieuwenhuizen, "An Introduction to Covariant Quantization of Gravitation," in Remo Ruffini, editor, *Proceedings of the First Marcel Grossmann Meeting on General Relativity, 1975, Trieste*. North Holland, Amsterdam, 1977.
[32] Carlo Rovelli, "Notes for a Brief History of Quantum Gravity," *Proceedings of the Ninth Marcel Grossmann Meeting (held at the University of Rome "La Sapienza"', 2-8 July 2000)*, in Robert T. Jantzen, Remo Ruffini and V. G. Gurzadyan, editors. World Scientific, River Edge, New Jersey (2002), gr-qc/0006061. Rovelli portrays particle physicists' views without endorsing them.
[33] Richard P. Feynman *et al*., *Feynman Lectures on Gravitation*. Addison-Wesley, Reading, Mass. (1995); original by California Institute of Technology (1963).
[34] Steven Weinberg, *Gravitation and Cosmology*. Wiley, New York (1972).
[35] J. Brian Pitts and William C. Schieve, "Null Cones and Einstein's Equations in Minkowski Spacetime," *Foundations of Physics* **34** (2004), p. 211, gr-qc/0406102.
[36] Roger Penrose, "On Schwarzschild Causality -- A Problem for 'Lorentz Covariant' General Relativity," in F. J. Tipler, editor, *Essays in General Relativity---A Festschrift for Abraham Taub*. Academic, New York (1980).

show what he took Steven Weinberg to be committed to accepting based on his remarks on the particle physics approach to Einstein's equations. While particle physicists often employ perturbative methods, they are not essential. The particle physics approach, by taking gravity to be just another field, takes the gravitational field to be no more relevant *a priori* to space-time geometry than electromagnetism or the weak nuclear force, for example. These forces are not permitted to violate the flat metric's causal structure or alter the topology of space-time. Vigilance is required to keep interacting spin 3/2 fields from violating the flat metric's causal structure.[37] Given that conceptual novelties are introduced only when required on technical grounds, an argument is needed for permitting a self-interacting spin 2 field (gravity) to violate the background null cone structure or affect space-time topology. One way to make such an argument would be to show that solutions needed on physical grounds must burst out of the restrictions imposed by the flat metric. Such arguments have rarely been attempted, however.

Penrose takes up this challenge by attempting to show that the particle physicists' view cannot be pushed to logical completion. In particular, Penrose argues that are two importantly different ways to relate the effective curved space-time metric of the Schwarzschild solution to a flat background metric. One way of relating the two metrics has the effective curved metric's null cone leaking out past the flat background metric's null cone, implying violating of causality in the sense of Special Relativity---which is relevant given the particle physicist's view that gravity is just another force living in Minkowski space-time with merely technical complications. That result would be problematic for particle physicists inclined to scientific realism and intent on treating gravity as just another force. (As noted above, many are rather pragmatic.) The other way of relating the two metrics has the effective curved metric's null cone everywhere inside the flat background metric's null cone (thus respecting special relativistic causality), but the integrated deviation between the two null cones diverges at infinity. Such divergence makes trouble for scattering theory, in which Weinberg was involved. (One perhaps could have both causality violation and difficulties in scattering if one were careless.) Thus the flat background metric either doesn't play its usual role as a bound on propagation of fields, or is inconvenient for Weinberg's scattering purposes. If someone thinks that the flat background metric is real and thinks that it make scattering convenient, then Penrose's argument poses a problem. If one believes only one of those claims, however, then Penrose's argument poses no serious problem. It is hardly news that long-range potentials have inconvenient scattering properties.[38] [39] Thus there appears to be no known serious objection to the possibility of treating gravitation consistently as a spin 2 field theory in Minkowski space-time at present.

Thus far I have addressed the question of what particle physicists ought to say about cosmology. Why should one entertain it as perhaps true? The particle physics approach to gravity has the unifying virtue of treating gravity and other forces in the same fashion. It might be useful in excluding the bizarre on principled grounds also.

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[37] Giorgio Velo and Daniel Zwanziger, "Propagation and Quantization of Rarita-Schwinger Waves in an External Electromagnetic Field," *Physical Review* **186** (1969) p. 1337.
[38] Herbert Goldstein, *Classical Mechanics*, second edition. Addison-Wesley, Reading, Mass. (1980).
[39] J. Brian Pitts and William C. Schieve, "Slightly Bimetric Gravitation," *General Relativity and Gravitation* **33** (2001), p. 1319, gr-qc/0101058v3.

Posted March 27, 2009

Underdetermination In Science

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1. The Underlying Picture.

I'd like to repeat an invitation made during the closing discussion. The project is to catalog the various attitudes that underlie positions in the debate over underdetermination in science. May I invite comment, additions and corrections to the list below?

My impression is that the debate is rooted ultimately in very different conceptions or perhaps just different perspective on science. My goal at this point is not the familiar one of deciding an issue by argument and debate. Indeed my sense is that argument and debate has done little more than force concessions on opposing sides so they start to agree on superficial matters but remain separated in their deeper outlooks. A fuller understanding of the debate, I believe, requires a kind of anthropological analysis that will bring these deeper views to light.

Here are five conceptions:

(a) ("Pollyanna/Pangloss") The world presents science with a difficult puzzle. Through creativity and ingenuity, scientists are able mostly to solve the puzzle and find the truths that lie behind appearances.

This optimistic view--hence "Pollyanna" and "Pangloss"--leads one to discount underdetermination as a minor threat to science. It is my view.[40]

Other views favor underdetermination:

(b) ("Einstein") Scientific theories are not merely passive reflections of the world, but, in significant measure, a product of human creativity. Underdetermination arises because of the

[40] I fully acknowledge that there are numerous cases in which the evidence we have fails to determine a particular theory. That circumstance is generic in newly developing sciences. My optimism is that these cases of underdetermination are not irresolvable. I am routinely astonished at just how much we can figure out. Two hundred years ago, who could have imagined that we could ever know the chemical composition of the sun, let alone discover a new element there? This optimism is just my opinion. Just as there are no good, principled arguments supporting a thesis of universal underdetermination, our understanding of inductive inference is too rudimentary to provide general and principled arguments for my optimism.

essentially creative character of scientific theorizing. It opens a space in which human creativity can be exercised to a fuller extent in the generation of many theories.

This view is more optimistic about scientists than scientific discovery. Its label comes from Einstein's remarks about scientific theories as the "free creations of the human spirit."

(c) ("Cartesian Demon") The world is hostile to us and out to trick us. It has done so successfully in the past. We have thought repeatedly that we have found the final truth only to discover that we were misled. We should resist and not let the world trick us again into thinking that this time we have discerned the truth behind the appearances. It is a useful philosophical project to understand better how we come to be misled.

This pessimistic view is a modern version of the skeptical pessimism famously sketched by the possibility of Descartes' deceiving demons.

(d) ("hubris")[41] Our inductive methods have been developed and tested in ordinary experiences. We overestimate their power when it comes to extrapolating them beyond the mundane challenges of ordinary life to profound challenges of the innermost secrets of space, time, matter, life and the mind.

See Kyle's statement for a fuller development. My reading of Brian's remarks on theories of massive photons and massive gravity suggest some sympathy with this approach. More precisely, his concern is that physicists overestimate the determining power of evidence and thereby fail to explore promising alternative theories.

Finally there is a view whose origins do not lie in considerations of evidence directly:

(e) ("relativist")[42] Science is a component of human culture and cannot be separated from its cultural context. One component of that context is the portrayal of scientists as heroic explorers using reason to discover the truths of nature. If we are to understand science, we should no more believe this portrayal that an anthropologist should believe the doctrines of some arcane cult that is subject to anthropological study.

This view, popular among some sociologists and historians of science, agrees that science does provide a narrative in which evidence is seen sometimes to provide strong support for certain of its hypotheses. That fact is taken to have no more significance than the fact that arcane cults have narratives in which the absolute truths of their doctrines are assured by the interpretation of sacred texts and other signs. Sometimes there can be internal indications that these narratives are flawed. In the case of science, the underdetermination thesis is such an internal indication.

I do not include other, more opportunistic motivations for underdetermination that do not reflect a deeper world picture that inclines for or against underdetermination. One might favor

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[41] Added April 16, 2009. This addition was suggested by Kyle Stanford's remarks.
[42] Added April 10, 2009. My thanks to Dana Tulodziecki for suggesting an addition along these lines.

underdetermination simply because one has some results in science that happen to fit with the doctrine of underdetermination, while not otherwise having a deeper sympathy with the view. Or one might find it expedient to endorse underdetermination as a way of advancing a minority theory in science. This latter tactic is limited. It does enable one to say that the majority view cannot have been selected solely on the basis of evidence. However that same limit applies to the minority theory; it is also underdetermined.

2. Unconceived Alternatives

My article on the underdetermination thesis (Norton, 2008) surveys how the thesis fares in relation to many different accounts of induction. I did not comment then on Kyle's fertile notion of "unconceived alternatives."

My sense is that Kyle's concept relates to most theories of inductive inference in the following way. Whenever one makes an inductive inference, one takes an inductive risk in one form or another. One generally does not reflect too much on just how the gamble may be lost. Talk of "unconceived alternatives" is a way of making the possibility of failure concrete. It relates most naturally to all eliminative forms of inductive inference. The one Kyle points to most often is abduction or inference to the best explanation. We can fail to identify the best explanation simply because we never thought of it.

The problem arises in all forms of inductive inference. Whenever we lose an inductive gamble, it is because something did not go as we expected. Those who had only ever seen white swans did not expect the black swans of Western Australia. Sometimes we may have consciously thought of how the gamble may be lost. That would be a conceived alternative. Sometime we may have not.

The mere fact of the pervasiveness of unconceived alternatives should not make us into inductive skeptics. What is of decisive importance is how likely these alternatives are. A credible underdetermination threat requires that the unconceived alternatives also be credible. There clearly are cases in which they are. Modern theories of quantum gravity provide a signal example. We can be pretty sure that we have scarcely begun to explore the alternatives. It will go differently in other cases. No doubt there are still as yet unconceived alternatives to the roughly elliptical orbits of our planets and to the periodic table of the elements. But I doubt they are credible.

For this reason, I see Kyle's case of fossils in the 16th and 20th century differently. He portrays it as a gradual shift in the inductive inference form used from one prone to the problem (abduction) to one that is not (projection). In my view, both are prone to the problem. However, in the particular cases Kyle describes, the abductions happened to be more troubled by them than did the projections, which are essentially immune to them.

Finally I should mention that I don't see the same sort of principled difference between abduction and projection that Kyle does. In accord with my material views on induction, the idea of "abduction" itself is merely a rough and ready gloss on a scattered collection of inductive inferences that we like to group together under the one label. The same is true of "projection." So

the progression of inductive inferences on fossils from the 16th to the 20th century is, in my view, a progression from weaker to stronger inductive inferences with correspondingly less inductive risk.

For more on abduction and other issues, see (Norton, manuscript a).

3. Ptolemy and Copernicus Again

Greg Frost-Arnold's remarks on the Ptolemy-Copernicus example in my talk give me the opportunity to correct a misimpression. I had not intended to make a point about underdetermination in real astronomy circa 1543. Rather I intended only to cook up a highly contrived "toy" example to illustrate vividly the notion of equivalence of theories, sometimes called notational variation. I imagined an oversimplified Ptolemy-like theory for Mars that employed only an epicycle and a deferent; and I imagined a Copernicus-like theory for Mars that employed perfect circular motion for the Earth and Mars. In so far as we restrict our attention only to observational astronomy, the two theories come out as equivalent. The equivalence is easily seen in that we map the Ptolemaic theory onto the Copernican by shifting Mars' epicycle into the circle that is the Earth's orbit.

Of course the moment we make things more realistic, or the moment we broaden our concern to include the dynamics that bring about the motions, then the intertranslatability no longer holds.

In retrospect, the example was chosen poorly since it is too much to ask a historically and scientifically sophisticated audience to forget all the history and science they know!

4. For Determination or Against Underdetermination?

From his remarks, my sense is that Brian Pitts and I differ quite widely on our underlying pictures of science, so we tend to read into each other's writings things that were never intended. I apologize in advance to Brian if the remarks below misconstrue his views.

My impression is that Brian sees me as advocating some thesis of evidential determination in science. While I am sympathetic to the idea that evidence has strong import, neither my papers nor my talk argue for a thesis of determination. Rather the point that I stress, especially in my talk, is that our understanding of inductive inference is so incomplete as to make such an argument unsustainable.

It is exactly this same incompleteness that forms the basis of my complaints about the underdetermination thesis. That thesis asserts that evidence must always underdetermine theory. It is a very strong claim about what is possible in principle for inductive inference. It must be distinguished from the simple notion of underdetermination, which I do not find controversial. That is, along with virtually all philosophers of science, I have no doubt that there are many cases in which the evidence we have fails to determine a particular theory and that this evidential lacuna can persist. This seems to be the case at present with all theories of quantum gravity.

My major point about the underdetermination thesis is one already widely recognized: most arguments for the thesis depend upon accepting just one defective account of induction, a simple-minded hypothetico-deductivism. I note, in contrast to this, that other more developed accounts of inductive inference do not yield similar failures of determination. However, elsewhere (Norton, 2003), I have urged that none of these is the One True Logic of Induction. While the Bayesian system Brian mentions has had some notable successes, I do not believe it can claim to be that One True Logic. (See Norton, manuscript b, for detailed reasons, some of which endorse Brian's hesitations about the approach.)

The upshot is that we just do not understand enough about induction to be able to sustain grand claims in either direction about the universality of determination or underdetermination.

Brian discusses Proca's theory of massive photons. In it, light propagates at speeds less than the constant c of the Lorentz transformation, according to just how much mass photons have. It is hard to see what moral can be drawn from the example. At best one can say that, temporarily, there was a problem picking between the massive theory and the standard theory, as long as one assumed only a very slight mass for the photon. However that underdetermination did not persist. If I have understood Brian's remarks correctly, Proca's theory of massive photons was abandoned with the advent of the electroweak unification. So at best the moral seems to be one of transient underdetermination, terminating in a decision on the basis of new insights. This story gives comfort to those who favor determination.

Even prior to this decision it is unclear what moral to draw. Proca's theory was not a theory but a family of theories, indexed by a parameter with an unknown value. This fact gave the theory temporary protection from experimental falsification. A demonstration that the photon mass cannot be such and such can always be countered by pulling another version of the theory from the hat with a smaller mass parameter. Nonetheless, the case seems one that inductive inference can decide. If the standard theory is correct, then, if anyone cares to do them, new experiments can continue to drive the mass parameter to lower and lower values until the possibility of some miniscule, as yet untested mass just seems much less credible than the zero mass of the standard theory. Or, if some version of Proca's theory is correct, that will eventually reveal itself. One might think that the possible smallness of the mass would be an obstacle. With sufficiently small mass, the speed of light might come out at 99.999%c, which is hard to discriminate from c. But it is a simple matter of Minkowski spacetime geometry that, if one observer finds a propagation at 99.999%c, there will be another moving relatively, that might judge it to be 90%c, which is easily discriminable from 100%c.

Finally, Brian mentions that the decision between Proca's and the standard theory is immune to my concern about the possibility of rival theories merely being notational variants of one another ("identical rivals strategy"). Here I agree with him. My remarks about the possibility of notational variants apply to the very special case of two theories with identical observational consequences. These two rivals are not identical observationally. One asserts a speed of light less than c; the other asserts a speed of light equal to c.

Addition posted March 29, 2009

Brian Pitts has now written a lengthy response (March 29) to these remarks. On pain of a combinatorial explosion, I will not react point by point. However this much summarizes my reaction. Brian pictures me as positively defending views that directly contradict his, whereas mine are more neutral positions or no definite positions at all.

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Posted March 29, 2009

John Manchak

I very much appreciate Brian's comments with regard to my talk. However, it is not yet clear to me how particle physics is, in any way, connected with global spacetime structure. I do not doubt that certain assumptions within particle physics can be used to derive Einstein's equation (or possibly some other local field theory). But, it seems that more must be said to support the view that this local derivation "suggests that particle physicists also have a claim on what is possible globally." Why should the particle physicist's physical sensibilities (gained presumably from doing local physics) matter, at all, to the cosmologist?

Posted April 10, 2009

Dana Tulodziecki

I'd like to thank Greg for his two questions, and answer them (very briefly). Greg's first question is how my approach differs from/extends upon Laudan's in *Demystifying Underdetermination*. Laudan gives one (historical) example of a case in which it is possible to invoke a methodological rule capable of determining theory-choice (341). However, he goes on to claim that "[w]e need not concern ourselves here with whether [the rule] is methodologically sound" (341), concluding just a little later that "[t]hat complex of rules and evidence *determined* the choice between the two systems of mechanics, *for anyone who accepted the rule(s) in question*" (second emphasis mine, 342).

I (obviously) agree with Laudan that we ought to look at the rules, but I think the question is precisely whether the rules we look at *are* methodologically sound, and what rules, if any, we are justified in accepting on an epistemic basis. But, in that case, the question arises of how we can establish the connection between these rules and a theory's (approximate) truth/success. In order to give substance to claims about the epistemic importance of methodological rules, we need to have some idea of what these rules are, and an argument to the effect that they are, in fact, epistemically significant. I want to suggest that we can do both by engaging in historical case-studies like that of Snow.

Greg's second question is how all this might help us with unconceived alternatives. The first thing to say is that I'm not sure how much help is needed -- I think that we can judge the severity of underdetermination only on a case by case basis (something that also came out in many different parts of the workshop). In this vein, I think that some unconceived alternatives will turn out to be legitimate threats to our current theories, and some will not. What an appeal to methodological principles can do is help us determine which unconceived alternatives are particularly threatening (for example, those that make no use of any epistemically interesting rules aren't).

Lastly, I also have a series of questions for Kyle. I now realise that I'm confused about what exactly Kyle's view is, so these aren't as well-formed as I'd like (but perhaps someone else can either make my worries more concrete or else explain why they're not really worries). Like John, I don't think there is a fundamental difference between the projective and abductive scenarios. But even if there were, what reason is there to think that theories/hypotheses typically fall primarily into one but not the other category? Hypotheses might have many different kinds of evidence going for them, some projective, some abductive. In addition, even the same evidence might play different roles in different parts of the theory, there might be several different routes (some projective, some abductive, perhaps) to the same evidence, and so on (although I'm not familiar enough with fossil theory to see how these points might apply in this particular case, and I can't think of any good examples right now). I take it Kyle's view is that a theory is less vulnerable to PUA if it's *primarily* projective; however, it seems that this would have the following consequence: in this case, the mere existence of additional (abductive) evidence would render a hypothesis more vulnerable to PUA than having less (only projective) evidence going for it. This seems like an odd consequence to me.

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P. D. Magnus

A few weeks ago, I participated in a **workshop on underdetermination** at the Pittsburgh Center for Philosophy of Science. The conference was fabulous, both socially and intellectually. Here's a post growing out of that, specifically about John Manchak's work on global features of spacetime.

The post is somewhat rambling, so let me begin by summing up:

> The underdetermination facing our theorizing about global features of spacetime is formally more like familiar illustrations of the problem of induction than it is like familiar examples of empirical equivalence. Yet (if Manchak is right) it is different than usual worries about induction because we could never have the right kind of background knowledge to justify the inductive generalization.

John Manchak discussed his work on observationally indistinguishable spacetimes: There are some properties which, if they held in our universe, we could not know to hold. Manchak gave the example of 'hole-freeness', the property that a spacetime has got if there aren't any holes in it. I'll stick with that example.

The proof (originally sketched in the 1970s by David Malament and recently proven by Manchak) models each spacetime point as a possible observer. Observers are then treated as knowing everything about the contents of their past light cones.* Now consider an (almost**) arbitrary hole-free spacetime ALPHA. We can cut up ALPHA and assemble a different spacetime BETA, such that there is a region of BETA corresponding to the past light cone of every observer in ALPHA *plus* an additional region that contains a hole. Observers who collected all of the observations afforded by ALPHA could not know whether they were in hole-free ALPHA or holey BETA.

Manchak argues that this is a result particularly about global spacetime structure and that it does not hint at any general kind of underdetermination plaguing scientific inference. Physicists attempt to overcome the underdetermination by putting restrictions on what counts as a physically plausible spacetime, but such restrictrions (argues Manchak) are *ad hoc* and ultimately unsuccessful. We don't ordinarily reckon with things like the structure of the entire universe, so our ordinary intuitions can't be relied on to constrain the space of possibilities.

All of that seems right to me, as far as it goes. Note that the underdetermination here is asymmetrical. If spacetime is *not* hole-free, then the hole must be in the causal past of some spacetime point and so an observer there could know about it. The underdetermination arises only if spacetime is hole-free, because all the hole-free observations can be embedded in a spacetime that includes holes elsewhere.

Familiar cases of (allegedly) empirically equivalent theories are not like this, but instead are symmetrical. Take the claim that we live in a physical world. Consider the rival Cartesian claim that we are immaterial things deceived by an evil demon into thinking that there is a material world. Assuming that the choice between these is underdetermined, it is underdetermined regardless of which of the two possibilities actually obtains.

Note, however, that common illustrations of the problem of induction do have an asymmetric structure like the spacetime case. Suppose we start with a world ALPHA in which all swans are white. We can construct a world BETA in which there are all the swans in ALPHA plus a black swan. If we only observe white swans, then it might or might not be the case that all swans are white. Yet if it's not the case that all swans are white, there is an observation that would show as much (an observation of one of the non-white swans).***

Or consider instead a world ALPHA in which all humans are mortal. Provided every observer has an open future, we can construct a world BETA in which all of the observations in ALPHA occur at some place and time but there are immortal people. This seems strictly analogous to Manchak's case, because the requirement of an open future is just the requirement that ALPHA not be causally bizarre.

If I am right about all of this, then there is a parallel between the inductive conclusions 'Spacetime is hole-free', 'All swans are white', and 'All men are mortal.' Yet I agree with Manchak that the first of these is importantly different.

Let's start with what John Norton calls a *material theory of induction*: Induction requires background knowledge about the domain of objects about which we are generalizing.

In the case of swans and whiteness, we know that natural species typically have variable colouration. So we conclude that swans are not the kind of thing that are likely to all be of the same colour. The observation of many white swans does not suffice to show that all swans are white, regardless of how many we observe.

In the case of humans and mortality, we know that human bodies are fragile things. People are apt to get injured or sick eventually. Moreover, bodies grow decrepit with age. So we are justified in concluding that all humans are mortal.

In the case of spactime and hole-freeness, as Manchak argues, we don't know anything which constrains the global structure of spacetime sufficiently to underwrite an induction. So we would never be in a position to conclude that spacetime is hole-free.

* John Norton raised the worry that there might be holes in our causal past which we wouldn't be able to notice. That kind of underdetermination is not at issue here. The question is just whether infallible observers of their hole-free pasts would ever be able justified in concluding that all of spacetime is hole-free.
** The proof excludes 'causally bizarre' spacetimes in which a single observer can survey all of an inextensible spacetime at once. This would require that some observers be able to see their own future; ie, time travel would be possible. Manchak retorts that if time travel were possible then indistinguishability would be the least of our problems.
*** To make it exact, add the constraint that one could never have observed all of the swans in the world. This is formally parallel to the assumption that spacetime is not causally bizarre. (Alternately, let the constraint be that one would never be justified in believing that one had observed every swan.)

Thinking more about **indistinguishable spacetimes** has led me to think about the contrast between underdetermination and indeterminacy. Somehow, I wrote a dissertation on the former without clearly thinking through the latter.

In a discussion note (http://philsci-archive.pitt.edu/archive/00004505/) that he wrote for the workshop but did not present, John Norton suggests that "the indistinguishability does not pertain to theory. We are not presented, for example, with general relativity and some competitor theory, indistinguishable from it. Rather, what we cannot distinguish is whether *this* spacetime is the one of our observations or whether it is *that* one" (p. 4). Yet this requires drawing a line between a theory and a detail filled out within a theory.

Suppose we accept a statement conception of theories. General relativity is a set of axioms and their consequences. Add some claim about spacetime (e.g. that it is hole-free), and it's one more axiom. The specification is still a theory and can be pitted against a rival specification.

One might still say: A theory is more than just any old collection of sentences. A theory is a system of *laws*. General relativity is a theory because it's got the general laws, but specifications just add local detail. This may separate the sheep from the population ecology, but it won't cut the mustard here. The specifications of spacetimes are definitely not *local* detail. Spacetime being hole-free is a fact about the global topology of spacetime. It's plausibly even a law. (Whether or not it is ultimately a law depends on what we think "law" means.)

Suppose we accept the semantic conception of theories instead. General relativity is a set of possible models. Models with hole-free spacetimes are a subset which can itself be treated as a theory.

I'm a **pluralist about theory concepts**, so I can't insist that there is *no* possible understanding of theory such that general relativity is a theory and the specifications are not. I just don't see what it would be.

So I don't think that Norton's attempt to make this *not about theory* succeeds. But I think he's right to think there's a difference between underdetermination *between rival theories* and indeterminacy *within a theory*.

Underdetermination obtains when we can't responsibly decide between rival theories. If this inability only holds for a narrow range of circumstances, then there isn't anything of especially philosophical interest: We begin in ignorance, do some research, and discover something. Underdetermination of the sort that typical concerns philosophers holds for a broad range of circumstances, possible every circumstance we could ever hope to be in. (In my work, I call this range of circumstances the 'scope' of the underdetermination.)

Indeterminacy obtains when a theory can be specified in different and significantly incompatible ways. For example, the number of particles in the universe is indeterminate in classical mechanics. If you specify the number of particles, then you can put the machinery of the theory to work - but the theory won't tell you how many particles you should consider. A different way of putting the point is that classical mechanics has models with any number of particles.

To get a feel for how underdetermination and indeterminacy interact, consider some examples:

A. The gravitational constant simply appears as a parameter in Newton's theory of gravity. The theory does not tell us what the constant must be. It's indeterminate. Yet, since there is no problem is supposing that there is a precise value for the constant in the world; we can try to formulate the specified theory that includes both the law of gravity and the correct value for the constant. We can determine the constant experimentally (within error bars) and so its value is not underdetermined.

B. It is possible to describe arrangements in classical mechanics such that two outcomes are equally compatible with the theory; e.g. **Norton's Dome**. This is clearly indeterminacy. We could specify which of the outcomes will occur, effectively forging a more specific theory that is not indeterminate in this way. Yet there is no principled reason to specify one outcome rather than another. Since we can't construct a Norton Dome, there is no way to resolve the matter experimentally. Even supposing we were justified in accepting classical mechanics, we would not be justified in accepting a more specific theory which avoided the indeterminacy. So the choice between specific theories would be underdetermined. Yet the Dome indeterminacy would not have conjured a rival to classical mechanics. Our acceptance of the more general theory might withstand underdetermination, even if

it arises for the specifications.

C. In Quantum Mechanics, as it's usually construed, particles typically don't have determinate positions and momenta. Yet we can't just freely imagine precise quantities out in the world. The **Kochen-Specker Theorem** puts constraints on there being precise values, precluding certain kinds of hidden variable theories. Presuming that QM were true, this limitation would not be an *epistemic* problem at all - there would be indeterminate quantities in the world. We could (in principle) know them as precisely as the world defines them, so it wouldn't be a matter of underdetermination.

D. In General Relativity (to return to the Malament/Manchak proof) the theory is compatible with spacetime having different global features. So we have indeterminacy. If some of those features obtain, then neither observation nor theoretical considerations would justify our thinking that they obtained. (See my previous post for a brief explanation as to why.) So we'd face underdetermination. Yet this underdetermination does not show that our acceptance of GR is underdetermined. We cannot responsibly decide between GR&H and GR&not-H, say, but that does not show that GR itself has any serious rivals.

Notice also that we might fret over questions of indeterminacy within theories that we think are simply false. We can no longer responsibly believe classical mechanics, for example, so we are not in scope of any interesting underdetermination for it against any rivals - but it can be philosophically rewarding to consider indeterminacies like the Norton Dome. I think that this is because indeterminacy is fundamentally a *logical* or *metaphysical* question, and so it may be rewardingly chased even into the den of otherworldly counterfactuals. Underdetermination is primarily an *epistemic* or *epistemological* question, and so it matters primarily insofar as it tells us something about limits on our ability to actually know things.

David Harker

Greg asks of my position how it differs in its predictions from those of the constructive empiricist. I think significantly. In the general case, consider a scientific theory that we identify as being better than the theory it replaced, which includes cases we're inclined to describe as modifications to a theory rather the introduction of an entirely distinct theory. The perceived progress achieved will not be attributed solely to increased empirical content. *Ad hoc* modifications achieve this much, but are not considered progressive. Progress must therefore involve appeals to considerations such as simplicity, consilience, etc. The constructive empiricist acknowledges the role of such factors, but denies they are indicators of truth. However, the fact that some theoretical claim is endorsed on the basis of pragmatic considerations, *relative to one data set*, is a poor reason for predicting its retention across subsequent cases of scientific change that range over more comprehensive data sets.

Rutherford's nuclear model of the atom was better than older models in virtue of its capacity to explain the alpha-particle scattering data. Whatever model of progress we adopt it is going to appeal to factors that the constructive empiricist would consider of mere pragmatic value. Despite a wealth of new discoveries and models, physics retains the opinion that atoms have massive nuclei. But, again, the pragmatic value of certain identifiable aspects of the nuclear model, relative to the early data set, is no reason for predicting their subsequent retention.

Second, for the record, I'm with John Norton in having an optimistic, Pollyanna view, about science. Science is providing increasingly truth-like models and theories; whether we should describe those theories as approximately true is a less tractable, and perhaps less important, question for realists.

Reaction to Underdetermination Workshop of March 21-22, 2009

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Let me begin by saying that the workshop was enormously useful to me, as I received extremely valuable input from a wide variety of people in a wide variety of forms concerning both my own ongoing research into underdetermination and my forthcoming SEP entry on the subject. I could not hope to summarize or acknowledge all or even just the most important feedback I received and so I will not try to do so. What I will do is try to address in somewhat more detail questions that came up at the workshop, particularly in regard to my own work, whose discussion is not already reflected in the papers as they were originally presented.

For my purposes the most useful result of the conference discussions was to highlight both the difficulties and the importance (each of which already struck me as considerable) of distinguishing the varieties of inductive evidence we have in support of various kinds of scientific claims and the plausibility of the challenges that can be raised to those forms of evidence (two issues that came up in nearly all of the talks that were given). I am sympathetic to John Norton's suggestion that inductive inferences always involve a gamble of some kind and that the central issue is just how likely or plausible it is that whatever could defeat the gamble (e.g. an unrepresentative sample, an unconceived alternative explanation) really obtains in a given case. But I am much less sanguine than many other workshop participants seem to be about the idea that this is more or less all that there is to say, and that the threat of underdetermination can therefore only be assessed locally or on a case-by-case basis. This is

because I think that while we are pretty good at assessing our degree of risk reliably in everyday cases of abductive or eliminative inferences, and perhaps other kinds of inductive inferences as well, I think we have convincing evidence that we are pretty poor at doing so in the context of scientific theorizing. For unconceived alternatives, our usual practice seems to rely on something like an availability heuristic: if we can't think of a theoretical alternative that is well-confirmed by the evidence, then we assume it is unlikely that one exists. (I discuss this inference in slightly more detail in "Scientific Realism, The Atomic Theory, and the Catch-All Hypothesis: Can We Test Fundamental Theories Against All Serious Alternatives?", forthcoming in *BJPS*.) Again, I think this heuristic is reliable in most everyday contexts of eliminative inference but not in most scientific ones, especially in fundamental theorizing (where it matters most). And John's musings about the credibility of unconceived alternatives in various scientific contexts (quantum gravity, planetary cosmology, the periodic table) seems to me to exemplify rather than resolve the problem. As John and Greg each suggest independently, I am open to the idea that both projections and abductions can be vulnerable to the problem of unconceived alternatives, and I certainly agree with John's assessments that "the progression of inductive inferences on fossils from the 16th to the 20th century is…a progression from weaker to stronger inductive inferences with correspondingly less inductive risk", and that in the particular case I discussed, "the abductions happened to be more troubled by them than did the projections, which are essentially immune to them", but if these judgments rely on nothing more than strong intuitions about the relative safety of the various inferences in question, then I think we are in deep trouble.

My own interest in the case of fossilization was prompted initially by the desire to understand why in some cases of what is undoubtedly fundamental scientific theorizing (e.g. the hypothesis of organic fossil origins) but not others, it seems a mere skeptical possibility that

there remain unconceived theoretical alternatives that are well-confirmed by the available evidence. I think identifying the projective evidence that accumulated throughout the 19th and especially the 20th century in support of the hypothesis helps explain that sense, but the more important question is whether it does anything to justify our confidence, and I do not see how it can unless we allow that there are systematic epistemic differences (though perhaps only differences of degree) between the kinds of evidence we now have in support of the hypothesis of organic origins and that in support of earlier accounts of fossil origins, or in support of the caloric theory of heat, or in support of a given theory of quantum gravity. I certainly do not mean to suggest that a systematic evaluation of the threat posed by unconceived alternatives in a given case must involve a simple dichotomy between abductive and projective forms of inductive support: as Greg notes, I am sympathetic to the idea that there is a continuum of risk from unconceived alternatives associated with different kinds of inductive inferences, and there are surely a wide variety of features of the background context that impact our vulnerability to the problem of unconceived alternatives in a given set of circumstances, from the availability of such projective evidence to our relative familiarity with the objects and relations making up the domain about which we are theorizing and presumably much more besides. But I think we must now set out to explore those features and the relationships among them, for unless there are systematic distinctions to be drawn between the features of different kinds of cases and the kinds of evidence we have in support of various scientific claims, and unless there is something quite general to be said about the respective vulnerability of those forms of evidence to various inductive infirmities, we are left with nothing but strong intuitions about cases, which I've suggested are demonstrably not worth much in the case of scientific theories. Those who are dismissive about the prospects of underdetermination will need something besides their (or even

our!) intuitions about particular cases to convince us, and those who are enthusiastic about the prospect of widespread underdetermination will need to make sure they haven't just overplayed their hands.

Although this part of my work is usually characterized as challenging scientific realism, I hope this illustrates why it can equally well be seen as a contribution to the realist cause: viz., as an effort to identify the circumstances in which realists might really be justified in regarding our theories (or parts, aspects, elements, or characteristics of those theories) as accurate descriptions of otherwise inaccessible domains of nature. The point all along has been to get us to pay attention to the kind(s) of evidence we have in support of a given theoretical claim rather than what the claim is about or who is making it.

Furthermore, I think there is an intriguing connection between the idea that our intuitions about inductive risk are unreliable when deployed in the context of theoretical science and what I see as the central issue in John Manchak's talk. I share John's frustration with the lack of any general answer to the question of what constitutes a "physically reasonable" spacetime. As emerged in the discussion, in making such judgments I think we (and here I mean physicists as well as philosophers) rely far too quickly and easily on physical intuitions gleaned from (or evolved in response to) our experiences with rocks, trees, and pools of water, and that any warrant we have for extending these intuitions to, for example, the structure of spacetime, is extremely weak. Without any convincing general account or defense of our grounds for dismissing hypotheses as "physically unreasonable", the prospect of underdetermination by serious scientific alternatives looms, of course, commensurately larger.

(Much of the further discussion of papers in the workshop's second half, particularly concerning empirical equivalents and the "identical rivals" response (that two putatively distinct

hypotheses are really notational variants of a single hypothesis) seemed to me to fit naturally with my own published views (e.g. in Chapter 1 of *Exceeding Our Grasp* and in "Refusing the Devil's Bargain: What Kind of Underdetermination Should We Take Seriously" (2001 *Philosophy of Science, 68* (PSA Proceedings): S1-S12) and so I will not comment further on it here.)

Let me conclude by trying to give an answer to John's question about the motivations, rather than the arguments, sustaining various sides of the philosophical controversies surrounding underdetermination. I have a slight advantage over some workshop participants in that I saw the talk by Wolfgang Pietsch at &HPS2 that got John wondering about this and had a chance to talk to him afterwards about it. What John seemed most centrally worried about initially was not whether and how various claims of underdetermination might be defended, but what makes people think it would or wouldn't be a good thing if they can (or cannot) be. Oversimplifying, the Pietsch talk concluded by saying "hurray for underdetermination" because it encourages creativity, variety, and heterogeneity in our scientific inquiry, and this got John wondering, I think, about whether different standards of value concerning the scientific enterprise are lurking in the background. People who think of science as impressive and interesting because it has enabled us to learn about aspects and features of the world that our predecessors could only dream about will see the prospect of underdetermination as threatening—as a problem or challenge that makes the central goal and source of value for scientific inquiry harder to achieve. But one might instead see science as valuable and interesting in the way that art is often taken to be: as a fundamental expression of human creativity and virtuosity. On this view the products of science are valuable not primarily because they tell us about the world (even if they do) but instead because, like paintings and musical

compositions, they are some of the most curious, complex, wonderful, and fascinating products of the human intellect. If that is their primary source of value, then the more of them the better, so all hail underdetermination, a condition which, when recognized, drives us to generate even more of these creative marvels. After all, who would wish for there to be fewer interesting movies, songs, or paintings in the world? (The version of the query in John's written remarks here seems less concerned to distinguish these questions about standards of value from claims about the accomplishments of science, but I think this has again invited people to start talking about what theses they think they can defend, rather than what motivates their interest and sympathies in the dispute.)

If I've got John's question right, I'm afraid that my answer will disappoint: I think this difference is real, but I don't think it is what drives allegiance in the disputes over underdetermination. I will take myself as a case in point. My own view is that scientific theories are indeed marvels of human creativity, but that their ability to serve as instrumental tools for getting around in the world and addressing our practical needs is what is most incredible about them (after all in science but not art, *the world pushes back*). It is further reflection on the evidence for the problem of unconceived alternatives embedded in the historical record of scientific inquiry itself that leads me to doubt whether our theories manage to do this by simply reporting to us how things stand in otherwise inaccessible domains of nature. Thus, if there is a non-argumentative affinity serving as the wellspring of my own sense that some claims of underdetermination are important, it is probably an appreciation of the sort of contingency that seems to permeate the historical record of scientific inquiry and what that contingency seems to reveal about the limits of our ability to limn the world's depths in particular epistemic circumstances, not a general enthusiasm for fostering more creative scientific theorizing rather

than less. Of course, upon further reflection such contingency might not seem so surprising after all: there is little reason to think that the cognitive abilities and conceptual categories of creatures selected for prowess in finding food, shelter, and mates out on the savannah will also be especially well-suited for uncovering the structure of matter, or of spacetime, or for unraveling the deep history of the cosmos.