Are Astrophysical Models Permanently Underdetermined?

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1. Introduction

In the spring of 2014, astrophysicists and cosmologists were reminded of the significance of models for their work when purported evidence for the birth of the universe was chalked up to nothing but dust. The BICEP2 Collaboration's discovery of B-modes in the polarization of the Cosmic Microwave Background (CMB) was initially touted as the "smoking gun" for a period of inflation in the very early universe. However, detractors responded quickly, arguing that the signal could be due simply to underestimated effects of dust in our galaxy.[1] The result's validity hinges crucially on the availability of foreground maps depicting expected contributions from nuisance sources obscuring the background signal. Astrophysicists use models of galactic dust to generate such maps, which are then subtracted from data sets to reveal the faint structure of the CMB.[2] Due to present uncertainties surrounding the properties of interstellar dust, these models often involve phenomenological equations, in which parameters that do not directly correlate to physical quantities are fit to observational data. The scientific merit of phenomenological models derives from their

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[1] See BICEP2 Collaboration (2014), especially their "Note added" on page 22, which discusses some of the responses to the discovery announcement.

[2] See e.g. Planck Collaboration (2014, 7).

utility and investigators can often generate multiple models without compromising their research aims.[3]

However, not all models in astrophysics are built in this manner. Although there are models that have been generated according to a mixture of aims, we can identify two ends of the modeling spectrum: phenomenological and physical. Some astrophysical models are generated with the requirement of physical plausibility as a priority. This luxury is typically afforded when the models are constructed with the aim of faithfully representing a phenomenon rather than for use in some other data analysis project. For instance, some astrophysicists have worked to faithfully represent the formation and evolution of galaxies in their models.[4] In contrast to the case of phenomenological models, when multiple physically motivated models of the same phenomenon can be produced that fit available data equally well, the models are problematically underdetermined.

The type of concrete, local, contrastive underdetermination discussed here differs from other well-known types of underdetermination. For instance, it occurs at the circumscribed level of competition between models of the same target rather than at the broader level of Duhemian holism.[5] I presuppose that there are some cases where scientists

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[3] See BICEP2 Collaboration (2014, 15) for five models used to evaluate the contribution of dust to the observed signal.

[4] I discuss present underdetermination between such models of galaxy formation in Section 6.

[5] See Duhem (1906/1954).

can agree on the reliability of their auxiliary assumptions and isolate underdetermination to competition between specific incompatible models of a target. Similarly, our focus differs from that of Stanford's (2006) argument regarding the significance of unconceived alternatives. If modelers suspect there could be other physically plausible models that they have yet to explore, then they should actually invest in that exploration. Otherwise, the way to make headway in science is to severely test the models that have actually been worked out, and to extend the scope of available data in the hopes of unearthing new inspiration.[6] When modelers do manage to conceive of multiple empirically supported alternatives, they need some prescription for breaking the underdetermination if they are to retain confidence that representational fidelity of their models is improving over time.

Philosophers have recently argued that permanent underdetermination is to some extent unavoidable in cosmology. For instance, drawing on results from John Manchak (2009; 2011) and John Norton (2011), Jeremy Butterfield (2014) argues that observationally indistinguishable models of spacetime will be permanently underdetermined, rejecting various attempts to justify the assumption cosmologists usually invoke to narrow the space of models, namely, that the universe is homogeneous and isotropic even in regions unobservable in principle. Such incorrigible underdetermination blocks confidence that developing the models further will increase their representational fidelity. How far does this barrier to scientific investigation extend?

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[6] For a relevant account of severe testing see Mayo (2010).

According to Ian Hacking (1983, 1989), the rot stretches well beyond the fringes of cosmology, all the way to the very heart of astronomy. In fact, he has gone so far as to claim that “astronomy is not a natural science at all” (1989, 577). Hacking’s position is that one cannot help but be a realist about entities that one uses as tools for the investigation of some other phenomenon. In the final two paragraphs of *Representing and Intervening* he pointed out that one cannot be cornered into realism about astrophysical entities such as black holes in this manner, since black holes simply are not manipulable. Similarly, he has protested “we cannot experiment on or with the sun” and “[g]alactic experimentation is science fiction, while extragalactic experimentation is a bad joke” (Hacking 1989, 559).

Indeed, Hacking has directly criticized astrophysical modeling, disputing the epistemic status of the field as a whole by arguing that astrophysical models only “save the phenomena” (ibid., 577). As a result, he advocates “astrophysical antirealism” (ibid., 555). Shapere (1993) and Rockmann (1998) have subsequently offered convincing critiques of Hacking' position, framing compelling philosophical counterarguments and pointing out various ways in which it seems that Hacking has misunderstood and/or misrepresented the scientific details regarding his primary case study: modeling gravitational lensing systems. While there is prima facie reason to think that our empirical access to the surface of the sun exceeds that to remote quasars, Hacking is wrong to suggest that accessibility scales simply according to the proximity.

However, Stéphanie Ruphy’s (2011) argument can be seen as renewed attempt at the sort of pessimism about the representational fidelity of astrophysical models that Hacking raised.[7] Although she does not cite his work explicitly, Ruphy continues Hacking's tradition by arguing for "a modest view of the epistemic function" of astrophysical models, i.e. as "merely saving the phenomena at hand rather than providing reliable knowledge on its underlying physics" (2011, 189). However unlike Hacking, her argument does not rely on his characteristic views regarding entity realism and she discusses examples of astrophysical models that Hacking does not. Instead, Ruphy argues that underdetermination in astrophysics will generally be persistent since structures can always be added to the competing models to maintain their empirical successes. If correct, her conclusion would undercut the aim of increasing the representational fidelity of astrophysical models, leaving them fit only for instrumental use in the manner of the galactic dust models.

While I think there are important epistemic questions to ask about empirical access that scientists have to phenomena that typically fall under the purview of astronomers, the present paper focuses on research in astrophysics. In discussing astrophysical models, I aim to address what Hacking and Ruphy take to be the more recalcitrant cases. In this paper, I argue that there is good reason to think that many cases of underdetermination arising in representation-driven astrophysical modeling will turn out to be transient—forthcoming evidence can be expected to resolve competition between multiple presently viable models. There are very real limitations to empirical access in the investigation of astrophysical

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[7] Ruphy also discusses a cosmological simulation, which I will not explore here. The two models of the Milky Way are the only astrophysical models she analyzes.

phenomena—black holes, gravitational lensing, quasars, gamma-ray bursts, star formation, galaxy formation, and so on. Even relatively close to home, the observation of large swaths of our galaxy in the visible wavelengths is simply precluded by that troublesome obscuring dust. Despite such challenges, astrophysicists enjoy relatively liberal empirical access in comparison to their colleagues in cosmology; often the task of collecting evidence crucial to advancing astrophysical models does not run up against the principled empirical horizons encountered in cosmological modeling.

The structure of the paper is as follows. In the next section I explicate the conditions under which underdetermination of physically plausible models may be expected to dissolve. I present and evaluate Ruphy's argument in Section 3, showing that it is missing support for the crucial claim that underdetermination is endemic to the field. In addition, I raise doubts regarding her claim that the models she discusses are in fact underdetermined and that the empirical successes of the models will be maintained in the manner she proposes. In Section 4 I offer my own example of underdetermination in astrophysics. Currently available evidence does not discriminate between two plausible alternative models of the dominant instability driving core-collapse supernovae. However, as I show in Section 5, modelers can reasonably expect to distinguish differential empirical support across these models in light of forthcoming observational evidence. In Section 6 I argue that my example is not uncharacteristically straightforward and mention three further examples of present underdetermination in representation-driven modeling lineages from some of the most prominent areas of astrophysical research (dark matter, structure formation, and gamma-ray bursts), which modelers have good reason to expect to be temporary. Taken together, these examples count against broad pessimism about the prospects for breaking underdetermination of physically plausible astrophysical models.

2. Transient underdetermination of physically plausible models

Scientists have many diverse aims ranging from generating accurate predictions, to satisfying explanations, to new technological innovations. They also sometimes aim to improve the fidelity with which certain models represent their targets. I take an inclusive view of models, which can include for instance mathematical objects, descriptions, simulations, plots and diagrams.[8] Models are often part of a larger body of active research.[9] An initial model may be altered so as to produce a lineage of subsequent models, which vary in resemblance to their progenitors. Aiming at increasing the representational fidelity of one's model involves aiming at increasing the representational fidelity of the model from its current state, no matter how presently misrepresentative that state is. When a model is altered by a physically

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[8] See Morgan and Morrison (1999, 11) for an account of the representative function of models as 'investigative instruments'. For modeling practices closer to that described here, although with a significantly different aim see Borrelli (2012, 195). See Suárez (2014) for an argument that astrophysical models of the internal structure of stars are useful fictions.

[9] Shapere (1993) also advocates for a dynamic account of astrophysical modeling.

plausible modification such that its empirical success improves, scientists have good reason to think that the representational fidelity of that model has been increased.

The notion of "physical plausibility" requires further explication. Consider a lineage of models that have been modified only in order to reproduce available observational data. Alone, this condition does not filter out phenomenological models of primarily instrumental utility. To do so, modelers may also require that their models be physically plausible. This requirement restricts the addition or adjustment of elements that are not motivated by known or suspected physical correlates. There are some important exceptions. For example, approximation schemes that can be demonstrated to do little harm to the representational fidelity of the target features of interest are often necessary. In addition, temporary patches may be tolerated in representation-driven modeling as long as they are identified and can be expected to be replaced by more realistic successors in future models. Scientists know that ad hoc modifications and blind curve-fitting are poor methods for generating better representations. Rather, models in a representation-driven lineage are altered in order to incorporate significant well-known features of their targets as well as new evidence.

However, when multiple such models can be maintained so as to fit available observations, confidence in the increasing representational fidelity of any given one of them is undermined. The successful continuation of representation-driven models thus relies not only on the increasing empirical success and physical plausibility of such models, but also on the condition that underdetermination is transitory. Let us stipulate that models of the same target are underdetermined only if a) they purportedly represent the same aspects of the same physical system at the same scale and b) they are not merely variants of the same model expressed differently.[10] In other words, genuinely underdetermined models compete for the same epistemic niche.

There is good reason to expect transient underdetermination to be a ubiquitous feature of improving model lineages. Faced with a scientific problem (e.g. an unknown mechanism or a discrepancy between observations and existing theory) modelers will try to generate physically plausible candidates in their attempts to solve it. At first it may be possible to construct several empirically adequate competitors. However, these different models may then be tested against one another by capitalizing on their distinguishing features to make new predictions regarding forthcoming data. In other words, a stage of underdetermination is a germane feature of the long road that is representation-driven modeling in empirical science.

Breaking underdetermination requires discriminating empirical evidence. One might want to argue that in the absence of such evidence, underdetermination could nevertheless be broken by appeal to extra-empirical virtues such as ‘parsimony’ or ‘explanatory power’. Unfortunately, these virtues need not track the representational fidelity of the models.[11]

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[10] See Bailer-Jones (2000) and Norton (2008) respectively for discussions of violations of these conditions.

[11] For alternative positions on the epistemic import of empirical evidence in astrophysics and cosmology see Kragh (2014), Ellis (2006, 2014), and Morrison (2009).

Consequently, if additional relevant evidence is not forthcoming in a given context, it will be difficult to be optimistic that present cases of underdetermination will be resolved.

Thus, breaking underdetermination requires first that modelers in fact extract different predictions from the contending models. Genuinely underdetermined models will necessarily be different in some respects. For instance, models that describe distinct physical phenomena will certainly have distinguishing features. However, whether or not modelers can in practice extract distinct predictions from these features is a further matter. The viability of generating such predictions will be indexed to the particular models and their relation to observables.

Second, the evidence relevant to those predictions must actually be collected. Whether or not such evidence will in fact be collected in the future can of course not be known beforehand. However, scientists can have good reasons to think that nothing prevents the collection of such evidence and (in the most promising cases) that the collection of such evidence is to be expected given imminently realizable experiments or observations.

Finally, once the relevant data has been collected modelers must actually use it to evaluate the previously underdetermined models. Thus, modelers can reasonably expect that present underdetermination will turn out to be transient when 1) distinguishing features can be extracted from the competing alternatives, 2) differential empirical evidence can likely be collected, and 3) model features can be evaluated in light of such evidence.

3. Ruphy’s Astrophysical Pessimism

Ruphy (2011) has argued that models in astrophysics are generally permanently underdetermined. Her argument takes the following form: she presents an example of model underdetermination, argues that we should expect the present underdetermination to be permanent, and claims that this expectation should generalize to the field broadly. Ruphy motivates the anticipation of permanent underdetermination with an account of how models are modified in the face of new evidence. She argues that in astrophysics, multiple empirically successful models of the same target can be constructed and maintained. In particular, new structures can be added to existing models as needed to accommodate new evidence, without changing the key components of the models. Ruphy's astrophysical case study involves models of our galaxy. She claims that multiple incompatible extant Milky Way models can be modified as required to fit observations by, for example, adding representations of more stellar populations (ibid., 189).

Ruphy extends the consequences of her case studies, claiming that they "reveal a *general* tension in computer simulation between realistic ambitions and the possibility of empirical confirmation" (ibid., abstract, emphasis added). In addition, in setting out the structure of her paper Ruphy claims that her "analysis will also shed light on a *rather widespread* form of representational pluralism in astrophysics and cosmology that follows from path dependency, to wit, permanent incompatible pluralism" (ibid., 179, emphasis added).

However, Ruphy never argues for the generality of such a tension or for the claim that underdetermination is "rather widespread" in astrophysics and cosmology; she simply passes from her case studies directly to her general epistemological lessons. For instance, she does not furnish reasons to think that the modeling practices involved in these particular examples should generalize. She chose to describe two galactic models (the SKY model and the Besançon model) that she states "happen to be the two most fully developed ones" (ibid., 186). However, without further discussion the reader is left to wonder if these models have been developed in a manner uncharacteristic of the field. This oversight significantly effects the plausibility of Ruphy's general claims. The conclusion of her paper, "that the prospects for realism (as opposed to the more modest epistemic aim of empirical adequacy) seem rather dim *in astrophysics and cosmology*" is simply not supported by her argument (ibid., 191-192, emphasis added).

In the remainder of this section I argue that in fact even Ruphy's own case study fails as a straightforward example of permanent underdetermination. As a result, whatever features of the example do generalize in the field (if any), they will not support Ruphy's broad pessimism. Let us consider in turn two elements of Ruphy's argument for the permanent status of galactic model underdetermination. First, she must establish that the models she discusses are in fact presently underdetermined, that is, that the two galactic models of the same target are both physically plausible and empirically successful. Second, she must show that the underdetermination may be expected to persist.

4.1 On Present Underdetermination

Ruphy argues that the galactic models are of the same target, plausible, incompatible, and that they are similarly supported by available empirical evidence (ibid., 186-87). If this were the case, then the models would be genuinely underdetermined. In more detail, Ruphy states that the models "have roughly the same intended content" and in particular, both models are aimed at "coming up with a realistic pictures of the main structural components of the Milky Way, both models aim to integrate the right structural components with their true physical characteristics (composition and shape in particular)" (ibid., 186). In other words, in both cases, modelers aim at representing the same galaxy faithfully by integrating physically plausible structures into their models.

However, the Milky Way models that Ruphy discusses are ill-chosen examples for her conclusion since, according to Ruphy, a galactic model in part serves "practical goals" and is used "as a tool of prediction" (ibid., 185). In other words, these models of the galaxy were in part developed instrumentally, in the manner of the dust models described at the outset of this paper. It is true that the emphasis in this model lineage has shifted recently towards representing the physical processes of galaxy formation and evolution. However, progenitors of the Besançon model were initially developed in the early 1980s for use in processing observations from the Hubble Space Telescope (Czekaj et al. 2014). Due to the practical value of galactic models, and the history of their development, it is not so surprising their representational fidelity should have been compromised in the service of efficient predictions accurate enough for application in observational campaigns. So although both models are relevant to the same physical system, the modelers' intentions were part representational and part instrumental, jeopardizing the condition that modeling choices have been made to increase or preserve physical plausibility. This casts doubt on the claim that the models are genuinely underdetermined. However, considered as instruments for data analysis, the existence of multiple successful models is epistemically unproblematic.

Setting aside the utility of the galactic models, we can also appraise their status qua representations. Ruphy claims that the models overlap in their purported representational content and that "both models enjoy comparable empirical support" in that they "conform roughly to the set of available and relevant observations" (Ruphy 2011, 187). Yet the galactic models are incompatible, Ruphy reports, since their features are significantly different. For example, stellar populations are distributed differently in the two models, the radial extent of the disk of the galaxy is different, and one model features a thick stellar disk while the other features a "molecular ring" (ibid.). Notice the striking character of this element of Ruphy's position. Put simply, she claims that models can conflict on basic aspects of the structure of our galaxy, and yet both agree with available evidence. To the uninitiated, this may seem unlikely, so it is worthwhile to evaluate the claim in some detail.

According to Ruphy, the incompatible features of the models cannot be assessed empirically. She argues that the empirical successes of the galactic models, such as they are, could be explained by the integration of multiple inaccurate lower level "submodels" that go into the construction of the two representations of the galaxy. However, according to Ruphy, these submodels cannot be directly checked against observations since "to make contact with data, a submodel often needs to be interlocked with other submodels" and so adding more components merely further obscures these inaccessible parts (ibid.). As a result, she concludes that "the empirical success of the whole group is a poor guide to the representational accuracy of the submodels" (ibid., 188). Such holism serves to prevent attempts to investigate the differences between models, which could be used to break the underdetermination.

In fact, Ruphy seems to suggest here that there is no way to distinguish whether or not our galaxy has a thick stellar disk or not by way of observation. However she does not provide any compelling reason to think that this is the case. Ruphy mentions the fact that from our vantage point *within* the Milky Way, we do not have the liberty of appraising its shape from afar, claiming "that no outside vantage point is available for direct observation of the shape, size, and composition of the main structural components of the Milky Way" (ibid., 185).

It is true that our perspective in the galaxy presents astronomers with a geometrical puzzle and that even today relatively little is known about the Milky Way's structure. For instance, as recently as 2009 Reid et al. reported that "after decades of study there is little agreement on this structure" and that "we do not really know the number of spiral arms…or how tightly wound is their pattern" (137). Yet why should our internal perspective hopelessly hinder observations of galactic characteristics such as shape, size, composition, and structure? In fact, observers have been able to refine the accuracy with which they can determine distances to astronomical objects as knowledge of the intrinsic properties of stars and observational techniques have improved. There is no in-principle barrier to obtaining accurate knowledge about the constituents of the galaxy. Observations from COBE, 2MASS, Spitzer and other astronomical surveys have significantly contributed to our current best maps of the Milky Way.[12]

Indeed, Reid et al. (2014) have used recent results from The Bar and Spiral Structure Legacy Survey (BeSSeL) to map the structure of some of the galaxy's spiral arms using parallax measurements of high-mass star forming regions in the galaxy, avoiding some of the difficulties that beset earlier kinematic surveys. Reid et al. then used their data to fit a model of the galaxy that includes physically plausible parameters such as disk rotation speed, solar motion in the direction of galactic rotation, the average peculiar motion of sources towards the galactic center and so on (2014, 6).

In addition, new discoveries of structural elements of the galaxy continue to be made. Evidence for the so-called "Fermi bubbles" (25,000 light-year gamma-ray emitting lobes extending to the galactic poles) extracted from data collected using the Fermi Gamma-ray Space Telescope's Large Area Telescope was published in just 2010 (Su, Slatyer, and Finkbeiner). Faithful models of the galaxy will eventually have to account for these structures.

In light of such progress, it is unclear why Ruphy thinks that discrepancies regarding galactic structure cannot or will not be settled by observations. For instance, although there is still debate regarding the existence of a "molecular ring" in our galaxy, relevant data sets

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[12] See e.g. Waller (2013, 201-9).

(e.g. the Galactic Ring Survey, which mapped carbon monoxide emission in the galaxy) are available and active research into the matter continues.[13] Even if Ruphy is correct in claiming that the SKY and Besançon models presently enjoy comparable empirical support, insofar as the modelers aim to faithfully represent the physical characteristics of the galaxy they should be able to leverage discrepancies between the models to uncover differential empirical support between them as new data piles up.

4.2 On Permanent Underdetermination

Ruphy advocates the permanence of galactic model underdetermination by claiming that for any new observation there will always be a physically plausible alteration that can be made such that the empirical success of multiple models is sustained. She writes,

> the adjustment processes…do not boil down to the ad hoc tinkering of the model…a galactic model cannot be tuned to fit new data by merely playing with the values of its free parameters…adjusting the model when new data come in often requires incorporating new structural components or/and new populations of stars (Ruphy 2011, 189).

Yet, according to Ruphy, underdetermination persists in the case of galactic models since new components can be added to those models to maintain their empirical successes without disrupting prior components (ibid.). Such adjustments pertain to the elements of the specific

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[13] See e.g. Roman-Duval et al. (2010); Dobbs and Burkert (2012).

model in question, leaving intact auxiliary assumptions about, for example, the reliability of the instruments used in data collection (ibid., 188). As a result, she concludes that the empirical success of such a model "cannot be taken as a reliable sign that it gets the relevant underlying physics right" (ibid., 190). The picture of modeling practice that emerges is something like runaway accretion: more structures could be added to old models as needed to accommodate new observational data and "fit between new data and outcomes of the model *can* be obtained without altering previously chosen key ingredients of the model" (ibid., emphasis added).

However, in order to motivate her pessimism about astrophysical modeling generally, Ruphy would have to argue not only that such accretion is *possible*, but that it is a feature of actual scientific practice. Generally, this turns out to be false. Astrophysicists have changed the key ingredients of their models in response to new empirical evidence. This has certainly been the case in the history of Milky Way models, as is immediately evident from the striking differences in structure between William Herschel's galactic model (which featured two flat bars or "strata" of stars interleaved) to Jacobus Kapteyn's model (including great "streams" of stars) to the contemporary consensus that the Milky Way is a spiral of some sort.[14]

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[14] See e.g. Hoskin (2012), van der Kruit and van Berkel (2000), and Reid et al. (2014) respectively.

Moreover, it is not clear that physically plausible structures *can* indeed be added to multiple models ad infinitum without revising existing features of the models. Ruphy does not provide an account circumscribing the properties of acceptable model additions. Elsewhere, discussing cosmological modeling practices, she does state that "alternative pictures would be equally plausible in the sense that they would also be consistent both with the observations at hand and with our current theoretical knowledge" (ibid., 184). This suggests that plausible additions to astrophysical models would be similarly motivated by background or auxiliary knowledge, for example knowledge that the modelers have about the composition and structure of other galaxies, gravitating systems generally, the behavior of hot gases, and the evolution of stars. However, according to Ruphy, astrophysicists do not derive their models from theory "nor do they proceed by abstraction, idealization, or simplification of a phenomenon they would have observed beforehand" but instead "grope their way toward agreement between predictions and observations by incorporating various ingredients that they *imagine* are parts of the real object" (ibid., 185, emphasis added).

In other words, Ruphy suggests that structures added to models in order to maintain their status as empirically successful in the face of new evidence are wrought from the fertile "imaginations" of modelers. However, the absence of justification of the sort Ruphy precludes (including theoretical motivation and inference from existing observations) conflicts with the expectation that model additions actually be physically plausible. Without these constraints, what would prevent the imagined components from drifting into fanciful speculation?

To summarize, Ruphy has not demonstrated that persistent underdetermination should be expected in the case of galactic models. There is at least some reason to doubt that the models she considers have been developed primarily with the aim of increasing representational fidelity. In fact, insofar as we consider the aspects of galactic models that are intended to faithfully represent features of the Milky Way, cases of present underdetermination may reasonably be expected to resolve with forthcoming evidence. Models of the galaxy have significantly evolved over time, recent progress in galactic cartography has been driven by observations of the Milky Way's structure, and further observations are on the horizon. Alternatively, insofar as we consider the models in their instrumental capacity—as useful for application in data analysis—the multiplicity of models presently on offer poses no problem: it does not amount to genuine underdetermination. As a result, Ruphy's case study does not support *general* pessimism about representation-driven astrophysical modeling. Indeed, evaluating whether an instance of present underdetermination can be expected to be transient requires considering case-specific details. In many astrophysical cases, the appropriate degree of optimism depends on contingent factors influencing the availability of new evidence. I turn now to discuss the prospects for breaking the underdetermination of a contemporary pair of astrophysical models.

4. Present Underdetermination in Supernovae Modeling

There are two competing models of the dominant mechanism driving explosions in core-collapse supernovae (CCSN), which are underdetermined by presently available evidence:

convection-driven neutrino reheating and standing-accretion shock instability. These competing models have been generated in the context of ongoing research to represent the physical processes involved in these supernovae. Model alterations have been motivated by background physical knowledge as well as striking observational evidence. In the absence of underdetermination, modelers would have good reasons to be optimistic that further efforts would continue to improve the representational fidelity of CCSN models.

To show that supernova models constitute a genuine case of underdetermination, it will be necessary to provide some preliminary details about the target phenomena and the efforts of astrophysicists to model them. Perhaps *the* most salient characteristic of a supernova is that it is an explosion. Yet despite the fact that significant research on CCSN has been ongoing for about fifty years, no consensus has been reached regarding the mechanism by which the stars explode. In fact, many models of core-collapses have produced duds—they just do not explode at all. The basic difficulty encountered in modeling a supernova is easy to understand.[15] According to the *bounce-shock* model (or *hydrodynamical mechanism*) the massive star is thought to exhaust its fuel and collapse under the influence of its own gravity until the core of the star becomes as dense as an atomic nucleus, forming a protoneutron star. The collapse is interrupted as material from the stellar

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[15] See Benz, Stirling, and Herant (1994) and Herant et al. (1997) for discussions of the aspects of CCSN modeling summarized here.

envelope runs up against the effectively incompressible core and bounces, sending a shock wave back out into the incoming mass around the core.

Initially modelers thought that this gravitational collapse and the ensuing shock and bounce would drive the *prompt* explosion of the star. However, the outgoing shock is now thought to lose energy from the release of neutrinos and nuclear dissociation and to *stall* as a result, thereby failing to eject the supersonic imploding outer shells of mass in an explosion. In other words, astrophysicists do not yet know how and why CCSN explode on the basis of the bounce-shock model.

While a simple model of supernovae was presented as early as 1960 by Colgate and Johnson, it was not until 1991 that supernovae modeling reached out of spherically symmetric models expressed in 1D into multidimensional simulations (Herant et al. 1997, 75). Indeed, about a decade ago, Limogni and Chieffi (2003) reported: “To date, models of the core-collapse supernovae in spherical symmetry have not yielded successful explosions” (406).

The transition away from symmetry was at least in part driven by observations made in 1987 of a supernovae in the relatively nearby Large Magellanic cloud. In particular, the so-called “Bochum event”, which refers to fine structure appearing in the spectrum of the supernova weeks after it was first observed, has been interpreted as revealing the rapid ejection of a clump of nickel (Utrobin, Chugai, and Andronova 1995). The Bochum event indicated that elements in the stellar envelope of the supernova progenitor were not arranged neatly into static shells like the layers of an onion. Convection flows in the star could mix envelope composition and produce such travelling clumps, however this asymmetric process could simply not be included until the models became at least 2D. Once multidimensional simulation got underway, the modeling community came to agree that convection should be considered an essential element of CCSN modeling. Herant et al. describe this shift in model building as “a new paradigm in which the supernova is viewed as a convective engine” (1997, 76).

This new approach opened up possible solutions to the CCSN explosion problem. Recent efforts countenance CCSN explosions as *delayed* rather than prompt, supposing that some as-of-yet neglected mechanism re-energizes the stalled shock front and completes the explosion. In models belonging to the *neutrino reheating paradigm*, neutrinos transported in a convective cycle deposit energy on the inner side of the stalled shock. If enough energy is deposited, the shock wave restarts and explodes outwards through the rest of the envelope. Thus, the convective mechanism provides one physically plausible and presently empirically successful model of CCSN explosions.

However, circa 2003 another multidimensional hydrodynamic instability arose in simulations modeling CCSN.[16] Standing-accretion shock instability (SASI) is an advective-acoustic cycle wherein perturbations originating at the shock front are advected to the central protoneutron star where they generate acoustic perturbations, which are in turn fed back to the shock front and serve to amplify the perturbations there (Ott et al. 2013, 2). This cycle

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[16] See Blondin, Mezzacappa, and DeMarino (2003) and references therein.

produces unstable large-amplitude low-order modes that have been described as "large-scale…sloshing motions of the shock front in three dimensions" (Hanke et al. 2013, 1). Neutrino reheating and SASI are thus two competing models of the physical process driving CCSN explosions. It may well be that both types of instabilities contribute to re-initiating explosions in CCSN.[17] However, modelers can still ask which instability is energetically dominant in this physical process: is the dominant instability produced by convection or sloshing? Unfortunately, no discerning data is currently available and so the two models are underdetermined at present.

The modeling lineages in which these two competitors developed are representation-driven and the modeling choices in those lineages have been made to both progressively increase empirical successes of the models as well as improve the physical plausibility of the models. These features support optimism about the increasing representational fidelity of CCSN models. However, if the present state of underdetermination is expected to be permanent, continued optimism will not be warranted. What are the prospects for breaking this underdetermination in the future?

5. Forthcoming Evidence on Hot vs. SASI

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[17] See Mitchell (2002) for an account of compatible pluralism involving models of multiple contributing causal factors at the same level of analysis, which must be integrated in concrete applications.

CCSN modeling exhibits the three features that warrant anticipating the transience of the present epistemic deadlock: 1) distinguishing features can be extracted from the competing alternatives, 2) differential empirical evidence can likely be collected, and 3) model features can be evaluated in light of such evidence. This provides us with good reason to remain optimistic about attaining increasingly representative models of these astrophysical systems.

Recent efforts have focused on trying to distinguish which of the instabilities is dominant. In particular, Ott et al. (2013) have simulated the collapse of a star using a general relativistic representation in 3D. This work was evidently inspired in part by the prior restriction of SASI's demonstrated potential to 2D simulations: would the same hold in 3D or are SASIs successes so far an artifact of low-dimensional modeling? Both SASI and neutrino-driven convection arose in the simulation, although the SASI oscillations died out while the convection became the dominant instability (ibid., 3). The dominance of convection over SASI in this simulation does not by itself demonstrate that convection drives explosions in actual CCSN. However, the authors were interested in extracting specific predictions regarding the production of neutrinos and gravitational waves during supernovae with the express aim of using those predictions to distinguish the significance of the two instabilities in real stars. In particular, they suggest that "neutrinos and gravitational waves may be direct probes of progenitor properties, supernova dynamics, and of the explosion mechanism" (ibid., footnote 12). While the authors admit that approximations made in the treatment of neutrinos in their model prevent them from generating useful predictions regarding the neutrino signal expected in observations of supernovae, they do argue that the gravitational wave signal extracted from their simulations can be usefully compared to observations.

For this convection-dominant simulation, the authors find an initial burst of gravitational waves with frequencies around 100-200 Hz followed by another at 400-1000 Hz (ibid., 22). They suggest that the production of this initial burst of gravitational waves would be uncharacteristic of SASI, which would instead produce a “slow rise to smaller amplitudes at later times” (ibid.). With the right observational data, these distinguishing characteristics would serve to differentiate models that are both presently viable according to available observational data.[18]

In other words, the gravitational wave signal that Ott et al. have extracted from their simulation could realistically provide a way to place constraints on convection-driven neutrino reheating vs. SASI dominance in observations of future supernovae originating in our galaxy, if such events are observed using gravitational wave-detectors such as Advanced LIGO, Advanced Virgo, or KAGRA (ibid.). In addition, perhaps the representation of neutrino physics in the model of Ott et al. can be elaborated and improved such that neutrino spectra *could* be extracted in the future and thereby serve as another set of distinguishing

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[18] Again, Mayo (2010) helpfully describes this general process in her discussion regarding implications of her approach to confirmation (severe testing) at the level of whole theories (36, 51-52).

characteristics. The authors do not indicate any reason why this should be impossible in principle, and the incentive is compelling.

Recall that Ruphy's description of astrophysical modeling practice involved the possibility of perpetually adding new structures to old models as needed to accommodate any new observational data. This lead to the worry that genuine underdetermination could always be maintained. However, this picture of modeling methodology is pretty squarely at odds with that occurring in the CCSN case. In particular, CCSN modelers have had to radically revise the core commitments of their models. Neutrinos initially played no role at all in CCSN models, whereas today they are an essential element. Furthermore, it seems that modelers initially thought that they could represent CCSN well with spherically symmetric models. However, this turns out not to be the case since asymmetries have been observed in supernovae and are suspected to play a significant role in actually generating the explosions. As a matter of fact, CCSN modelers do not simply add structures to their models to accommodate new evidence. When they elaborate their models, they do so in physically motivated ways. For example, they add representations of processes that are expected to be relevant based on background physical knowledge.

Ruphy also worried that holism prevents direct assessment of submodels constituting an empirically successful composite model. However, as Lentz et al. (2012) demonstrate, it is possible for modelers to take steps to evaluate the significance and representational fidelity of submodels even once they have been assembled into composite models by systematically generating a series of simulations based on model variations. These authors try removing general relativistic effects, various details of neutrino opacity and other effects from CCSN and find that each of those components significantly "affect the progress of stellar collapse" in their simulations (ibid., 11). This information leads the authors to insist that all such components "must be included in multidimensional simulations of core-collapse supernovae to ensure physical fidelity" and that past models that have omitted such components "need to be better understood or phased out" (ibid., 12).

Currently there certainly is a dearth of data that would be desirable for evaluating proposed CCSN mechanisms. Nevertheless, the aim of CCSN model builders has been to account for the data that *is* available as well as to prepare predictions for future research. In the interim, agnosticism with respect to the competing models of supernovae instabilities is warranted. However, modelers have good reasons to expect that the present state of underdetermination will turn out to be transitory.

6. Further Prospects for Breaking Underdetermination in Astrophysics

One could object that the particular example I treat is a special (perhaps deceivingly easy) case, and worry that the vast majority of astrophysical modeling projects are more vulnerable to permanent underdetermination. However, modeling CCSN is not uncharacteristically straightforward. First, the physics of supernovae is staggeringly complicated, involving the confluence of gravitational physics, thermodynamics and nuclear physics. The difficulty of integrating all of the required physics in one model, and calculating the dynamical evolution of the stellar system in any detail requires the use of supercomputers. CCSN models typically have many parts (or 'submodels', in Ruphy's language) including those describing the state of the core of the star, the evolution of elements throughout the explosion, and the transport and interaction of various kinds of particles at high densities.

Second, naïvely one would like to directly observe the process of collapse and explosion in real stars. Unfortunately, the stellar envelope hides these processes from direct observation. Neutrinos and gravitational radiation can pass through the envelope of the star and carry information about the inner depths to observers on Earth. However, despite the tremendous number of neutrinos thought to be released in CCSN, only a few (about 19 total) have been collected so far due to the enormous distances between supernovae and detectors on Earth and no gravitation radiation from supernovae has been observed to date.[19]

Finally, astrophysicists interested in supernovae must be opportunistic. Recall Ruphy lamented that our position within the galaxy prevents us from observing it from the outside, suggesting that "[i]magination thus plays a crucial role in the building of galactic models" (2011, 185). But if observing the Milky Way from within is like trying to investigate an ocean while standing in the surf, the study of supernovae is more like whale watching. The last observed supernova occurring in the Milky Way was witnessed in 1604, that is, before the telescope and well before X-ray, infrared, neutrino and gravitational wave detectors.[20] Other astrophysical problems are difficult in comparable ways.

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[19] For a brief discussion of these observations see Giunti and Kim (2007, 528-30).

[20] See e.g. Adams et al. (2013).

There are numerous examples of in-progress astrophysical modeling in which presently viable contending models are expected to be culled as the field develops. While I cannot defend them all in the present paper, I mention three further examples of current model underdetermination in some of the most prominent areas of astrophysical research, which will likely break down with forthcoming evidence.

One of the most well-known topics in astrophysics is the hypothetical 'dark matter' proposed to explain unexpectedly flat galactic rotation curves. The idea is that 'halos' composed of a hitherto unknown type of non-baryonic particle extend well beyond the stars and gas in galaxies.[21] In contrast, proponents of Modified Newtonian Gravity (MOND) have argued that the aberrant rotation curves could be equally accommodated by an adjustment to gravitational theory.[22] These alternatives have been held up as paradigmatic cases of underdetermination (see e.g. Kosso 2013). However, preferential evidence for the dark matter proposal could be revealed if terrestrial experiments designed to detect candidate dark matter particles in our galaxy, such as the extant Axion Dark Matter eXperiment, are successful (see e.g. Hoskins 2011).[23] Dark matter vs. MOND thus serves as an addition example of underdetermination which astrophysicists can reasonably expect to expire.

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[21] On the proposal for dark matter see Zwicky (1937); for an early discussion of the structure of halos Navarro, Frenk, and White (1996).

[22] For the original proposal, see Milgrom (1983).

[23] Ruphy falsely states that "by definition dark matter cannot be observed" (2011, 180).

Another case of present underdetermination involves understanding the history of structure formation in the universe. Simplifying considerably, two viable broad-brush alternatives are the "top-down" and the "bottom-up" scenarios.[24] In the former, large structures are thought to form early and subsequently fragment into galaxy-sized objects while in the latter, relatively small objects combine together to make up larger structures later on. While much evidence has already been gathered from large-scale galaxy surveys such as the Sloan Digital Sky Survey, further data (especially from instruments such as the Hydrogen Epoch of Reionization Arrays) will continue to constrain structure formation models.[25]

Finally, much recent astrophysical research has focused on investigating the nature of short-duration gamma-ray bursts (GRBs). The origin of these bursts is still not well understood. Currently, multiple plausible models exist for GRB progenitors including merging compact binary objects (such as neutron stars and black hole pairs) or magnetars formed by a variety of different processes.[26] Although recent research preferentially supports the compact object merger model, more observations are required to distinguish between alternative progenitor scenarios. This data is expected to come from existing instruments

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[24] See Shandarin and Zeldovich (1989).

[25] See for example the National Research Council's (2011) panel report on Radio, Millimeter, and Submillimeter Astronomy from the Ground (439-99).

[26] See Berger (2011) for details regarding such models.

such as Swift and Fermi, as well as future gamma-ray surveys and gravitational wave detectors.[27] Such examples are easy to identify, and could be multiplied further.

7. Conclusion

I have argued against anticipating that underdetermination be permanent in astrophysics generally. The prospects for breaking underdetermination of models should be considered in the appropriate scientific context and appraised in light of the framework I have presented. Modelers can anticipate underdetermination to be transient when distinguishing features can be extracted from the competing alternatives, differential empirical evidence can likely be collected, and model features can be evaluated in light of such evidence. I have shown that in the case of underdetermination of CCSN models, distinguishing features have been extracted and discriminating evidence that can be used to differentially evaluate the models can reasonably be anticipated. In addition, I have suggested why we should expect more examples of this sort, offering three that could be developed in more detail. These cases count against the strong form of pessimism regarding the epistemic status of astrophysical models that Ruphy advocates. There is good reason to expect that in many instances the underdetermination of astrophysical models is only temporary.

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[27] Berger (2013) provides a review of open questions and prospects for their resolution in short-duration GRB research.

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