Chapter 2
Laboratory Astrophysics: Lessons for Epistemology of Astrophysics

Nora Mills Boyd

Abstract Astrophysics is often cast as an observational science, devoid of traditional experiments, along with astronomy and cosmology. Yet, a thriving field of experimental research exists called laboratory astrophysics. How should we make sense of this apparent tension? I argue that approaching the epistemology of astrophysics by attending to the production of empirical data and the aims of the research better illuminates both the successes and challenges of empirical research in astrophysics than evaluating the epistemology of astrophysics according to the presence or absence of experiments.

Keywords Experiment · Observation · Astrophysics · Dimensional analysis · External validity · Hydrodynamics

2.1 Introduction

If they mention astrophysics at all, philosophers of science often claim that experiments are impossible in astrophysics. The purported lack of experiments in astrophysics is usually taken to be a shortcoming of the field, an epistemic handicap. Indeed, the lack of experiments is painted as one of the most distinctive features of the epistemology of astrophysics in contrast to the so-called experimental sciences, thereby motivating special attention by philosophers of science. For example, Morrison (2015) and Jacquart (2020) have argued that, while lacking traditional experiments is a prima facie problem for astrophysics, astrophysicists successfully supplement their methodological toolbox by using computer simulations instead of experiments. Thus, the purported lack of experiment in astrophysics serves as a premise for arguments that simulation is an apt replacement for empirical research in astrophysics: "In the astrophysics case we may want to say that simulation is

N. M. Boyd (✉)
Department of Philosophy, Siena College, Loudonville, NY, USA
e-mail: nboyd@siena.edu

N. Mills Boyd et al. (eds.), *Philosophy of Astrophysics*, Synthese Library 472,
https://doi.org/10.1007/978-3-031-26618-8_2

an acceptable source of experimental knowledge simply because we are unable to conduct materially based experiments in the way we can with other types of systems" (Morrison 2015, 214).[1]

Rather than take up the question of whether simulations can really serve as an apt replacement for empirical research here (for the record: I doubt they can), I want to focus on the prior issue already assumed in arguments such as those of Morrison and Jacquart, regarding the role of experiments in the epistemology of astrophysics. Is it really the case that there are no experiments in astrophysics?

However we ultimately want to answer that question, we must admit that it is certainly the case that there are many experimental physics laboratories that identify themselves as dedicated to astrophysical research. The University of Washington's Center for Experimental Nuclear Physics and Astrophysics (CENPA), the Compact Accelerator System for Performing Astrophysical Research (CASPAR) at the former Homestake Gold Mine, the Laboratory for Underground Nuclear Astrophysics (LUNA) at Gran Sasso, and the Laboratory Astrophysics branch of Harvard's Center for Astrophysics are just a few examples. This prevalence of 'laboratory astrophysics' in contrast to the philosophers' denial of experiments in astrophysics raises a bit of a puzzle. Do researchers at these laboratories conduct astrophysics experiments after all? And how does the answer to that question reflect back on the epistemology of astrophysics—on what we can hope to learn through empirical research in astrophysics?

This chapter will argue that powerful similarity arguments available in physics can sometimes span terrestrial laboratory experiments and celestial systems. In other words, there are indeed experiments in astrophysics. But, like all external validity arguments, these powerful similarity arguments have limitations and can break down. Care must therefore be taken to ensure the conditions that support the desired argument obtain in the intended domains. In Sect. 2.2, I briefly discuss some relatively straightforward examples of laboratory astrophysics that illustrate both its long pedigree and how manipulating material in a terrestrial laboratory can count as astrophysical research. These examples show that astrophysics is not a purely observational science. In Sect. 2.3, I present a more detailed case study of a laboratory research that I will eventually argue (in Sect. 2.4) does not quite succeed in attaining its astrophysical aspirations. The reasons for this particular shortfall are instructive—they demonstrate the crucial importance of establishing that the appropriate conditions obtain to support the intended similarity argument. The final section highlights the main methodological lesson for philosophers of science interested in understanding the epistemology of astrophysics in practice.

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[1] Jacquart (2020) argues that simulations can be used for hypothesis testing in astrophysics: "because of the methodological challenges in astrophysics, comparison with observational data is extremely limited and in some cases impossible because there are no observations [. . .] I think it is clear that the simulations are not just testing a model but are playing the role of hypothesis testing in astrophysics [. . .] While a direct experiment would be helpful, as discussed above, for these kinds of systems in astrophysics this is the only means by which hypotheses can be tested" (1215).

The distinctions that we use to structure our inquiry can be fruitful for understanding science in practice or they can lead us astray. The distinction between observation and experiment has not served us well in appreciating the moves and arguments germane to empirical astrophysics. Instead, it is more fruitful to structure our inquiry by attending to what researchers in astrophysics are trying to study and to what in fact they have empirical access. In short: it's not whether it's an experiment that matters, it's how you use it.

2.2 Astrophysics as So-Called Observational Science

Astrophysics is often lumped under the description 'observational science' with fields like astronomy and cosmology. In the same breath, the lack of traditional experiments in astrophysics is taken to be an epistemic problem for astrophysics. The most extreme denigrator of astrophysics is undoubtedly Ian Hacking. In "Extragalactic Reality: The Case of Gravitational Lensing" Hacking quipped: "Galactic experimentation is science fiction, while extragalactic experimentation is a bad joke" (1989, 559). He explained, "the method of [astrophysics] is the same as that of astronomy in hellenistic times. Model, observe, and remodel in such a way as to save the phenomena" and in contrast, "[n]atural (experimental) science is a matter not of saving phenomena but of creating phenomena [. . .] But in astrophysics we cannot create phenomena, we can only save them" (577–578). Indeed he went so far as to say that "*astronomy* is not a natural science at all" and thus by implication, because it shares the same method, neither is astrophysics (577). This view of natural science is clearly too restrictive. Experiment, interference, and creation are not *necessary* for properly scientific research—surely at least *some* research in astronomy and astrophysics counts as bona fide natural science. However, Hacking is not alone in expressing the view that there's something wrong with astrophysics on account of the lack of experimentation in that field. We see this view reflected in the more recent work of some philosophers of astrophysics, as when Sibylle Anderl writes: "Astrophysics and cosmology share a common *problem* in that they both need to acquire knowledge of their objects of research without directly interacting, manipulating or constraining them" (2016, 653, my emphasis) and when Melissa Jacquart writes "Astrophysics faces methodological *challenges* as a result of being a predominantly observation-based science without access to traditional experiments" (2020, 1209, my emphasis). The common thought seems to be: experiments are impossible in astrophysics and astrophysics is epistemically poorer than it otherwise would be on that basis.

However, as I have already mentioned, it is not clear that astrophysics actually lacks experiments. In fact, astrophysics was born in the laboratory. The birth of astrophysics came with the application of physics to astronomy, in particular with the application of spectroscopy to light from the sun, and then to stars and nebulae (Becker 2011; Hearnshaw 2014). By comparing spectra thrown from elements committed to flame, arc, and spark in the laboratory to spectra from celestial sources, spectroscopists were able to match terrestrial sources with celestial ones. With this came the revolutionary possibility of determining the presence of particular elements in astronomical bodies (their chemical composition), and of determining the relative line-of-sight motion of such bodies via the determination of astronomical redshifts, thereby allowing for the addition of depth to our maps of cosmic structure.

Since those early days of astrophysics, the field has gained tremendous scope and embraced new aims and projects. Astrophysicists still use the chemical composition and redshift of celestial sources in their research, but also seek to understand the dynamical evolution of astronomical objects, processes, and systems, and the physical mechanisms in play. They investigate the causes and evolution of supernovae and their remnants, the formation of stars, planets, and galaxies, the flow of energy and material, the interactions of plasma, gravity, magnetic fields, and so on.

Still, in some ways just like in the early days of astronomical spectroscopy, astrophysics is about understanding the application of physics to astronomical targets and that application is often carried out in physics laboratories. There is a venerable branch of experimental physics devoted to accelerator-based nuclear astrophysics. With terrestrial accelerators, nuclear physicists can, and have, studied nuclear decay chains of astrophysical interest. Consider, for example, research on the second-forbidden beta decay of Boron-8. Solar neutrinos are produced by a combination of different nuclear reactions in the sun, and each of these needs to be carefully characterized in order to compare predictions to data from solar neutrino detectors. Although they are quite rare, some of the highest energy solar neutrinos originate from the second-forbidden beta decay of Boron-8 into the ground state of Berylium-8.[2] Nuclear physicists have studied the Boron-8 decay spectrum using terrestrial accelerators such as the University of Washington's Tandem Van de Graaff accelerator (Bacrania et al. 2007). In such nuclear physics experiments, researchers create conditions in the laboratory using ion sources, accelerators, and detectors to study the same kind of physical processes occurring elsewhere in the universe. Insofar as Boron and beta decays on Earth are of a kind with Boron and beta decays off-Earth, terrestrial accelerator experiments can study the very same kind of physical processes in the laboratory that are of astrophysical interest (see also Evans and Thébault 2020, Section 3). This is indeed 'experimental nuclear physics and astrophysics.'

There are also efforts to detect dark matter from our galactic halo in laboratory settings—that is, *not* waiting for celestial messengers to travel very long distances from their native environments to interact with detectors waiting to receive them on Earth, but rather capitalizing on the fact that our planet is swimming in a cosmic sea. For instance, some of these laboratory dark matter searches are being

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[2] Characterizing these branching ratios is important because if their decays are numerous enough, they could serve as a significant background for solar neutrino research, which at present is one of the main empirical access points to physics beyond the Standard Model of particle physics.

conducted using instruments that have been called ‘haloscopes’ because they aim to detect dark matter from our Milky Way’s own galactic halo right here on Earth. The Axion Dark Matter eXperiment (ADMX) is one example. ADMX aims to detect the signal of dark matter axions in a microwave cavity inside a powerful superconducting magnet housed in the basement of the Center for Experimental Nuclear Physics and Astrophysics at the University of Washington. The thought is that if galactic dark matter is composed of axions (undoubtedly a big “if”), then the magnetic field of the ADMX instrument will sometimes interact with these halo axions and produce a detectable signal. The dark matter axions would be expected in the laboratory microwave cavity, because we, the laboratory, and the cavity, are all riding along inside the Milky Way’s dark matter halo—we’re swimming in the stuff. In this research, the axions (if they exist) are not traveling from afar to be received by passive detectors. Rather, the experimental apparatus is intervening on the halo axions present in the laboratory via the strong magnetic field in the cavity. ADMX is just one example of laboratory research on an astrophysical target, from the relative comfort of our own planet. Empirical astrophysical research has also involved attempts at producing dark matter candidates using terrestrial particle accelerators (see e.g. Giagu 2019 and references therein).[3]

In short, astrophysics investigates the nature of celestial objects and processes using a suite of resources from physics, and some of that research—laboratory astrophysics—involves research in terrestrial laboratories. Laboratory astrophysics, even from its origins with astronomical spectroscopy, has involved studying conditions relevant to physics in space in laboratory settings, for instance by empirically investigating the spectra associated with different chemical elements and the spectra of decaying nuclei that occur throughout the universe. Thus, laboratory astrophysics includes investigation of physical phenomena that occur in both on-world and off-world settings. What makes astrophysics *astrophysics* is that it investigates the nature of celestial objects and processes using a suite of resources from physics. And what makes laboratory astrophysics *laboratory* astrophysics, is that it carries out such investigations using terrestrial experiments.

The very existence of laboratory astrophysics seems to undermine the ‘no experiments in astrophysics’ maxim we often see in philosophy of astrophysics. Moreover, the existence of laboratory astrophysics experiments might be surprising to those who conceive of astrophysics as a characteristically observational science (together with astronomy and cosmology). This surprise could lead us to expand our conception of astrophysics and to see the field as involving both observational and experimental research. Of course, someone like Hacking could still respond that the experiments employed in astrophysical research do not involve experimenting upon genuinely astrophysical targets—such as stars, black holes, supernovae and galaxies—and that it is this latter type of experimentation that would be relevant for

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[3] The story regarding analog black hole experiments is related, but more complicated. See Unruh 1981, Dardashti et al. 2017, 2019, Evans and Thébault 2020, Crowther et al. 2021, Field 2021, and Field manuscript. See also the contribution from Alex Mathie, Chap. 14 in this volume.

being promoted to the status of ‘experimental science’ and thus for the epistemic status of astrophysical knowledge.[4] But I think that this response misses what is so fascinating about the examples of laboratory astrophysics I have highlighted. Accelerator-based nuclear astrophysics, haloscope experiments, and dark matter production experiments all experiment upon targets that are instances of physical types that occur both on Earth and in space. As I will discuss further below, if one is unwilling to countenance these experiments as astrophysical experiments, then one should also be unwilling to countenance most laboratory experiments as intervening on their targets in the relevant sense since in virtue of being conducted in the laboratory, laboratory experiments do not intervene on instances of their targets in the wild, but rather on instances of the relevant type located in the laboratory. This would be counterproductive to the project of someone like Hacking, who certainly would not want to undermine the epistemic usefulness of all laboratory experiments. Of course, arguments do need to be furnished to support the crucial claim that the instances in the laboratory belong to the relevant type, and these arguments are not always successful (as indeed my primary case study below will illustrate). This is a general challenge for scientific research however, not a specific handicap of astrophysics.

For my own part, I think that noting the fact that there are experiments in astrophysics and that thus astrophysics is not a purely observational science is not, in itself, particularly interesting. This is because I think that a field can be empirical without performing experiments.[5] Indeed, I claim that the existence of laboratory astrophysics betrays the unhelpfulness of the distinction between observation and experiment for philosophy of astrophysics.[6] Ignoring that distinction, and replacing it with another framework allows us to better notice and theorize the epistemically significant aspects of laboratory astrophysics. This alternative framework helps us to see where the ‘epistemic action’ really is, in a way that is obscured when we approach this field of research with questions about observations versus experiments. What is philosophically interesting about laboratory astrophysics is not the existence of astrophysics experiments simpliciter, but rather the methodological and epistemological strategies that researchers use to study astrophysics in laboratory settings. Instead of attending to the distinction between ‘observation’ and ‘experiment’ (or ‘observational science’ and ‘experimental science’) in our

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[4] An anonymous reviewer helpfully suggested this possible response on behalf of my interlocutors.

[5] Thank you to an anonymous reviewer for pressing me to clarify this point.

[6] In fact, I think the distinction between observation and experiment is largely unhelpful for the epistemology of science more broadly, not just in the context of astrophysics. In a separate manuscript, coauthor Dana Matthiessen and I argue against the usefulness of this distinction in general (manuscript). We argue that philosophy of science ought to shift its focus to other features of empirical research methods that better track the epistemic benefits of methods that researchers choose between in practice. Here, I want to come at these issues from a different angle: the framework premised on there being an important epistemic difference between observational and experimental sciences is unilluminating for the epistemology of much significant empirical research in astrophysics.

investigation of the epistemology of astrophysics, we should attend to the production of empirical data. When we pay attention to the production of empirical data against the backdrop of the aims of the research, we can better resolve the challenges and opportunities of the field, and we can better appreciate the continuity of astrophysics with other fields of empirical research while also remaining sensitive to any distinctive philosophically interesting features it may have. In the following section, I present a case study that clearly shows the advantages of attending to the production of empirical data rather than the presence or absence of experiment for understanding the epistemology of the research.

2.3 Laboratory Supernova Research and Physical Similarity Arguments

Some laboratory astrophysics research purports to investigate instances of physical phenomena that occur in both on-world and off-world settings via laboratory experiments. How is the epistemology of this research supposed to work exactly? To get some purchase on this question, I am going to consider a particular laboratory astrophysics experiment in detail, so that we can investigate what is involved in practice, and how it is all supposed to hang together.

The particular case I am about to describe is philosophically valuable because the premise of the experiment—that we can study supernovae by shining lasers on plastic and foam in an Earthly laboratory—is, on its face, peculiar enough to teach us something interesting about what doing empirical astrophysical research is like in practice. This is what drew me to the case in the first place. But as I worked deeper into the details, I was surprised to find that the interpretation of the results of this particular research that the scientists offer does not quite go through. So ultimately, I will also argue that this case sheds light on epistemic challenges in laboratory astrophysics.

Research at the National Ignition Facility (NIF) at Lawrence Livermore National Laboratory predominantly focuses on laser confinement for fusion. At peak power, NIF focuses 192 laser beams on a small volume of material (“about the size of a pencil eraser”), delivering more than 2 million joules.[7] Studying matter in such high-energy-density states also has applications beyond the energy sector. When they are not trying to advance fusion technology, NIF researchers use this laser facility to study nucleosynthesis in stars and supernovae, instabilities in supernovae, opacity of stars, black hole accretion, nuclear reactions in stars, and planetary interiors—in short: astrophysics.[8]

For present purposes, I want to focus on a particular paper, Kuranz et al. (2018a), which published some of the NIF laboratory astrophysics results. In this paper, the

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[7] https://lasers.llnl.gov/about/what-is-nif

[8] https://lasers.llnl.gov/science/discovery-science

authors report results from a series of NIF experiments first designed in 2009 that aimed to study the Rayleigh-Taylor (RT) hydrodynamic instability, which occurs at the boundary between fluids of different densities, where the lower density fluid is somehow being pushed into the higher density one. At the interface between the fluids, characteristic finger-like shapes develop and then evolve mushroom-cap like tips that coil and expand. You may have seen something similar while pouring cream into coffee (if you had a clear cup). This instability is thought to occur in supernovae at the interface between the forward shock moving outwards into the relatively low density circumstellar medium around the exploding star, and the induced reverse shock in the relatively high density expanding stellar ejecta. The NIF researchers wanted to investigate the possible effects of high-energy fluxes on the structure of RT instabilities in supernovae. In particular, they were interested in whether or not material would be removed from the interface between the two shocks in cases where the instability is evolving under high-energy-flux conditions (Kuranz et al. 2018a, 2–3). Understanding the evolution of the remnants has implications for studying the timing of supernovae and, relatedly, supernova progenitors and the physical mechanisms that drive the explosions.

To study this phenomena in the laboratory, the NIF researchers use their powerful laser system to create such a shock in a test target: a little plug of plastic and foam. To do this, the NIF laser system is focused on a small holhraum (a cavity), which produces x-rays as it is energized by the lasers. These x-rays are then absorbed by the test target, producing a blast wave though relatively high density plastic into lower density foam. The experimenters report on two different conditions: a high flux case and a low flux case. By recording radiograph images of the test target material as it undergoes the blast wave, researchers can compare the structure of the instability as it evolves under the two conditions.

What they found was that in the high flux case, there were no mushroom caps on the characteristic finger-like shapes, and that the height of the region of mixed density was smaller than in the low flux case—in other words, the high flux conditions did seem to alter the shape of the instability. The researchers wanted to link this laboratory-generated data from x-ray blasted plastic and foam to astrophysical objects and processes and to draw conclusions about the evolution of the RT instability in high flux astrophysical conditions. In service of this aim, they consider a particular supernova (SN 1993J), where they suspect that the RT instability would have been subjected to high fluxes based on previous observations and modeling of that supernova (see Suzuki and Nomoto 1995; Fransson et al. 1996).[9] Various model parameters fitted to empirical data from this supernova suggest that in a dimensionless sense, i.e. comparing the relevant dimensionless numbers (more on this below), the energy fluxes present in the supernova would

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[9] Note that the initial motivations for focusing on SN 1993J had to do with the fact that the researchers expected the RT instability to be present since, based on previous observations and modeling, they expected the interface of the two shocks. That alone is not enough to establish the similarity arguments ultimately necessary to support their conclusion due to the open question about how high fluxes would affect the dynamics of matter at this interface.

have been larger than those in the laboratory experiment. That is, the energy fluxes due to heat conducted from the shocked circumstellar medium back into the shocked stellar ejecta are evidently larger with respect to the astrophysical system than the fluxes present at NIF with respect to the experimental system (see Kuranz et al. 2018a, Table 1).

Insofar as the structure of the RT instability was affected in the experimental setup, the experimenters reason that the structure in the case of the supernova should have been affected too. Indeed, they suggest that insofar as the "energy fluxes are larger, in a dimensionless sense, in the emergent [supernova remnant] than they are in the lab experiment" and the fluxes "have a noticeable effect in the lab experiment", then the astrophysical fluxes "seem likely to have a larger effect in the [supernova remnant]" (Kuranz et al. 2018b, 9). They conclude: "realistic models of [supernova remnants] must account for the effects of thermal conduction to accurately predict their evolution at epochs immediately following the shock breakout" (Kuranz et al. 2018a, 5).

How is it, exactly, that conclusions about supernova remnants are supposed to have been drawn from terrestrial experiments? The short, but as we will see, not quite satisfactory answer is that one can argue that the physics, the RT instabilities under the influence of high energy fluxes, is the very same in both cases, such that in experimenting on x-ray-blasted plastic and foam, researchers are probing the very same kind of physics playing out in far distant supernovae.

The sort of reasoning exhibited here is not uncommon, especially in hydrodynamics. It is a powerful and widespread practice in physics and engineering to draw inferences about the behavior of physically similar systems by establishing that certain similarity criteria are met in the systems of interest (Sterrett 2009). Even without knowing which particular form the physical equations characterizing some system should take, if one knows which physical quantities a phenomenon or behavior of interest depend upon, then via application of the principle of dimensional homogeneity, it can be possible to determine a set of dimensionless ratios that pick out a class of systems that will be physically similar with respect to that phenomenon or behavior (ibid., 816–817).[10] The Reynolds number is perhaps a familiar example of an informative dimensionless number. The Reynolds number expresses the ratio of inertial to viscous forces in fluid flow and can be expressed as the local flow speed multiplied by the characteristic linear dimension of the system of interest, divided by the kinematic viscosity of the fluid. Certain values of the Reynolds number correspond physically to the transition between laminar and turbulent flow in a system (considered 2300 for a circular pipe, for example). Thus, by calculating Reynolds numbers for appropriate systems, one can predict

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[10] The principle of dimensional homogeneity applies to dimensional equations. A dimensional equation is constructed by taking an equation relating physical quantities and replacing the symbol for the quantity with the associated dimension. When the dimensional equation is expressed using the dimensions of the basic quantities of a coherent system, dimensional homogeneity is achieved when the exponents of the dimensions of the basic quantities are the same on both sides of the equation (Sterrett 2009, 815–816).

if/when/where to expect turbulence, that is, a behavior of interest. This sort of reasoning to physical similarity based on dimensionless parameters is incredibly powerful where it can be achieved. The inferential payoffs do not come out of thin air of course, they are hard won via empirical knowledge, choices made in setting up the formalism, and finesse in characterizing the systems and phenomena of interest (ibid., 816). Moreover, in practice, researchers deploying similarity arguments via dimensional analysis rarely manage to (or aim to) capture physical similarity between systems of interest in *all* respects. The physical similarity established is circumscribed and often approximate (ibid., Section 6). Nevertheless, the fact that physical systems afford such similarity arguments at all may well constitute one of the most extraordinary epistemic resources of the physical sciences in comparison to the life and social sciences.[11]

In the National Ignition Facility experiment, the researchers attempt to deploy just such an argument from physical similarity via dimensionless parameters. Kuranz et al. (2018a) make use of a dimensionless parameter which they call the "Ryutov number" in their similarity argument, which can be interpreted as the ratio between pressure forces and inertial forces associated with a hydrodynamic system. We can trace the NIF researchers' use of this particular dimensionless number to their reference of a paper by Ryutov et al. (1999), titled "Similarity Criteria for the Laboratory Simulation of Supernova Hydrodynamics". Hydrodynamic systems well-described by the Euler equations can exist at vastly different scales. Ryutov et al. argue that as long as certain conditions they specify are met in the systems of interest—that is, viscosity and thermal conductivity are negligible, the energy density per unit volume of the fluid is proportional to pressure, dynamic influence of magnetic fields is absent, and the initial conditions are geometrically similar—the hydrodynamic behavior of the systems will be the same. Indeed, Ryutov et al. go so far as to state that if those similarity conditions are satisfied in an experimental and a natural system (i.e. in a laboratory and an astrophysical system), the two systems are *identical* with respect to their hydrodynamic physics (1999, 823). In particular, a laboratory system meeting these conditions should exhibit identical hydrodynamic behavior to an astrophysical system that also meets these conditions and has the same value of the dimensionless parameter mentioned above, which Kuranz et al. call the "Ryutov number" and which Ryutov et al. (and others) call the "Euler number." In attempt to avoid confusion, from now on, I will refer to this dimensionless number that is so crucial to the epistemology of the NIF experiments as the "Euler/Ryutov number".

Ryutov et al. caution that the similarity of the hydrodynamic behavior of systems can break down, however, when energy flow by particle heat conduction and/or energy flow by radiation flux are non-negligible: "The limit of applicability of this

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[11] On the history of the concept of physically similar systems, see Sterrett (2017b). On universality arguments, see the work of Robert Batterman, e.g. Batterman (2002). For an analysis of the relationship between universality arguments à la Batterman and analog black hole arguments see Field (2021). Sterrett (2017a) contains a discussion of analog black hole experiments in relation to similarity arguments.

similarity is set by the validity of Euler's equations as an adequate description of the hydrodynamics" (1999, 826).

With this introduction to the experiment, let us attempt to unpack its epistemology. I take it that the NIF research I have just described may be readily identified as "experimental" without generating too much controversy. While I do not know of any characterization of what it means to be an "experiment" or "experimental" with any specificity that has been widely adopted in philosophy of science, experiments are often associated with a cluster of features that obtain in the NIF research. It is, after all, research in which the scientists prepare special conditions in their laboratory apparatus to test the outcome of varied conditions, which they manipulate themselves (in this case, by generating high and low flux conditions using the NIF laser system). However, noting this experimental character of the research does rather little to illuminate its epistemology. Is this laboratory research on plastic and foam (experiment or not) informative with respect to astrophysics? If so, how is that supposed to work? In the following section, I will argue that we make more headway in explicating and evaluating the epistemology of this empirical research if we attend instead to how the empirical data are produced and what the research target is supposed to be. To do this, it will be helpful to first have a view of what makes data empirical in general.

2.4 Attend to "Empirical" Not "Experimental"

Elsewhere I have argued that data, including astrophysical data, are *empirical* with respect to some target when there is an interpretation of the provenance of those data using the resources of an epistemic context, such that the data are products of causal interaction with that target (Boyd 2018). By 'epistemic context', I mean the collection of conceptual, theoretical and representational resources from the perspective of which the data is to be interpreted. It is important to note that data are empirical *relative to a target*. Without specifying a target it is impossible to say whether some particular dataset is empirical or not. Data are also empirical *relative to an epistemic context* and the epistemic context supplies the resources with which the data are interpreted. Data never speak for themselves, but rather always require interpretive resources. In particular, data require background theory to furnish a causal story connecting the worldly target of interest to the data collection and recording process.

An important feature of the view of what makes data empirical that I am defending is the causal production of data. As I said, to be properly empirical, data should have been produced by causal processes that connect the worldly target of research to the process of data collection and recording from the perspective of the epistemic context in which the data are to be interpreted. There is no perspective outside of an epistemic context from which the causal processes can be identified and traced. Indeed, there is no perspective outside of an epistemic context from which a worldly target can be identified in the first place. Yet, using the resources of an epistemic context, it can be possible to answer the question: were these data produced by causal interaction with the target?

Taking this view of what makes data empirical onboard, let us return to the National Ignition Facility Rayleigh-Taylor hydrodynamic instability experiment. How should we construe the data generated by the NIF experiment—are they empirical *and* astrophysical data? Following my view we should ask, first: what is the worldly target of the National Ignition Facility research and second: by causal interaction with what has the data been produced from the perspective of the relevant epistemic context?

Here are a few possible answers. First, we might say that the worldly target is SN 1993J, the particular supernova that NIF researchers highlighted as possibly displaying the RT instability under high flux conditions, yet the causal interaction producing the experimental data is with NIF plastic and foam targets, thereby ruining the *empirical* nature of the data with respect to the astrophysical target. The laboratory data was not produced by causal interaction with SN 1993J. Or we could say that NIF plastic and foam targets are the worldly targets of research too, but that would seem to ruin the *astrophysical* nature of the data.

To recover a sense in which this experiment produces empirical astrophysical data, we could construe the worldly target as the general class of RT hydrodynamic instabilities in high-energy-density states of matter. Then *insofar as laboratory systems and far removed astrophysical ones instantiate this very same physics*, investigating the effect of high energy fluxes on hydrodynamic instabilities in laboratory plastic and foam is just to investigate the very physics playing out in astrophysical contexts. NIF data are empirical with respect to high-energy-density states of matter and their behavior since there is an interpretation of the provenance of those data such that they are the products of the causal interaction of the matter energized and confined by the NIF lasers with the laboratory detectors systems. *Insofar* as such high-energy-density states are instantiated in faraway astrophysical systems also, the data gathered in NIF experiments can be used to constrain astrophysical theorizing. So, we should like to know, what is the justification for thinking that the same physics in instantiated in both contexts? As I have already alluded, the NIF researchers appeal to the hydrodynamic similarity of the two contexts.

At first glance, the move suggested in the previous paragraph might seem like slight-of-hand by mere redescription. Can it really make a difference to the epistemology of the research whether we think of the target as a plastic and foam target or as an instance of a class of physical systems? The answer is 'yes', but of course it is not the mere redescription that is doing all the heavy lifting, but rather the arguments and evidence in the background that justify treating the systems of interest as belonging to the relevant class.[12] In this case, the heavy lifting is

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[12] I use the words "class", "kind" and "type" interchangeably, hoping any more metaphysically inclined readers will forgive me.

done by the justification of the relevant similarity criteria and the evidence for their applicability to the systems of interest.

The general epistemology of science issue here is that of external validity, which is both ubiquitous and absolutely crucial for the epistemology of empirical science (see e.g. Morgan 2003, Currie and Levy 2019, Leonelli and Tempini 2020, and Evans and Thébault 2020). 'Externally valid' experimental results are those that are valid outside of the local laboratory conditions (see e.g. Guala 2003, 1198). In much empirical research, arguing for external validity involves addressing features or conditions of the proximal and ultimate research target that could plausibly make a relevant difference. No two targets or experiments are identical in all respects. In practice, scientists must concern themselves with discerning (to the best of their abilities) features and conditions that might make a *relevant* difference, and then either providing arguments that the differences may be ignored for the limited purposes at hand, concocting circumstances so that the differences become negligible, or else modifying the way that they conceive of the scope of their research so as to responsibly accommodate those differences.

Without characterizing research targets as belonging to a type, we would be locked into an insufferably parochial epistemology, or, to borrow a delightful phrase that Alison Wylie cites from Bruno Latour: we want to avoid "tragically local" data (Wylie 2020, 285). Avoiding tragically local data can involve strategic characterization of the research target, and good arguments (backed by good auxiliary evidence) to support that characterization. I have highlighted a certain kind of similarity argument that can be made for some hydrodynamic systems. There are, of course, other types of arguments that can be made in other contexts. External validity claims need to be justified, they may be challenged by those clever enough to come up with physically plausible difference-makers that have not yet been taken into account, or by surprising empirical results. In general, scientists do well to take opportunities to empirically check their external validity arguments, and indeed to rigorously seek out such opportunities. This is an important part of what is involved in arguing for the epistemic significance of a result and, in particular, of what is involved in eliminating candidate sources of error, confusion, and alternative explanations.

I like to think about the work that goes into making good external validity arguments in terms of what I have elsewhere called "enriched lines of evidence" (Boyd 2018). The idea is that the epistemic utility of an empirical result depends on the details of its provenance. To use an empirical result in an epistemically responsible way, one has to know quite a bit about what assumptions have been baked into it. Some of those assumptions will cause epistemic problems for certain applications, for instance, in constraining a particular hypothesis or in attempting to combine the result with others that were generated by incorporating different assumptions. On this view, supplying good arguments for external validity involves arguing that the assumptions baked into an empirical result will not cause epistemic problems for the intended application. As it turns out, just such a problem seems to manifest in the NIF experiments with which we have been concerned here. If the argument from hydrodynamic similarity that links the laboratory and astrophysical systems as belonging to the same type requires that the energy flux from heat conduction in the systems be negligible, but the experiment in question is designed precisely to investigate how high energy flux from heat conduction influences the structure and evolution of the hydrodynamic instability, does the very aim of the experiment undercut the specified connection to astrophysical targets like SN 1993J? To address this worry, we need to know a bit more specifically what the criteria regarding heat conduction and radiation flux that would need to be met are, and then to check whether in fact those criteria are fulfilled in this context.

Following Ryutov et al. (1999), the source of the similarity criteria that the NIF researchers invoke, the criterion regarding heat conduction is that convective transport needs to dominate conduction in the systems of interest (828). Regarding radiation flux, convective transport ought to dominate the radiation contribution to thermal diffusivity, or, they explain, in cases where it is inconvenient to determine this due to difficulties evaluating the mean free path of photons, it is sufficient to show that the lower limit of the radiation cooling time is much larger than the characteristic hydrodynamic time (825). So, armed with these details, we can ask: do these conditions indeed obtain in the systems of interest in the NIF experiment and its astrophysical counterpart?

It seems these conditions are in fact *not* met. As the NIF researchers explain, for the supernova:

> The interface between the shocked ejecta and the shocked [circumstellar medium] thus arises hydrodynamically, and the transition across it will initially occur in a few ion-ion mean-free-paths [...] Because pressure is continuous across such an interface, the temperature is much higher in the shocked, less dense CSM than in the denser ejecta. This leads to the possibility of radiative or conductive transport of energy into the denser ejecta, which in turn can affect the evolution of the Rayleigh-Taylor (RT) instability at the interface by ablating material from it. In addition, there is a phase when radiation from matter heated by the reverse shock also might affect the RT. (Kuranz et al. 2018b, 3–4)

In particular, they state that "pure hydrodynamics may well be insufficient to accurately predict the structure of the young [supernova remnant]" and that, regarding heat conduction: "As the shock structure at the interface between the [circumstellar medium] and the dense ejecta forms, heat flow is possible by radiation and by electron heat conduction [...] the radiative losses from the shocked layer produced by the reverse shock are found to be large enough to cool it significantly... [the energy flux associated with the cooling is much greater than the mechanical-energy flux] for a period of time, showing that the hydrodynamic model is not sufficient to accurately describe the behavior" (ibid., 5–6). Indeed, the authors state that "the incoming energy flux by heat conduction is larger than the incoming mechanical energy flux, by a factor of 1,000 at all times" and reason that this flux by heat conduction "is large enough that that this energy flux may be a dominant effect in establishing the structure of the layer" (ibid., 6).

We can see the failure to meet the necessary similarity conditions fairly directly by attending to the dimensionless parameters characterizing the laboratory and astrophysical systems (Kuranz et al. 2018b, Supplementary Table 2). For instance, consider the energy flux ratio R, which has the radiative energy flux in the numerator and the mechanical-energy flux in the denominator. For the similarity condition to be met, the denominator would have to swamp the numerator, yet R for the supernova is 10^3. Note also that the Euler/Ryutov numbers for the two systems are not the same: for SN 1993J at 0.1 years the Euler/Ryutov number is 4 and for the NIF experiment it is 5. On this latter point, the researchers explain that while the relevant sense of physical similarity between systems does not require the precise identity of all dimensionless parameters, Ryutov's et al.'s argument does require that the Euler/Ryutov number be the same for the systems whose similarity is to be established. In this case, the authors note that the Euler/Ryutov numbers for these two systems differ according to best estimates, but that the assumptions upon which those estimates have been made "could vary by at least an order of magnitude" (Kuranz et al. 2018b, 9). In response to these circumstances, they clarify in a supplementary note to the primary publication that the main goal of the work is not to scale their laboratory results to a specific astrophysical object, such as SN 1993J, but "rather to show the importance of energy fluxes in the evolution of young supernova remnants" more generally (Kuranz et al. 2018b, 9).

Here is the argument that I think these authors would need to make regarding the astrophysical relevance of these NIF experiments. Drawing on Ryutov et al., they suppose that in general, hydrodynamic systems that meet Ryutov et al.'s similarity criteria and have the same Euler/Ryutov number will exhibit the same hydrodynamic phenomena. Therefore, in particular, if the laboratory system and astrophysical systems of interest meet Ryutov et al.'s similarity criteria and have the same Euler/Ryutov number, then they will exhibit the same hydrodynamic phenomena. It then needs to be established that the laboratory system and the astrophysical systems of interest in fact meet the criteria and have the same Euler/Ryutov number. Then, supposing the NIF experiments show that the laboratory system displays a particular hydrodynamic phenomenon, the astrophysical systems can be expected to display that same phenomenon too.

The problem is that the similarity argument evidently fails in this instance since it is not the case that the laboratory system and the astrophysical ones meet the criteria and share the same Euler/Ryutov number. Instead, neither the laboratory system nor the astrophysical systems of interest meet the criteria necessary for the similarity argument to go through (and, at least for the particular supernova remnant the researchers considered) these systems have different Euler/Ryutov numbers. Therefore, while the experiments demonstrate that high flux conditions do influence the structure of the RT instability in the laboratory conditions, the argument for astrophysical relevance seems incomplete.

What *can* be gleaned from this case, what did we learn from the results? It seems clear that modeling these systems as evolving hydrodynamically is not appropriate, and therefore we do not have good reasons to suspect that the usual-shaped mushroom-cap RT fingers will show up in supernova remnants with high-energy flux conditions. Expecting such structures in supernovae remnants was premised on hydrodynamic modeling, but we have seen that the high-flux conditions make such modeling inappropriate. But this much might have already been clear before the NIF experiments on plastic and foam were ever performed. That is, reason to doubt the applicability of the usual hydrodynamic evolution of the RT instability in supernova remnants where high-energy flux conditions are present, could have been gleaned already from the conditions and argument set out in Ryutov et al. (1999). Combining these arguments with empirical data from SN 1993J supporting the presence of high-energy fluxes in that system, would have already been enough to cast doubt on the evolution of the structure of the RT fingers in the resulting supernova remnant. If the final epistemic payoff of the NIF experiment is supposed to be the relatively modest point that caution is warranted regarding predictions and interpretations of RT-like structures in supernova remnants in the presence of high-energy fluxes, then it is not clear that the experiment on plastic and foam adds anything new. One might consider the epistemic payoff as rather the demonstration that ablation of material from the RT fingers occurs in the experimental conditions. That is all well and good, but again, that result in itself does not speak to the astrophysical systems. Nevertheless, in some passages the NIF researchers seem to advance an epistemic payoff that goes beyond the modest point. They seem to argue that since the conditions that are responsible for the demonstrated phenomenon of interest in the laboratory setting are, in a dimensionless sense, larger in the astrophysical system, that ought to give us reason to expect the phenomenon in the astrophysical system as well—and that the effect would be larger in the astrophysical system (Kuranz et al. 2018b, 9). Unfortunately, the success of that argument depends on the soundness of the similarity argument, which in this case has evidently failed.

Let us take stock. I have suggested that properly empirical data must derive from a causal chain that has one end anchored in the worldly target of interest. In astrophysics, that does not necessarily mean that the target has to be outside of the terrestrial laboratory. Powerful similarity arguments are available to cast some laboratory targets and far removed astrophysical ones as instantiating identical physics. Insofar as phenomena that are identical with respect to the relevant physics can occur in laboratory conditions, then physics that occurs in astrophysical systems as well as laboratory ones can be studied on Earth. So laboratory astrophysics teaches us about astrophysics, by teaching us about kinds of physical phenomena that occur in laboratories and in astrophysical systems. Laboratory astrophysics teaches us astrophysics, by teaching us physics. Noticing this illuminates the continuity between empirical research in astrophysics and empirical research in other branches of science. Scientists conducting empirical research often need good external validity arguments to avoid producing tragically local data. This is certainly true in laboratory astrophysics. However, the unsurprising need for such good external validity arguments does not entail that astrophysics lacks experiments, is characteristically observational, or does not (sometimes) occur in terrestrial laboratories.

The powerful physical similarity arguments leveraged in laboratory astrophysics research can break down when the necessary conditions do not obtain. If the relevant conditions in the laboratory and astrophysical targets are not the same, then the crucial epistemic link will be broken. In such cases, other arguments would have to be furnished in order to justify couching data derived from terrestrial targets as both empirical and bearing on astrophysics. If such arguments cannot be made, then the research may still be construed as yielding empirical data, just not as bearing on astrophysics. Of course, even then, that does not mean that astrophysicists could not learn about their astrophysical targets using other methods.

2.5 Lessons for Epistemology of Astrophysics

As philosophers, our approaches to the epistemology of science can be more or less fruitful. Philosophers of science working in a normative mode sometimes deploy what we take to be informative distinctions to guide our inquiry. The most obvious example is Popper's falsifiability criterion for demarcating science from pseudoscience (1959). Methodologically, the falsifiability criterion tells us what the salient feature of a case is going to be. Approaching a case with the falsifiability criterion in mind tells us to pay close attention to whether or not it is met in that case, and to draw the associated normative judgements about it. A distinction such as falsifiable/unfalsifiable can thus structure how we approach the work of normative epistemology of science in practice by guiding our attention to certain features as salient for the epistemology of science.

Whatever you think about the utility of the falsifiable/unfalsifiable distinction in particular, there are other distinctions that play this sort of role in guiding attention in epistemology of science. A historically influential one has been the distinction between theory and observation (Boyd and Bogen 2021). Thinking within a traditional empiricist framework, in which pure observation was theory-free and thus suitable for confirming or disconfirming predictions from theory, philosophers of science would approach cases with questions such as: "Is this observation theory-laden in a way that would prevent its effective use for theory testing?" The theory/observation distinction generates an investigative framework in which the question of theory-ladenness becomes especially salient in the investigation of science in practice. From the perspective of twenty-first century philosophy of science, this distinction looks like a red herring. Philosophers of science largely agree that there is no empirical data that is totally theory-free, and furthermore that theory-laden empirical results can be perfectly useful for constraining theorizing. Indeed, it is *in virtue of* being imbued with theory that results can do the work of constraining theorizing (Boyd 2018). Some ways that theory can be integrated into empirical results do cause epistemic problems, but the interesting question is not whether the results are laden at all.

Similarly, normative epistemology of science in practice that approaches cases with the observation/experiment distinction in mind thereby operates within a framework that emphasizes the presence or absence of physical manipulation of the research target as especially salient for the epistemology of science. Is this a fruitful approach? Suppose we had approached the National Ignition Facility laboratory astrophysics research with the question "Is it an experiment?" in mind. Such an approach may have been natural from the perspective of a framework that prioritizes experiments in the epistemology of science. If, as we saw Hacking put it, natural science *is* experimental science, then the primary thing (or at least a very dominant thing) we want to know in investigating the epistemology of some science in practice is whether or not it rises to the standard of experimental science. On this approach, when we learn that a research project is observational, we learn that it is not experimental, and thus that the research is in this aspect, epistemically impoverished.

However, we are now in a position to clearly see just how unilluminating it would have been to ask if the NIF laboratory astrophysics research is experimental or involved experiments. As it happens, it did. And perhaps for those who thought astrophysics was characteristically observational, this (or indeed the existences of laboratory astrophysics research at all) would have been a surprise. But noting that the NIF research makes use of experiments does not, by itself, imply anything epistemically interesting. This is because the observation/experiment distinction does not make an epistemic difference in general. Like the theory/observation distinction, the observation/experiment distinction is largely a red herring for the epistemology of science (Boyd and Matthiessen manuscript). It sets philosophers of science up to attend to certain features of their cases as salient, but those features distract from the locus of epistemic action.

Nevertheless, there is something interesting going on in the NIF case for epistemologists of science. When we shift to a framework that foregrounds empirical data, we approach cases with questions such as “What is the worldly target?” and “By causal interaction with what had the data been produced from the perspective of the relevant epistemic context?” We saw that laboratory targets can instantiate the same physics as astrophysical targets, under the right description and with the right arguments. In the NIF case in particular, we encountered an instance in which it may have first seemed that powerful similarity arguments could be made to justify understanding the Rayleigh-Taylor instability in laboratory plastic and foam and in distant supernovae as instantiating the same physics. However, it turned out in the particular experiment that was the subject of my case study, that the aims of the experiment (namely, to study the evolution of this instability under high energy flux conditions) undermined the needed similarity argument. Notice how our choices in framing which features of scientific research count as salient for the epistemology of science serve to obscure or illuminate the site of epistemic action (or problems in the research). Thinking that astrophysics is an observational science, we might have simply dismissed the NIF research as not astrophysics, and missed the role of the powerful similarity arguments that can sometimes link together certain physical systems under one description. But once we pay attention to what the research target is and how the data are produced, we see that, as it happens, the needed powerful similarity argument does not go through in this case. One could easily miss what has gone wrong in the epistemology of this research insofar as one focuses on it as an experiment, or as purportedly an experiment relevant to a physically distant system. Instead, by tracking the aims of the research and the production of empirical data in it, we were able to notice how the conditions of the experiment affected the application of a crucial similarity argument.

I suggest that these lessons motivate a methodological shift in the epistemology of science in practice, the need for which we can see with particular clarity in the epistemology of astrophysics. Rather than attending to the presence or absence of experiments to investigate the epistemology of science, we ought to instead attend to the target of the research and the processes that produce the empirical data. Whereas attending to the former unhelpfully obscures the epistemology of astrophysics, and the epistemology of laboratory astrophysics in particular, attending to the latter is more helpful for highlighting features salient to the epistemology of science, both within astrophysics and in empirical science more broadly.

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