Otávio Bueno* and **Steven French.**** ***Applying Mathematics: Immersion, Inference, Interpretation.*** Oxford University Press, 2018. ISBN: 9780198815044 (hbk). Pp. xvii + 257.

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Otávio Bueno and Steven French have produced a readable, informative, and compelling account of mathematics as it is deployed in physical theory. The book is built on their previous work on the partial structures framework, mathematical application, and philosophy of (quantum) physics.

It offers a compelling case for partial structures as a framework for understanding how mathematics is applied in physical theory. Further, it seeks to find a "middle way" between seeing the "effectiveness of mathematics" as unreasonably mysterious or unreasonably inevitable. By bringing insights based on Michael Redhead's notion of "surplus structure" to the partial structures approach—along with historically attentive work in philosophy of quantum physics—the authors make the case for their middle way. Finally it fortifies the case against the efficacy of indispensability arguments, both traditional and enhanced.

Chapter 1, entitled "Just How Unreasonable is the Effectiveness of Mathematics", sets the stage for their middle way by presenting and critiquing Mark Steiner's [1998] influential version of Wigner's question. Steiner takes a Pythagorean view of how significant portions of mathematics function in physical theory, namely, that scientists employ formal analogies between physically interpreted portions of mathematics and distinct pure (not interpreted physically) mathematics to "guess" at the laws of nature. And of course, understood as bare guessing at laws based on formal analogies (and other relationships) with in pure mathematics, the "methodology" does seem to have been mysteriously effective. In a clear and historically informed few pages (3–9), the authors neatly show how Steiner's case, in the context of Schrödinger's wave equation, depends crucially on conflating the context of discovery with subsequent efforts to interpret the theory, and also on an historically unsustainable reconstruction of the discovery. The next two sections, entitled "Mathematical Optimism" and "Mathematical Opportunism," first detail problems with an opposite extreme that expects any physical phenomena to be straight forwardly isomorphic to a mathematical structure, and

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then endorse a more historically sensitive, case-specific opportunistic approach that recognizes the intermediary role of idealization and other physical representations of the phenomena to the application of mathematics.

Chapters 2 and 3 present Bueno's and French's partial structures account of application, and Chapters 4–7 concern how the account can be put to work in the analysis of the development of aspects of quantum theory.

The authors motivate and present their partial structures account on the tails of the notion of *surplus structure* [Redhead, 1975], the idea that when connecting a (pure) mathematical theory/structure to a physical system/model, only a portion of the pure theory/structure is explicitly understood to correspond to an aspect of the physical system/model. An important feature of the backdrop of the partial structures account is that it *begins* with an "already mathematized" physical domain whose (mathematical?) structure is mapped to a mathematical theory/structure (44). But importantly, "only *some* structure is brought from mathematics to physics; in particular, those relations that help us find counterparts, at the empirical set-up, of relations that hold at the mathematical domain." In the mathematical domain "empirical problems can be better represented and examined" (45). Typically not all of the information about which aspects of the physical structure can be mapped to the mathematical are available, and more information may accrue as the empirical theory evolves. The "partial" aspect of the account helps capture this incompleteness and, crucially, how it evolves through scientific progress. A partial relationship, say R over domain of interest D, is understood as an ordered triple $\langle R_1, R_2, R_3 \rangle$ in which R_1, R_2, and R_3 are exhaustive and mutually disjoint sets over D, where R_1 is the set for which R is known to hold, R_2 is the set for which R is known to *not* hold, and R_3 is the set for which it is not known *whether* R holds.[1] Thus R_1 and R_2 represent what is currently known about how the mathematical structure aligns with the physical, and R_3 is left open, aligning with the "surplus structure" and capturing the open and evolving aspect of scientific theory.

Next—and this is really the payoff for the approach with respect to application—we see that these partial structures and morphisms help "establish *inferential relations* between empirical phenomena and mathematical structures" (52), which is the lynchpin in their general account of the applicability of mathematics:

[1] In order to facilitate comparing partial structures, the authors further introduce partial isomorphisms and partial homomorphisms, with the latter being especially important because it does not require the structures to be of the same cardinality, which of course is often not the case between physical structures and mathematical ones (43–44).

> It is by embedding certain features of the empirical world into a mathematical structure that it becomes possible to obtain inferences that would otherwise be extraordinarily hard (if not impossible) to obtain (51).

This encapsulates their three-stage inferential conception of mathematical application (IDI):

Immersion establishes a mapping (generally partial) from the physical system (model) to an appropriate mathematical structure.

Derivation draws consequences (makes inferences) within the mathematical formalism.

Interpretation maps the formal consequences back onto physical system (model)—the interpretive mapping involved may or may not be the inverse of the one involved in the Immersion step.

At this point (the end of Ch. 2), the authors have established their middle way between mystery mongering and undue optimism and their account of how mathematics is applied to a physical system/model *already understood to be mathematized to some degree*. That is, everything in the account so far has been predicated upon already having a mathematized physical structure or model—more generally, a mathematized or readily mathematizable representation of the physical phenomena of interest. Chapter 3 is thus an effort to defend their general account representation in science, which again is a formal one involving various morphisms and structures, as described above. The chapter is a helpful and plausible defense of their formal account of scientific representation, especially for those of us without firm convictions on the issue(s).

We are now to what I found the most interesting and informative parts of the book, the four chapters carefully engaging the development of quantum theory with an eye toward their account of application. And while this may be partly due to my particular position as a reader—someone trained in the mathematics but not so much the physics, and especially not the history of the physics—these four chapters have standalone value for all scholars thinking about the application of mathematics. These chapters take the reader into the actual historical details of application, rather than dealing with typical toy examples. And importantly, the scientific work can be seen to be developing concurrently with the development of the mathematical theory. It is precisely these actual instances of application that must be considered in theorizing mathematical application. I will not try to fully summarize the chapters here, instead I will narrate briefly their general topics.

The authors describe Chapters 4–7 as a series of case studies illustrating how application crucially involves "constructing the special circumstances" via idealizations that allow physical theory to be embedded in mathematical theory. Chapter 4 looks at the introduction of group theory into quantum mechanics in the later 1920s and early 1930s and employs the (differing) details of the Weyl and Wigner programs to argue that attention to the idealizing work on the physics side demystifies Steiner's account of application. Chapter 5 is concerned with the explanation of the superfluid behavior of liquid helium in terms of Bose-Einstein statistics (by London); it vividly illustrates the Immersion step of their account as a "bottom-up" way of "preparing" the physical phenomena for the more "top-down" step of applying the mathematical theory to it. In Chapter 6 we get an exceptionally clear account of the development of von Neumann's algebras (of operators) as a supplement to the Hilbert space formulation of quantum mechanics—important lessons from the episode include the theoretical unification of quantum states, probability, and logic, and also the further inter-relationships between pure mathematical structures (Hilbert space, functional spaces in general, and once again, group theory, but in a distinct way from Chapter 4).

Finally, Chapter 7 is a study of Dirac's delta function and its well known problematic role in quantum mechanics—in particular, establishing the equivalence of matrix and wave mechanics, but also its (putative) role in the discovery of the positron. In a turn that anticipates the remainder of the book, part of the chapter concerns whether and in what sense the delta function was *indispensable* in these two roles. The authors present a careful case that it was not indispensable in either case; by focusing on the Interpretation (of the inferred mathematical surplus structure) stage, they neatly argue that—in part by paying careful attention to the details of Dirac's reasoning—it was not formalistic consideration of the surplus math that led him to posit the positron, but rather physical considerations and insight that eventually allowed him to come up with a physical interpretation of the surplus mathematical structure (negative energy solutions). Indeed, "the surplus structure in and of itself has no heuristic force—it is only when there are the relevant grounds, or reasons, in play that the structure has significance (144)," and instead "the crucial work was done by *interpreting* the mathematical formalism (without a physical interpretation, no empirical predictions could ever be obtained from the latter)" (147).

The eighth chapter continues with indispensability issues, which have of course been at the center of cases for mathematical Platonism since Quine. The first part addresses the traditional indispensability argument (IA): that the indispensable use of mathematics in physical theory commits us to the reality of the mathematical objects involved as well as the physical. The authors show how the Dirac delta function case undercuts the immediate (and perhaps ultimate) plausibility of IAs; they rehearse the more sweeping problems with it and move on to the focus of the current debate, the "Enhanced Indispensability Argument" (EIA), which focuses on examples of mathematical *explanation* playing an indispensable role in science. The discussion of the IA is a helpful pulling together of consensus. The extended discussion of the EIA is an important furthering of the debate, in my opinion. In particular, by carefully distinguishing between the representing/indexing role of math and its (putative) explanatory role and by returning consideration to the actual (vexing) examples like the role of symmetry and the permutation group from Chapter 4, we are led to see that there are at least six ways in which one can understand the explanatory work done by invoking the permutation group as grounded in the physical world (causal or structural), and need not "open any door to mathematical structure in the role of explanans" (173). The final eight pages consider the warrant for countenancing physical entities with "hybrid" properties (having both abstract mathematical and physical features), e.g., quantum spin. The authors' case against hybridity involves a careful consideration of the possible motivations for this from current physical theory (found lacking) and also the prospects for actually articulating a metaphysical account of what it would mean to be both mathematical and physical—pointing out that while not impossible to construe, there are alternatives to hybridity (e.g., structural realism) that seem to do the job without incurring the metaphysical costs.

In the final substantive chapter, Chapter 9, the authors undertake a defense of their partial structures account against the claim that it cannot accommodate explanatorily ineliminable mathematical operations (due largely to Robert Batterman). The idea is that the partial structures account, with its focus on mappings between physical and mathematical structures, is unable to capture important applied mathematics examples in which some kind of mathematical limiting operation plays the main role. That is, operations that do not require one to "associate a mathematical entity or its properties with some physical structure had by the system of interest" [Batterman, 2010, 4]. And that in fact, "in some important examples of explanations in physics, certain mathematical operations feature *ineliminably* in the *explanans*" (193), and thereby resist being folded into the partial structures IDI account.

Bueno and French respond in two directions: they argue (1) that their account can and does accommodate mathematical operations, and further that (2) the onus is on the critic who claims that such mathematical operations explain the physical phenomenon to provide an account of *how* they explain, and that no plausible account has been offered or appears to be forthcoming. In the end (after much helpful discussion), they boil down their disagreement with Batterman to the following.

> The core of our disagreement with Batterman now becomes clear. For us, the relevant [asymptotic limiting] techniques sketched above involve significant surplus structure. For Batterman, the claim that they provide understanding and explanation means that the structure cannot be surplus. (206)

They spend Section 9.9 outlining four "basic requirements" that explanations in general satisfy, and argue that Batterman's asymptotic explanation proposal appears not to satisfy them, and thus need not be seen as a novel kind of mathematical explanation that proves intransigent to their account. Section 9.10 is a critique of an account (from Batterman and Rice) of explanation that is more supportive of asymptotic explanation.[2]

And now for a few critical remarks.

First, I wonder if the authors are not courting a kind of undue optimism of their own. They write that their model theoretic version "effectively blurs the pure/applied distinction" (158), but this distinction is important because, depending on the stage of the development of the mathematics and the science, a theory may be best thought of as either an application of a pure theory, or simply as mathematized physical theory. To capture this, a distinction between *pure* and *applied* mathematical theories is needed [Peressini, 1997, 1999, 2003, 2008]. The authors have addressed the distinction as developed in Peressini [1997] in various places, resisting it because it presumes that the mathematics and physics are already distinguished (158–159), a presumption which is of course highly contentious, even doubtful, given modern physical theory.[3] But this is wrong,

[2] The last chapter, Chapter 10, is called a conclusion, but actually has an important discussion of the discovery of the Ω^- particle by Gell-Mann and Ne'eman.

[3] Bueno [2003, 31] puts the concern more explicitly:

> "Peressini's account assumes that the mathematical and the physical theories are already distinguished. On this account, we apply group theory to quantum mechanics by assigning group-theoretic notions to quantum mechanical principles.

though their reading is not unreasonable; there is more detail in the later papers (especially Peressini [2008]) that is important here. There the term "pure mathematical theory" is employed to refer to the theories that occupy mathematicians and whose (apparent) subject matter is a mathematical object/structure (e.g., number theory, real analysis, functional analysis, group theory, set theory, etc.), distinguishing them from "scientific theories which, to varying degrees, make *use* of pure mathematical theories, that is, mathematical (or mathematized) scientific theories." Importantly, "the distinction between pure mathematical theories and mathematical scientific theories is underwritten by the latter's deployment of a physical interpretation of *part of* the mathematical vocabulary that mathematical theories lack" [Peressini, 2008, 105]. This is close to Bueno's and French's development of surplus structure and the very important and complicated role of the physical interpretation steps. An additional section of Peressini [2008, Sec. 2.1] discusses the pure/applied relationship itself. There it is argued that "neither the pure theory nor the applied theory is in all cases epistemically prior" (106); the claim is supported by considering several historical episodes in the history of mathematics and of science. The pure/applied distinction is then refined as follows:

> Until now, I have been taking "applied mathematical theories" and "mathematized scientific theories" to be the same thing; however, they must be distinguished. Because not every mathematized scientific theory is also an application of a (pure) mathematical theory. There are mathematized scientific theories that do not bear the "applied" relationship to any pure mathematical theory, and so, strictly speaking, should not be considered applied mathematical theories. In these examples a scientist develops a technique in order to solve certain physical problems by what appear to be mathematical methods (e.g. evaluating a certain type of integral, multiplying an integrand by a certain "function," dividing by a certain mysteriously small quantity, etc.). But in fact the new "mathematical" method makes no sense mathematically, and hence is not a physically interpreted version of a pure theory [Peressini, 2008, 106–107].[4]

> The two domains (of group theory and quantum mechanics) have already to be clearly separated for this proposal to get off the ground."

[4]Examples given there are Newton's fluxions/differential calculus, Dirac's delta function, and Feynman's path integrals. Peressini [2003, 221–222] also contains a similar argument in the

So articulated, it does not require that the mathematical and the physical theories be “already clearly separated.” It requires only that the pure mathematical theory (*if and when there is one*) is distinguishable from the mathematized scientific theory. Surely this is not problematic. The theory of groups offered in an abstract algebra text is distinct from the fragments of group theory present in quantum mechanics: the former lacks the very physical interpretation that the authors make such compelling use of in resisting IA and EIA theorists.[5]

The potential “undue optimism” I am leading up to is the view that for any mathematized scientific theory, there corresponds a pure mathematical theory of which it is an application. Dirac’s formal operations with his delta function embedded within a quantum mechanics theory was a mathematized scientific theory, and *as it turned out*, a pure theory was developed (Schwartz’s distribution theory) that could render *a different version* of the Dirac quantum account an applied mathematical theory. But it is not obvious that *any* such mathematized scientific theory can be seen as an applied pure theory, that is, there is no reason to think that for any scientifically embedded formal operations there is a pure mathematical theory to be had for which it is an application. But the authors’ model theoretic account seems to run contrary to this point in that on it, what is happening is that the physical QM model/structure is embedded in a mathematical structure involving vector spaces of complex integrable functions and linear operators but that also include the delta function. Is this structure a pure mathematical theory? It may be as close as we get on their account, but this (notoriously) is not a legitimate mathematical theory—there is no such function in the context of the mathematical theories employed.[6] It would seem then that any such set of formal techniques is going to end up being a pure mathematical theory. Or at the very least, the possibility of asking the question is eliminated, since again, the authors’ account “effectively blurs the pure/applied distinction (158).”[7]

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context of confirmational holism and IAs.

[5]Back then I unfortunately also used the notion of “empirical bridge principles” in addition to “physical interpretation,” which I believe helped engender such misunderstanding.

[6]Dirac was in need of a *particular* linear functional, F, that mapped a given function to its value at zero, but he was also constrained because in the context of his account he needed F to be expressible as an integral, i.e., $F(f) = \int_{-\infty}^{+\infty} f(x)\delta(x)dx = f(0)$ for some $\delta(x)$, and of course it turns out that there is no such $\delta(x)$, though there is such a functional to be had in more the generalized setting of distributions or by generalizing from Riemann integrals to integrals defined over different measures, etc.

[7]Perhaps the flip side of such optimism would be individuating mathematical theories so loosely (or blurrily) that *any* formal structure that “embeds” (in the authors’ sense) the mathe-

Interestingly, Bueno and French (136–137) have a very nice discussion of whether the delta function is ultimately a distribution, maintaining that it is not. They speculate that their "framework of partial homomorphisms could quite naturally capture both the open-ended nature of Dirac's theory and the manner in which it can be related to Schwartz's." I am not as optimistic. For as Jesper Lützen [1982, 164] carefully argues in his philosophically fascinating "pre-history" of distributions:

> The theory of distributions is *not* structural mathematics in this sense. To be sure the theory of distributions is built on the highly axiomatized mathematical field of functional analysis, but it is not axiomatic in its construction. It unites different mathematical methods and theories not by imbedding them in an axiomatic structure in which the nature of the elements is irrelevant, but precisely by constructing new mathematical objects: the distributions.

As I take it, Lützen's point is that in at least some historical threads in the development of pure mathematical theory, it is the mathematical objects themselves—not some formal structure in which they may be embedded—that is the key to understanding the relationship between mathematical techniques (e.g., employing an impossible function in an integral to define a linear functional) and their rigorization in pure mathematics (distribution theory).

Of course the authors' approach does provide, via partial-morphisms between the theories understood structurally, *a way* in which Dirac's theory can be related to Schwartz's, but this relationship misses most of the interesting theoretical dynamics that Lützen spends a book discussing. Something like the partial-morphism relationship is certainly *a part* of the meta-level background framework that Lützen implicitly employs in his (provisional) answers to questions like, who invented distributions and when? ("Sobolev in 1936 and Schwartz in 1950," Lützen [1982, *v*, but see also 159ff].)[8] But in this context, this meta-level

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matical techniques in question counts as the same pure mathematical theory.

[8]The authors themselves recognize the potential need for other meta-level frameworks, as will come up below.

Also, the authors' framework is perhaps more clearly at play in Lüzen's more philosophical claims, like "who was the first to use distributions in mathematics?" (Fourier in 1822) and "where do we first find them implicitly in a rigorous theory" (Bochner in 1932). It is also impossible not to mention a whimsical (though not irrelevant) parallel between one of the author's (French) "Viking" approach to analytic metaphysics [French, 2014, Sec. 3.2] and Lützen's case for Schwartz rather than Sobolev as the "true" discoverer of distributions, namely, that it is in same sense that framework seems to play much the same role that mathematical logic's framework of set theoretic foundations and formal deductions plays with respect to proving results in other areas of mathematics—i.e., not much of one.[9] The point here is to understand how Schwartz' theory of distributions is related to a "fusion of several tricks, methods, notions and ideas" [Lützen, 1982, 163] out of which it arose, and that is why Lützen engages the conceptual connections between a broad swath of concrete mathematical analysis, and a bit of functional analysis—but *nothing* from model theory or its ilk in mathematical logic.[10]

Another provocative challenge to such applications of the authors' account comes from Browder [1975, 585] (quoted in Lützen [1982, 162]), who argued that the theory of distributions lack their own "specific power" (unlike, for example, spectral theory), but do have an "important organizing role", like a "language rather than a methodology." Again, an analysis of the different roles for distribution and spectral theory—both of which are applied to other pure mathematics (e.g., linear partial differential equations, differential operators, Fourier transformations)—would not seem to gain much from a model-theoretic treatment.[11]

My second thought concerns a different but related aspect of the model theoretic approach. The authors write that, in a certain sense, theirs is a "modest" conclusion: they do not take the partial structures IDI account as constitutive (in any sense) of scientific theories, but rather as "meta-level devices" pragmatically justified based on "whether they help us, philosophers of science, achieve our aims, whatever they may be" (233). In the spirit of this pragmatic meta-project, I would suggest that when considering application (more) from philosophy of mathematics than philosophy of science, we might well need additional, differently oriented meta-level devices—especially in the context of (1) actual mathematical practice itself and also (2) the application of mathematical theory within mathematics itself.

Regarding (1), formal model theoretic approaches to mathematical theories are limited in their utility when analyzing mathematical application as social

Columbus "discovered [encountered!] America," not the Vikings, despite their arriving earlier—and interestingly, it was Schwartz himself who pointed out this Viking argument to Lützen (159).

[9] See for example Azzouni [2013, esp. 326-327] and Carter [2019, esp. Sec. 3.1.1].

[10] I owe thanks to Otávio Bueno for pushing me to make this point clear.

[11] And yet, Lützen's Appendix, "Alternative Definitions of Generalized Functions," considers three distinct ways to define distributions and then three further mathematical theories that, in some sense, *contain distributions* (or something like them), but are not quite equivalent—analyzing such claims might well benefit from the authors' treatment. Interesting work awaits!

practice; additionally, they often do not match up very well with how (non-logician) mathematicians think about mathematical entities, concepts, confirmation, and theories.[12] Regarding (2), Bueno's and French's (convincing) case that the mathematics itself (without physical interpretation) never need be explanatory (contra Batterman and others), does *not* obviously carry over to the application of mathematical theory *to mathematics itself*, since it is the *physical* interpretation of the pure mathematical structure that "insulates" it (as it were) from being explanatory within physical structures to which it is applied. While the issue of explanatory value within mathematics is complex and far from settled, as long as mathematicians' senses of it are taken seriously, then at least the possibility of math-on-math application being explanatory (in addition to justificatory) must remain open. To mention one example, Christopher Pincock [2015, 3] explicitly argues for the explanatory value of the theory being applied in a proof that brings together "two distinct mathematical domains where one domain is judged to illuminate the other" in the context of a Galois Theory proof that applies algebra techniques to number theory.

Finally, to revisit the question of "hybridity" in a different way: there may also be examples of the application of pure mathematical theory that are hybrids between being physical and mathematical. Consider, for example, the branch of mathematics known as *numerical analysis* (NA). Fundamentally, NA involves using mathematical techniques to generate numerical solutions to mathematical expressions. It has been argued that numerical methods can play instrumental, essential, and even explanatory/exploratory roles [Peressini, 2010]. In particular, the exploratory use of NA is rather pronounced in general relativity.

Einstein's field equations are notoriously difficult to solve for large velocities with small separations of similar masses in the presence of strong field phenomena; it was as recent as 2005 that breakthroughs were made in achieving stable solutions to such problems (e.g., binary black holes). Numerically solving Einstein's equations is known as "numerical relativity" (NR) [Cardoso et al., 2015, 7]. The NA being done in NR is rather singular as it is does not involve the standard approach of bracketing the physics and simply applying the pure mathematics (numerical analysis) to another pure mathematical problem (e.g., nonlinear system of PDEs). Rather, (1) the science is being reconsidered in the hope of discovering some physically motivated insight or simplification or reduction that will render the numerical problem tractable, and (2) the application of

[12] See references in Note 9 and also Azzouni [2006, Ch. 7–8], Azzouni [2009], and Wagner [2017, Ch. 3] for more extensive discussions.

the (unsuccessful) numerical methods to the actual problem are being simulated on related but simpler and better understood problems in order to gain insight into why the method goes fails on the actual problem.[13] Thus, "…NR is a gray area which could lie *at the intersection between* numerical analysis, general relativity (GR) and high-energy physics" [Cardoso et al., 2015, 7, emphasis added]. While I agree wholeheartedly with Bueno's and French's point that "no matter the strength of one's naturalistic inclinations, some caution must be exercised in taking scientists' own reflections on their practice at face value" (203, note 29; 226, note 6), this does seem to be a distinct kind of application that calls out for sustained attention by philosophers of science and math.

In Peressini [2010, 344–347], it is argued that in NR "the relationship between the formal theory and the physical world is richly mediated by the numerical analysis" and that in such cases where "the formal theory's numerical behavior is not fully understood … it requires the explanatory and exploratory machinery of numerical analysis to shed light on it in order for the model to make tractable predictions. Similarly, numerical insights can motivate mathematically equivalent ways of decomposing or understanding the formal model that can in turn shed light on the physical system itself." This quotation is not particularly clear regarding "explanatory" and "exploratory," and might (fairly) be interpreted as making a Batterman-like point about the mathematics being a necessary part of a physical explanation. I suspect *now*, however, that the authors' case against the explanatory role of *pure* mathematical theory for *physical* phenomena could be extended to the case of NR, though NR is still awaiting sustained philosophical attention like that given by the authors to QM.

In closing let me say clearly that you will want to have this book around to refer to if you do any work in the philosophy of math/physics/science. And if you work on mathematical application and explanation, you should read and carefully consider the book in its entirety.

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[13]For details, discussion, and references see Cardoso et al. [2015], especially Sec. 7, which has a "focus on applications of NR outside its traditional realm."

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