Taking Approximations Seriously:
The Cases of the Chew and Nambu-Jona-Lasinio Models

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1 Introduction

Philosophers of science have been writing about scientific modelling for many decades. The traditional focus on the relationship that models bear to abstract theories, and more recent debates about scientific representation and the ontological status of models, have arguably distracted attention away from an important aspect of modelling practice in mathematised sciences which we focus on here: the role played by approximation methods.

In order to help rectify this neglect, and lay some groundwork for an account of the way that approximation methods are implicated in mathematical modelling, we advance two descriptive theses in this paper. Firstly, paralleling claims about models made by advocates of the "models as mediators" slogan (Morgan and Morrison, 1999), we argue that approximations need to be recognised as a distinct species of scientific output, whose construction does not flow automatically from the kinematic and dynamical structures posited by a theory or model, a claim we call the **Distinctness** thesis. Secondly, we claim that, in at least some cases in modern physics, approximations play an integral role in assigning empirical and physical content to a model, in a sense which is difficult for some extant philosophical approaches to scientific modelling to account for, a claim we call the **Content Determination** thesis.

We support these claims with an analysis of two prominent historical episodes in post-war high energy physics: the development of the Chew and Nambu-Jona-Lasinio models. In both cases, the eponymous physicists were working with old Hamiltonians which had been introduced in the context of previously existing research programs. Physicists working in these research programs had used different approximation methods in combination with these Hamiltonians, generally without much success. Chew's and Nambu's contribution, as we will see, consisted of pairing those old Hamiltonians with new, much more successful approximation methods. In Chew's case, the approximation he developed was the key to connecting a previously empirically barren idealized model to the phenomenology of the strong interaction. In the Nambu-Jona-Lasinio case, the achievement of the new approximation scheme was to demonstrate that a model with exact chiral symmetry could describe massive fermionic particles—a result previously taken to be impossible. It thus played a crucial role in attaching a physical interpretation to an old Hamiltonian system.

We certainly do not claim to be the first to engage with approximation methods within the purview of the history and philosophy of science. In fact, there are several streams of literature which share an affinity with the position developed in this paper. In the philosophy of science, authors such as Mark Wilson, Robert Batterman and Nicolas Fillion have done important practice orientated work on applied mathematics which highlights the importance of approximation crafting in various ways—see, for instance, Batterman (2000, 2002, 2006), Wilson (2017) and Fillion (2021). A number of recent works in the philosophy of quantum field theory (QFT) have also engaged with the use of approximations in that context, treading similar ground to our analysis of our case studies—see James Fraser (2020), Koberinski (2021) and Miller (2021). Finally, historians of physics have discussed the importance of approximation crafting in the development of many areas of modern theoretical physics—some examples being Darrigol (2005, 2013), James and Joas (2015) and Kaiser (2009) (which we discuss further below). We see the main contribution of our paper as making the distinctive role played by approximation methods fully explicit. We hope that our approach may form a foundation for bringing together these existing strands of the literature, which have so far mostly developed independently, into a coherent narrative about the philosophical and historical significance of approximation methods.

The paper is structured as follows. The following section states our two theses and situates them in the context of the extant philosophical literature on scientific modelling. Sections 3 examines the Chew model and section 4 the Nambu-Jona-Lasinio model. We conclude, in section 5, by reflecting on further historical and philosophical questions which can be posed once our two claims about approximation methods are on the table, drawing inspiration, in particular, from David Kaiser's (2009) discussion of the "vanishing" of "scientific theory" in post-war high energy physics.

2 Two Theses about the Practice of Approximation

It will be important to start with some discussion of the key terms approximation, idealization, theory and model. These terms are, perhaps inevitably, somewhat nebulous and are not used consistently in the philosophical, historical or scientific literature, giving rise to the possibility of misunderstandings and merely verbal disputes. As we will discuss in more detail shortly, our claims about the role of approximation methods in the modelling process can be described in different, but essentially equivalent, ways depending on how one uses the term 'model'. Our first goal in this section will thus be to set out the conceptual distinctions we wish to make use of while being explicit about the conventional nature of some of the relevant terminology.

Specialising to the context of modern physics, we will use the term theory to refer to general frameworks like classical mechanics, non-relativistic quantum mechanics and QFT.[1] These frameworks do two basic things. They tell us what kind of mathematical structure is used to represent the space of possible states a system can have—quantum mechanics designates Hilbert spaces for this purposes—and they provide a framework for describing how a system's state evolves over time—quantum mechanics does this via the Schrödinger equation.

These precepts laid down by a theory are abstract, however. Additional input is needed in order to obtain a representation of a particular system. This is, at base, what the modelling process is about. Invariably in science, we have no hope of describing the behaviour of a real system out in the world exactly. We instead start by writing down a fictional system that is intended to provide an inexact representation of the target, often getting some of its properties radically wrong while, hopefully at least, getting others right. This practice of representing a real system with a simplified fictional one is often referred to as idealization in the philosophy of science literature (Norton 2012, Morrison 1999). We will follow this usage and refer to the fictional system employed in this way as an idealized system.

In order to specify a particular system to act as an idealization one has to "fill in" the abstract kinematic and dynamical structures afforded by a theory. In the context of quantum mechanics, for instance, writing down a particular system means specifying a particular Hilbert space and Hamiltonian operator (which implements the Schrödinger

[1]Another way of drawing the theory-model distinction, which has some currency in both the scientific and philosophical literature, uses 'theory' as an honorific to indicate community acceptance and 'model' to indicate provisionality. Newtonian gravity and quantum electrodynamics, for instance, are referred to as theories in this sense, though they are models in the abstract-particular sense focused on here. The notion of a model as a representation of a particular system will be most relevant for our purposes and we will not take either of these terms to imply anything about confirmatory status.

equation time evolution). Our case studies in the following sections will involve quantum field theories (QFTs).[2] How QFT should be articulated as an abstract theory, and how particular QFT systems are to be constructed, are tricky questions. The fact that the exact mathematical structures underlying QFT are somewhat nebulous is significant for our discussion, and this issue will be elaborated on later on (especially in section 5). However, for practical purposes at least, the key choice that goes into selecting a QFT system to use as an idealization is, like the quantum mechanics case, the choice of a Hamiltonian—now a product of field, rather than position, operators.[3]

Where does approximation fit into the picture? While there is, as we said, some terminological variation, in the philosophical literature the notion of approximation is often explicated via a contrast with the practice of idealization. Whereas in the case of idealization one provides an inexact description of the target systems properties by introducing a new system, in the case of approximation, one provides an inexact description of particular properties of the target system directly, without referring to a new system (Norton, 2012). The key distinction, therefore, is between indirect and direct modes of representation. How this distinction applies to mathematical modelling practice is best grasped by way of an example.[4]

Suppose the target system we are interested in is a quantum harmonic oscillator acted upon by a linear external field. The Hamiltonian of the system (written as a function of the external field strength V) is:

$$H(V) = \frac{p^2}{2m} - \frac{1}{2}m\omega^2 x^2 - Vx. \tag{1}$$

This system, in fact, admits exact solutions. We can thus obtain a closed expression for

[2]See Koberinski (2021) for a related discussion of how the terms theory and model and can applied in the QFT context.

[3]Often, of course, field theorists worked with a Lagrangian formalism so we should really speak of the choice of a Lagrangian here. We do not think that the differences between Lagrangian and Hamiltonian formalism are particularly important for our discussion of approximation practice, so for the sake of reducing potentially confusing terminologically clutter we refer to the Hamiltonian throughout this paper. Whichever function is used, the key point is that it is taken to describe contain the distinctive dynamical information about a particular physical system.

[4]Some authors have used a different strategy in order to engage with the topic of approximation, which stays closer to the methods of applied mathematics and does not make use of the notion of an idealized system (see, for instance, Batterman 1997, Fillion 2021). We employ the more conventional strategy of drawing a distinction between idealization and approximation as it provides a convenient language for our **Distinctness** claim (which, in turn, underlies the more programmatic remarks that we make in Section 5). However, note that our analysis is not necessarily wedded to this terminological framework. As long as an alternative framework for describing modelling practice has a place for approximation methods in its taxonomy it should be possible to formulate a version of **Distinctness**.

the ground state energy:

$$E_0(V) = \frac{1}{2}(\hbar\omega - \frac{V^2}{m\omega^2}). \tag{2}$$

This is, of course, an artificial toy example designed for expository purposes only. When it comes to real physical systems, we do not know how to write down an exact Hamiltonian and even if we could, exact solutions would be out of the question. We thus need to extract information via some other means, the general strategy being to simplify the problem in such a way that the aspects of the target system we are trying to predict or explain are not much affected.

Suppose, in our example, that the strength of the external field is weak relative to that of the harmonic oscillator coupling. There are two ways that one might go about neglecting the effects of the external field. On the one hand, we might take the limit $V \to 0$ of the Hamiltonian. In this case, we would be moving to a new Hamiltonian, $H(0)$, and thus using a new system (the familiar harmonic oscillator) as an idealization of our original system. On the other hand, we might simply take the $V \to 0$ limit of a particular quantity we want to calculate. We might use $E_0(0)$ to approximate $E_0(V)$. This is the way that philosophers of science typically precisify the notion of approximation. In the case of idealization, we construct a new model, which captures some of the features of the target system while distorting others; in the case of approximation, we construct an inexact description of one of its properties directly, without referring to a new system.

Note that, in this example, these two approaches are interchangeable, or rather, the former encompasses the latter. The ground state energy of the harmonic oscillator is $E_0(0)$ so, as far as calculating this quantity is concerned, taking the $V \to 0$ limit of $H(V)$ and $E_0(V)$ give the same results. Redhead (1980) suggests that this is always the case: it will always be possible to embed an approximation within a corresponding idealized system. In general, philosophers of science have had much more to say about idealization than approximation. Idealizations are physical systems in their own right, albeit fictional ones, and consequently have much richer representational content than approximations, which are often understood as purely formal objects. As Morrison (1999, 42) says, "approximations are introduced in a mathematical context and the issue of physical interpretation need not arise". Since philosophers have tended to emphasize the explanatory goals of science, and to understand explanation in terms of the representation of underlying physical entities and structures, it is easy to see how models might be taken to be 'doing the real work', as it were, with approximations piggy-backing upon them.

Recently, a number of philosophers have started to question this deflationary reading of the idealization-approximation distinction on general philosophical grounds. Norton (2012) argues that Redhead's equivalence claim is false, pointing to examples where taking the infinite limit of a parameter of an idealized system and taking the corresponding limit of a particular quantity defined on it lead to different results. Others have suggested that taking approximations seriously as a distinct kind of theoretical output helps to sheds light on conceptual problems in the foundations of physics (James Fraser 2020, Fletcher 2019).

Our focus is different, however. What we are interested in here is developing an account of the role that approximation plays in actual scientific practice. The prioritization of idealization has arguably led philosophers of science to focus on simple models, often drawn from non-mathematical sciences, so that questions about the nature of indirect representation can be posed without intrusions from the travails of applied mathematics. If we look at what theoretical physicists spend their time doing, however, we find that a great deal of their energy is consumed by the construction of approximations. Whether or not approximations can always be embedded in an idealized system in some ideal reconstruction or not, in practice the business of crafting approximations for particular purposes often proceeds without doing so. Thus, if we take a practice-orientated perspective, we need to take approximation seriously as worthy of historical and philosophical analysis.

With the aspiration of laying some groundwork for a descriptive account of the role that approximations play in scientific modelling practice, we advance two theses in this paper. The first we call, for short, **Distinctness**:

> **Distinctness:** Approximations need to be recognised as a distinct category of theoretical output from idealized systems, which jointly play a role in the modelling of real-world systems. The practice of their construction also needs to be recognised as distinct from the practice of constructing idealized systems.

An important terminological clarification is in order here, as this claim can be read differently depending on how one uses the term 'model'—a concept which we have so far deliberately not precisely defined. Some philosophers explicitly or implicitly use the term 'model' in the same way that we have used the term 'idealized system': that is, they take a scientific model to simply be a fictional system that is used as an indirect representation.[5] On this usage, when one selects a Hamiltonian and assigns some physical meaning to the parameters and variables that appear in it, one has presumably specified a model. Adopting this nomenclature, our claim would be that, in addition to models, one must recognise approximations as a distinct class of representations that work in tandem to

[5]Structuralist approaches to models and scientific representations often identify models with set-theoretic structures which satisfy the axioms of the underlying theory. See, for instance, Redhead (1980) for a structuralist characterization of models which matches up with our usage of the term idealized system. Fictionalist approaches to scientific representation often identify models with the fictional entities introduced in the act of indirect representation; see, for instance, Godfrey-Smith (2006).

fulfil physicist's goals of predicting and explaining real-world phenomena. Alternatively, some authors favour a broader usage of the term model. Morgan and Morrison (1999) take models to be anything that mediates between abstract theory and empirical data. Leonelli (2007) is another example of a more inclusive views, which allow other representational resources besides idealized systems to be considered as models, or parts of models. On these more permissive readings, an approximation can perhaps be considered a component of a scientific model—as we shall see, this would seem to reflect scientific usage of the term model in the case of Chew and Nambu-Jona-Lasinio models. Adopting this nomenclature, our claim would be that, besides idealized systems, approximations need to be acknowledged as a distinct component of a scientific model.

Ultimately, we do not think much hangs on which of these terminological options one adopts (we continue to use the term idealized system for the more restricted sense of model to avoid potential ambiguities, however). The important point is that there is more to modelling in mathematized sciences than the construction of idealized systems.[6] As the second clause of **Distinctness** indicates, an important sense in which approximations are distinct theoretical products is that their construction does not flow logically from either general theoretical principles or the kinematic and dynamical structures posited by a given idealized system. One of the key points we want to make with our case studies is that it is far from obvious what kind of approximation technique should be coupled with a given Hamiltonian. Crafting an appropriate approximation for the modelling task at hand requires additional theoretical input, insight and creativity. One of the claims that advocates of the "models as mediators" slogan wanted to make about models is that their construction is, in some sense, autonomous from the construction of abstract theories. **Distinctness** can be read as making an analogous claim about the autonomy of approximation construction from the construction of idealized systems and theories.

While we want to avoid subjugating approximation crafting to idealization, we also want to avoid assimilating it to pure mathematics. The term "approximation method" suggests the existence of abstract, repeatable, algorithms for generating approximations, which can be applied to different models. The detachable nature of approximation methods, which allows them to be transferred between different fields of applied mathematics, is certainly an important aspect of approximation practice that historians and philosophers of science could likely also benefit from paying more attention to. We emphasise the opposite side

[6]One strategy for dodging these terminological difficulties is to move focus away from theoretical products, like 'models', and towards theoretical process, like 'modelling'. From this perspective we could rephrase **Distinctness** as claiming that we need to recognise approximation as a distinct sub-practice of the broader process of modelling (which we already effectively endorse in our current phrasing of the thesis). Once again, we would like to remain neutral on whether moving to a thoroughly process orientated terminological framework is superior here.

of the coin here, however. We council against viewing approximations as purely formal tools which can be taken off the shelf and mechanically applied without attending to the details of a particular modelling context. Perturbation theory and the Hartree-Field approximation formed the basis of Chew's and Nambu-Jona-Lasinio's approximations, but much of their work was concerned with adapting these approaches to the target phenomena at hand. Thus, we claim, approximation should be viewed as a genuine part of the process of modelling a particular real-world system. **Distinctness** asserts that idealized system and approximation construction are different activities, but not that they are completely independent and unrelated: they are intertwined aspects of mathematical modelling.

The second, more ambitious, claim which we want to advance in this paper we call **Content Determination**:

> **Content Determination:** Coupling an approximation to an idealized system is, in at least some cases, instrumental in assigning determinate empirical and physical content to it.

That approximations are involved in the elaboration of a models claims about the world, and especially its empirical predictions, is perhaps not particularly startling from the point of view of extant accounts of scientific representation. A common idea in many of these approaches is that modelling is fundamentally a matter of so-called surrogative reasoning. We posit a fictional system, study it, drawing conclusions about its properties, and then transfer these conclusions over to the real-world system it is supposed to represent. If we admit that these chains of reasoning are not strictly deductive, as will surely be necessary if we are to make contact with actual scientific practice, it appears that this picture can straightforwardly accommodate a role for approximations in deriving claims about an idealized system.

Content Determination is to be read as asserting something stronger than this, however. The characterization of surrogative reasoning just sketched seems to assume that any claim that is obtained by way of an approximation is, in principle, already contained within the representational content of the relevant idealized system, or at least follows from it deductively. Approximations merely play a supporting role, giving us epistemic access to a system's properties when analytic solutions are not available.[7] We find it implausible to interpret our case studies in this way. The approximations developed by Chew, Nambu and Jona-Lasinio were required in order to assign meaningful representational content to their models in the first place, or so we will argue. While Chew's approximation

[7] We confess that we are caricaturing our foil somewhat here. Sophisticated versions of the major approaches to scientific representation may be able to accommodate our points about the distinctive role played by approximation methods in mathematical modelling, though it is up to the advocates of these views to show this.

was primarily deployed in the service of deriving empirical predictions, it is hard to see these predictions as already being contained within the strong coupling Hamiltonian he inherited from his predecessors. We, instead, want to understand his approximation as associating new empirical content with this Hamiltonian. In the Nambu-Jona-Lasinio case, the approximation they developed, in fact, led to a fundamental reinterpretation of the particle content associated with a Hamiltonian, undermining the implicit assumption in the naive picture of surrogate reasoning sketched above that physical content can be assigned to an idealized system before approximations arrive on the scene.

Part of the reason that approximations play a more fundamental role in establishing the representational link between model and reality in our case studies is that the characterization of QFT systems before approximation methods are brought to bear is, at best, extremely thin. In the 1930s the problem of ultraviolet divergences had led to serious doubts about whether QFT was even a consistent theory. The development of renormalized perturbation theory immediately after the war partially solved this problem, allowing physical and empirical claims to be extracted from a QFT Hamiltonian via perturbative approximations of scattering amplitudes. However, the question of how a QFT's kinematics and dynamical equations could be written down in an exact form, outside of the confines of the new perturbative formalism was not answered and worries about the ultimate consistency of QFT lingered. Perturbation theory thus came to play an indispensable role in attaching empirical predictions to a field theoretic Hamiltonian, and QFT systems were invariably interpreted through a perturbative lens.[8]

Both of our case studies take place in the 50s and 60s, however, by which time high energy theorists were pushing up against the limitations of the perturbative approximation, especially as a tool for describing strong nuclear interactions. What we see in the stories of the development of the Chew and Nambu-Jona-Lasinio models are a series of attempts to develop new approximation methods which could take perturbation theory's place as a means of attaching representational claims to a field theoretic Hamiltonian. Crucially though, the deep questions about the mathematical foundations of QFT remained unaddressed: Chew, Nambu and Jona-Lasinio used old Hamiltonians as inputs for new approximation methods without addressing whether a QFT system corresponding to that Hamiltonian could in fact be constructed.

In addition to providing a starting point for a broader historico-philosophical investigation of the role of approximation methods in scientific modelling practice a second aim of the

[8]For instance, the standard practice of reading the particle content of a QFT off from the kinetic terms in its Hamiltonian is arguably grounded in the use of the free theory (with interactions completely turned off) to describe incoming and outgoing scattering states within the perturbative formalism. The Nambu-Jona-Lasinio story is ultimately all about overcoming the limitations of this interpretive approach.

present project is to shed light on the specific, and potentially novel, status of approximation methods in post-war high energy theorising. We will expand on this later theme further in section 5, relating it to David Kaiser's claims about the "vanishing" of scientific theories in the post-war period. The following two sections turn to our case studies, using them to further elaborate and support our **Distinctness** and **Content Determination** claims.

3 The Chew Model

We start the historical portion of the paper with an examination of the so-called Chew model, an early approach to the strong nuclear interaction developed by Geoffrey Chew during the 1950s. We shall see that what was called the Chew model was, in reality, the combination of an old Hamiltonian, introduced in the strong coupling program of the 40s, and a new approximation developed by Chew. This story makes clear that the construction of an appropriate approximation method is a substantive additional step to the construction of an idealized system and important theoretical innovations often occur at this stage. It thus supports **Distinctness**. We also find that Chew's approximation method played an essential role in extracting empirical predictions from this Hamiltonian, bringing some of the first concrete successes in modelling strong interactions. We suggest that it is difficult to understand Chew's achievement as merely providing access to some sort of pre-existing empirical content, that was somehow already contained within the strong coupling Hamiltonian. Researchers working in the strong coupling program, in fact, had derived different empirical predictions from the same Hamiltonian by making use of a different approximation method—although these predictions, unlike Chew's, had been mostly unsuccessful. The case-study thus supports **Content Determination**.

The section is divided into two subsections, one for each of the two main ingredients of the Chew model. We start by tracing the origins of the Hamiltonian Chew inherited from his predecessors (3.1) and then turn to the approximation method that he developed in the early 1950s (3.2).

3.1 The Strong Coupling Hamiltonian

Throughout the entire series of papers in which Chew developed the Chew model, he made use of the very same interaction Hamiltonian (Blair and Chew 1953b, Chew 1953, 1954a, 1954b, 1954c, 1954d). The Hamiltonian in question is meant to describe the interaction between nucleons and pions and it reads as follows:

$$\mathcal{H}_{int} = \sqrt{4\pi}\,\frac{f}{\mu}\int d\vec{r}\rho(\vec{r})\sum_{\lambda=1}^{3}\tau_\lambda\;\vec{\sigma}\cdot\vec{\nabla}\phi_\lambda(\vec{r}). \tag{3}$$

In this expression, the pion field is represented by ϕ_λ, whereas the nucleon is represented by a continuous charge distribution $\rho(\vec{r})$. The quantities μ and f are both constants, with μ representing the mass of the pion and f being a dimensionless coupling constant. Finally, the τ_λ, σ_i with $\lambda, i \in [1, ..., 3]$ are Pauli matrices acting on the isospin and spin degrees of freedom of the pion field, respectively.

Although Chew himself noted that this Hamiltonian had been introduced by others, contemporary authors writing on the Chew model have tended not to make much of this fact (Polkinghorne, 1989; Cushing 1990). As we shall soon see, however, careful consideration of the origins of this Hamiltonian is crucial to understanding Chew's role in the development of the model and, in fact, the very nature of the model itself.

In his articles on the Chew model, Chew attributed Hamiltonian (3) to Pauli, who had used it in a series of lectures on meson theory in 1944 (Blair and Chew 1953a, p. 170, 179; Blair and Chew 1953b, n. 5; Chew 1954d, n. 1; Pauli, 1948). Although it is true that Pauli had discussed this Hamiltonian in that context, and although other scholars have acknowledged this much in their treatments of the Chew model, simply attributing the model to Pauli does not begin to do justice to the larger story behind its origins (Cushing, 1990). The 1944 lecture cited by Chew, in fact, was devoted to a now largely forgotten attempt to come to terms with the strong interaction that was known as the strong coupling approach. It is to this line of research, therefore, that we must turn in order to understand the reasons behind the introduction of Hamiltonian (3), and the manner in which it was used.

The strong coupling approach was developed in response to a number of perceived difficulties in the study of the strong interaction. Chief among them was the suspicion, widely shared at the time, that the dimensionless constant measuring the strength of the coupling between nucleons and pions was larger than one. The nature of this problem is worth elaborating upon so as to better appreciate the logic behind this program. Since its inception in 1935, Yukawa's "meson" theory had provided the paradigmatic framework for the application of quantum field theory to the strong force. In order to extract empirical predictions from this theory, however, one had to follow the example provided by quantum electrodynamics and use perturbation theory to approximate the values of the relevant quantities of physical interest.

This procedure started by dividing the Hamiltonian of the system into a free Hamiltonian and an interaction Hamiltonian:

$$\mathcal{H} = \mathcal{H}_0 + g\ \mathcal{H}_{int}. \tag{4}$$

If the coupling constant g is known to be small, as in the case of quantum electrodynamics, then we can consider the interaction Hamiltonian to be a small perturbation of the free Hamiltonian and apply the usual formalism of perturbation theory to approximate the energy eigenvalues of the full Hamiltonian $\mathcal{H}$ in terms of those of the free Hamiltonian $\mathcal{H}_0$. This results in a series expansion in terms of rising powers of the coupling constant g of the following form:

$$E = E_0 + \sum_{n=1}^{\infty} g^n\ A^{(n)} \tag{5}$$

In quantum electrodynamics (QED), of course, one found that the coefficients $A^{(n)}$ of the series diverged, yielding seemingly absurd results for the energy eigenvalues of the interacting system. But in the case of the strong interaction one had the additional worry that the coupling constant used as a parameter in the power series could very well be larger than one. This possibility was indeed suggested by the fact that the nucleus seemed to be more tightly bound than the hydrogen atom, and it added a further layer of absurdity to the very idea of applying perturbation theory to the study of the strong force.

This concern led the German physicist Gregor Wentzel to devise an alternative approximation method that would be more appropriate to the study of the strong force (Wentzel, 1940). Wentzel's idea consisted of assuming that the coupling constant for the strong interaction was indeed larger than one, and of reversing the roles that the free and the interaction Hamiltonian play in the perturbative scheme outlined above. The procedure would thus still start by dividing the Hamiltonian of the full interacting system into a free Hamiltonian and an interaction Hamiltonian as in (4). Since the coupling constant g was now assumed to be large, however, it was the free Hamiltonian that had to be taken as a small perturbation of the interaction Hamiltonian. One would then go on to apply the formalism of perturbation theory to obtain an estimate of the energy eigenvalues of the interacting system. Because the roles of the free and the interaction Hamiltonian had been reversed, however, the resulting series differed from the usual result given by (5) in two important ways. First, the expansion parameter for the series was not the coupling constant anymore, but its inverse, $1/g$. This was good news, obviously, since g had been assumed to be larger than one. The second difference, however, was that the series now provided an estimate for the energy eigenvalues of the full Hamiltonian in terms of the eigenvalues of the interaction Hamiltonian:

$$E = E_{int} + \sum_{n=0}^{\infty} (1/g)^n B^{(n)} \tag{6}$$

Since the eigenvalues of the interaction Hamiltonian were normally harder to determine than those of the free Hamiltonian, this introduced an additional layer of complexity to the problem. As a result of this, papers in the strong coupling approach often included long sections in which the algebra necessary to diagonalize the interaction Hamiltonian was laboriously worked through. The main goal of developing an approximation method that yielded a power series in terms of the inverse of the coupling constant, however, had been accomplished.

We thus find that the interaction Hamiltonian given by (3) was introduced to be used in combination with the approximation method devised by Wentzel and others starting in 1940. As it turns out, this explains some of the features of the Hamiltonian itself. The fact that the nucleon is modelled by a charge distribution $\rho(\vec{r})$, in particular, is explained by the fact that, in the strong coupling approach, the free Hamiltonian needs to be taken as a small perturbation of the interaction Hamiltonian—an assumption that would be undermined by the inclusion of a kinetic term for the nucleon in the free Hamiltonian. The rest of the features of Hamiltonian (3), including the specific choice of the spin and isospin operators τ_λ and σ_i, are given by the choice of a specific kind of Yukawa coupling with certain parity properties. The specific choice made by Pauli in his 1944 lecture, and later endorsed by Chew, corresponds to the symmetrical pseudoscalar version of the Yukawa theory that was widely agreed to be correct after the discovery of the pion in 1947.

Before we go on to comment on the manner in which Chew used this Hamiltonian to develop the model that now bears his name, it is worth saying a few words about the goals that physicists working in the strong coupling approach set for themselves, and about the ultimate fate of their research program. An obvious major goal consisted of recovering the values of some of the quantities characterizing the simplest processes involving the strong interaction. The cross-section for pion-nucleon scattering and the magnetic moments of the proton and the neutron, in particular, were the two main quantities that physicists working in this tradition aimed to recover. Pauli, Dancoff, Serber, and others thus mobilized the alternative perturbative formalism that we introduced above to derive estimates for these two quantities from the various versions of meson theory that were still available at the time (Pauli and Dancoff, 1942; Serber and Dancoff, 1943).

Unfortunately, their results were generally disappointing. The question of the convergence of series (6), for one thing, was far from being resolved. The charge distribution ρ used to model the nucleon introduced a cut-off in momentum space that prevented the coefficients $B^{(n)}$ in (6) from diverging. This provided a way of bracketing out the divergence problem for the time being but, of course, the results obtained by proceeding in this manner still diverged in the limit in which ρ reduces to a point source. It was generally expected that this problem would eventually be resolved by some fully-fleshed-out, yet-to-be-developed version of the strong coupling approach, but this hope was never realized. The fact that the divergence problem was shared in one way or another by virtually every other quantum field theory available at the time, however, made this problem seem less pressing.

In any case, the predictions obtained for the cross-section of pion-nucleon scattering and for the magnetic moment of the proton and the neutron tended not to be very accurate. For the first one of these two quantities, a trick was devised to bring theoretical predictions and experimental results in agreement with each other. This trick was suggested by Oppenheimer and Schwinger who, building on previous work by Heisenberg and Bhabha, suggested in 1941 that the cross section for pion-nucleon scattering could be made smaller—as required by experiment—by the presence of nucleon "isobars" with higher values of charge and spin (Oppenheim and Schwinger 1941). This suggestion was added to the agenda of the strong coupling approach with some success, as some authors did manage to incorporate isobars into their theories in a manner that would reduce the cross section for pion-nucleon scattering in the required ways. Theoretical estimates for the magnetic moment of the proton and the neutron, however, irremediably tended to miss the mark by a lot.

Confronted with these mixed results, interest in the strong coupling approach slowly waned as the 1940s moved forward. The partial success achieved by the recovery of the value of the cross section for pion-nucleon scattering, together with the numerous assumptions built into the strong coupling approach, did spur some debate as to how exactly the promise of the whole approach was to be assessed. However, the discovery of an alternative explanation for the abnormally small cross section for pion-nucleon scattering brought the strong coupling approach to an abrupt end in 1947. As it became clear with the discovery of the pion that year, physicists had until then been mistaking the muon for the pion. Having failed to look at the right phenomena in the first place, it was no surprise that theoretical and experimental estimates for pion-nucleon scattering failed to be in agreement with each other.

3.2 Chew's Approximation Method

After 1947, the strong coupling approach was mostly abandoned. Aside from sporadic references in the occasional review article, or in more didactic pieces such as the lecture series by Pauli that Chew cited, one finds few mentions to it in actual research articles after the discovery of the pion. In 1952, however, Chew went back to the strong coupling Hamiltonian and started using it for his own purposes. He did so in a series of papers that he published between 1953 and 1954, in which he effectively developed the Chew model. Chew's main goal throughout this series of papers, as we shall see, consisted of developing an approximation method that would produce empirically adequate predictions when coupled with the strong coupling Hamiltonian. As we advanced at the beginning of this section, in fact, the Chew model is nothing but the combination of the strong coupling Hamiltonian with a new approximation method developed by Chew.

Chew's work on the Chew model arrived at a time in which physicists doing research on the strong force seemed to have run out of alternatives. On the one hand, and as we just saw, the strong coupling approach had failed to deliver much by means of empirically accurate predictions. This seemed to suggest that the coupling between pions and nucleons was not strong after all, and that standard perturbation theory may be applicable to the study of the strong interaction. But when weak coupling calculations were performed, they also turned out to be irremediably at odds with experience (Noyes et al, 1952). By 1950 or so, therefore, the simultaneous failure of both weak and strong coupling approaches seemed to leave no way forward in the study of the strong interaction.

In a talk given at the Third Rochester Conference in High-Energy Physics in 1952, Chew suggested a way out of this apparent dead end (Noyes et al, 1952). As Chew pointed out, weak coupling calculations were often based on a number of approximations that had not been consistently implemented. Weak coupling estimates for the cross section of pion-nucleon scattering, in particular, tended to neglect nucleon recoil and nucleon-antinucleon pair formation, effectively treating the nucleon non-relativistically. In order to implement this approximation consistently, however, one needed to introduce a cut-off limiting the momentum of the pions involved in the process so as to prevent them from reaching the energy necessary for creating a nucleon-antinucleon pair. Since weak coupling calculations did not include this kind of measure, Chew argued, their failure to match experimental data could not be taken as an indication that the coupling constant associated to the pion-nucleon interaction was not small (Noyes et al, 1952).

The strong coupling Hamiltonian included precisely this kind of cut-off, so Chew's proposal consisted of adopting it for the purposes of performing weak coupling calculations. Chew did not mention the strong coupling approach in his talk at Rochester, or in any of his papers on the Chew model, merely citing Pauli's lecture series when necessary (Blair and Chew 1953, Chew 1953). Pauli's lecture series did devote a chapter to the strong coupling approach, and it was in this context that Hamiltonian (3) was mentioned. The lecture series, however, aimed at offering a comprehensive treatment of the meson physics of the time, and it covered many other topics apart from the strong coupling approach. It is perhaps because of this that, as we mentioned above, historians of physics have missed the broader story leading to Chew's adoption of Hamiltonian (3). Chew's reasons for resorting to this Hamiltonian were very different form Wentzel's, of course, so this omission is perhaps excusable if one is solely concerned with understanding Chew's own work on the Chew model. Unfortunately, this failure to account for the origins of Hamiltonian (3) has obscured an important fact, namely the fact that Chew's efforts to understand the strong force were part of a larger process in which different approximation methods were created and then coupled with the same Hamiltonian.

Chew's new approximation method was developed as a response to a central problem facing the straightforward application of perturbation theory to Hamiltonian (3). In a pair of journal articles that he published in 1953, Chew attempted to carry through on the proposal that he had aired at Rochester, calculating perturbative terms for the strong coupling Hamiltonian up to fourth order (Blair and Chew, 1953). He went on to demonstrate that the value for the pion-nucleon scattering cross section thus obtained was consistent with the empirical data available at the time (Chew, 1953). Furthermore, the value of the coupling constant f that resulted from this calculation turned out to be 0.8, which seemed to vindicate Chew's weak coupling approach to the problem.

Another issue arose here, however, which problematized this first attempt to attach a new approximation to the strong coupling Hamiltonian. Chew's calculations showed that some of the fourth order terms in the perturbative expansion stemming from (3) were similar in magnitude, or even larger, than the second order terms. This raised two issues. Firstly, it suggested that the expansion could not be convergent. As it happens, the possibility that QFT perturbation series diverged, even after renormalization, was being investigated around this time, with a consensus forming that renormalized QED perturbation theory was, in fact, divergent (see Blum, forthcoming). While this might raise foundational questions about the status of the perturbative expansion, the strong apparent convergence of QED perturbation theory over the first few orders reassured theorists that its expansion was at least an asymptotic series, whose truncations could be trusted to provide good approximations. A second, more practical problem that arose for Chew, however, was that the terms he had calculated did not even seem to display good apparent convergence. The fact that fourth order contributions to the standard perturbation series were found to be relatively large called the reliability of a perturbative approximation strategy into question. If the series was already starting to diverge then calculating higher order corrections would actually make the approximation worse rather than refining it, raising the possibility that the apparent empirical success of these early calculations was merely a fluke.

On top of this theoretical concern, it became clear that the agreement between Chew's 1953 results and empirical data was not as close as it initially seemed. Throughout the early 1950s, there had been hints about the presence of a "resonance" that manifested in a sharp increase of the cross section for pion-nucleon scattering at around $210 MeV$ (Chew 1954b). The existence of this resonance was of considerable theoretical interest as it seemed to indicate that a particle with mass equal to $210 MeV$ and spin and isospin values equal to $3/2$ intervened as an intermediate state in the scattering process. Chew's results could not account for this kind of behavior so, when evidence for the existence of the $(3,3)$ resonance—as this phenomenon came to be known—solidified in 1954, he was forced to revisit his treatment.

It was in response to these problems that Chew developed his new approximation method. He did so in a series of four further articles that he published in 1954 (Chew 1954a, 1954b, 1954c, 1954d). Chew's new approximation method consisted of two main ingredients. On the one hand, Chew performed an infinite sum containing terms of different orders in perturbation theory. The result provided by this infinite sum constituted the lowest order of approximation of his new method. On the other hand, Chew devised a method for assigning a new, lower order to all the anomalously large terms in the perturbation series. This scheme could be used to produce higher order approximations in Chew's new method. Although Chew did not go on to check that his new series expansion converged, he now found good apparent convergence in the first few terms, and furthermore managed to show that his new approximation could account for the $(3,3)$ resonance (Chew, 1954b, 1954c). His new calculational scheme also rendered a value of 0.02 for the coupling constant, which was once again consistent with Chew's weak coupling assumption.

Having accomplished this, Chew tried to account for the rationale behind the new approximation method that he had developed. In order to do that, he pointed to a number of resonance effects that took place as the frequency of a virtual pion, ω, came close to that of the real, incident pion, ω_0 (Blair and Chew 1953, Chew 1954b). As Chew showed, some of the terms in the series that resulted from applying perturbation theory to Hamiltonian (3) included factors of the form of $\omega - \omega_0$ in their denominator. Whenever the value of ω approached that of ω_0, therefore, their size became far greater than that of a typical term of their order, compromising the convergence of the series. Chew's new approximation technique, then, had the effect of rearranging the terms responsible for these kinds of effects as required to circumvent the problem.[9] This early account, however, was never completely fleshed out and its importance was soon de-emphasized by Chew. Already in 1955, he launched a new attempt to account for the success of the Chew model, this time in cooperation with Francis Low (Low 1955, Chew and Low 1956). This new attempt

[9]But note that, as we saw above, the lowest order in Chew's new approximation method included an infinite sum mixing terms of all orders in perturbation theory. Higher approximations also resulted from adding terms of different orders in the original perturbative expansion, but they included a finite number of terms only. For these reasons, the rearrangement of perturbative terms effected by Chew in 1954 amounted to a genuinely new approximation method, as opposed to a mere reordering of the terms in the standard perturbative series. This is of course the reason why Chew's new approximation method could account for the new behavior exhibited by the $(3,3)$ resonance, while his earlier perturbative treatment of 1953 could not.

focused on the abstract formal properties of the Chew model, as opposed to investigating the concrete physical mechanisms responsible for its success. Towards the end of the decade, this line of research would lead to Chew's work on the S-matrix program, which was characterized by a similar attitude (Cushing 1990). Chew's shifting priorities as the decade went by, then, together with the new philosophy behind the S-matrix program, resulted in the original task of accounting for the success of the Chew model never being fully accomplished.

Although doubts remained about the ultimate foundation behind his work, in any case, the results that Chew obtained in 1954 were perceived to be a great success. It is owing to these results, in fact, that the "Chew model" is still remembered as an important achievement. In spite of Chew's somewhat liberal calculational leaps, there is ample reason for this warm reception of his results. After all, Chew's was the first attempt ever to succeed at connecting a Yukawa-type nucleon-pion model of the strong interaction to experience—after 15 years of unsuccessful attempts. It did so only for a very restricted non-relativistic domain, but it incorporated the latest phenomenology, including the $(3,3)$ resonance, and it showed that weak coupling techniques could successfully be applied to the strong force, at least in some regimes.

We return now to our theses about approximation practice laid out in Section 2. Our historical analysis of the development of the Chew model provides fertile ground for expanding on the two theses introduced there.

The story we have just told provides strong support for, and a vivid illustration of, **Distinctness**. We have seen that what is called the Chew model is really nothing but the combination of an old Hamiltonian, which Wentzel had introduced in the context of the strong coupling approach, with a new approximation that Chew developed in 1954. Can the calculational manipulations which Chew introduced somehow be embedded within a new idealized system? Maybe in some extremely revisionary reconstruction—we find this doubtful but we have given no arguments against it here. If we are interested in describing actual scientific practice, however, this question is largely irrelevant. It is clear that Chew made no attempt to associate each order of his new series expansion with an idealized system. If our aim is to describe scientific modelling practice, therefore, we need to see Chew as being primarily concerned with the construction of an approximation, in the sense introduced in section 2, rather than idealization.

The Chew case lends substance to the claim that approximation construction is an autonomous enterprise with respect to the construction of idealized systems and theories. The approximation method that Chew devised in 1954, unlike Wentzel's, ended up achieving the ultimate goal of empirical adequacy. But there was no way of telling beforehand that this was going to be the case. There was no obvious clue, in particular, that could have been discerned from the Hamiltonian—either by itself or in combination with empirical data—that indicated which one of the two approximation methods was going to produce empirically adequate predictions. Furthermore, Chew was not simply taking approximation methods off the shelf and blindly applying them. While the approach he settled on was grounded in the standard set up of perturbative QFT, Chew built something new out of these raw materials, introducing a novel series expansion which incorporated non-perturbative elements through the use of partial series resummation. That is to say, his approximation technique was thoroughly tailored to the phenomenon he was trying to model.

In light of all this, we find it difficult to make sense of the historical progression between 1940 and 1954 we have described without acknowledging approximation construction as a distinct stream of theoretical development. The virtual totality of the theoretical efforts carried out in these decades was directed at the development of an approximation method that could extract accurate empirical predictions from Hamiltonian (3), with no attempts being made to modify the Hamiltonian itself. This demonstrates that the positing of an initial idealized system is just one part of the modelling process and in the context of post-war high energy physics at least, the bulk of theorists' time and energy is, in fact, often directed at the construction of approximation methods.

The Chew model also provides fertile ground for considering our stronger claim **Content Determination**. We have seen that Chew's primary modelling aim was to accommodate the empirical data on nucleon-pion scattering which was being produced in large quantities during the 50s. His new approximation method clearly played an essential role in achieving this aim, transforming a previously empirically barren Hamiltonian into a powerful predictive tool. One might try to reconcile this story with the conventional narrative about surrogate reasoning discussed in section 2: Wentzel introduced a new idealized system in 1940, and Chew's approximation method succeeded in giving us epistemic access to its empirical content in 1954. This seems to us to stretch historical facts beyond recognition. While the interaction Hamiltonian (3), was given the same basic physical interpretation throughout our story—ϕ being taken to represent the pion field, ρ the charge density, and so on—it is hard to see the empirical content of the model as being completely specified in 1940.

A fundamental impediment to such a reading is that it was not even clear how to write down a QFT's basic kinematic structures and fundamental dynamical equations during this period (see section 5 for more discussion of this point). In order to determine what the empirical content of Hamiltonian (3) was, Wentzel, Chew, and the rest of physicists involved in our case-study would have had to mobilize the apparatus of QFT to extract empirical predictions out of it. Given the serious difficulties that afflicted the foundations of QFT at the time, however, it is hard to see how this could have been done. The theorists we have been considering typically did not fret over these questions, instead making do with a fairly loose characterization of their underlying idealized system and moving on swiftly to the business of constructing an approximation. Against this background, it seems more accurate to say that both Chew and Wentzel, and the remaining researchers in the strong coupling program attempted to associate new empirical content with Hamiltonian (3), with Chew finally succeeding at providing accurate predictions in 1954.

Note also that none of the various physicists that worked with Hamiltonian (3) made any serious efforts to evaluate the merits of their preferred approximation method on independent grounds. Chew, for instance, did argue that weak coupling approximations had not been consistently implemented prior to his own work. And he also pointed out that the value of $f = 0.02$ that he obtained with his own treatment vindicated the assumption of a weak coupling between nucleons and pions. But since the Chew model was only applicable in a very limited non-relativistic domain, this result had little bearing on the appropriateness of the fully relativistic strong coupling approach. Beyond these kinds of vague indications, in any case, the main protagonists in our story made little effort to account for the success or lack of success of the approximation methods available to them—and it is hard to see what shape those efforts could have taken, given the kinds of foundational difficulties that QFT faced at the time. Instead, it was the success or lack of success of those methods in providing empirically adequate predictions that was taken as a measure of their appropriateness. Given all this, then, we find that our case-study provides evidence in support of **Content Determination**.

4 The Nambu-Jona-Lasinio Model

Our second case study concerns the development of the Nambu-Jona-Lasinio model, another model of nuclear interactions which was introduced by Yoichiro Nambu and Giovanni Jona-Lasinio in 1961. As with the Chew model, what is called the Nambu-Jona-Lasinio model was not really a novel idealized system—rather it was a new approximation method affixed to an old Hamiltonian. This Hamiltonian, in fact, has a rather interesting back story, finding its origins in Heisenberg's abortive final theory program of the 1950s. Also like the Chew case, the success of the new approximation method displaced the memory of this origin story and the connection with Heisenberg's earlier model was largely forgotten. Unlike Chew, however, Nambu's and Jona-Lasinio's primary aim in crafting their approximation was not the derivation of empirical predictions. Their model was more speculative than phenomenological in character and the contribution of their approximation method consisted of articulating the model's basic physical content by clarifying its particle content. This second case-study thus complements and extends the lessons gleaned from the Chew case.

This section is once again divided into two subsections, the first devoted to Heisenberg's Hamiltonian (4.1) and the second to the new approximation method developed by Nambu and Jona-Lasinio (4.2).

4.1 Heisenberg's Hamiltonian

The success of the Chew model revitalized research in the strong interaction by delivering some good news after almost two decades of persistent failures. As we saw in the previous section, however, the Chew model was highly phenomenological in nature, and it was only successful in a very restrictive non-relativistic domain. This left open the question of what a fundamental, fully relativistic theory of the strong interaction might look like. Although Yukawa's meson theory appeared as the most promising candidate at first, the progressive discovery of new strongly interacting particles throughout the 1950s made clear that Yukawa's theory—which only accounted for the nucleon-pion interaction—could not be the last word about it. Around the late 1950s, therefore, some theoretical efforts started to be made either to generalize the Yukawa theory to the new particles, or else to replace it with a new fundamental theory.

One of the most radically revisionist approaches considered in this period was due to Werner Heisenberg. During the 1930s, Heisenberg had believed that all matter could very well be composed of electrons and nucleons. But as more and more particles kept being discovered during the 1950s, he started preaching a new kind of reductive monism according to which all forms of matter were composed of a single fundamental field (Heisenberg 1950, Heisenberg et al 1955, Heisenberg 1957, Dürr et al 1959). More precisely, Heisenberg's idea was that all existing particles were, in reality, bound states of this newly introduced field, which was however never observed in isolation. The challenge, of course, was to develop a theory that could flesh out this idea in detail.

In order to do that, Heisenberg had to write down an interaction term characterizing the dynamics of his new fundamental field.He reached his preferred form for this term by the following line of reasoning. First, he assumed that his new field ψ was a Dirac spinor—this would make it possible, in principle, for both fermionic particles, like the electron, and bosonic particles, like the photon, to be recovered as composites of this basic field. He then imposed general constraints on the Hamiltonian describing this field, including Lorentz invariance and the following two global $U(1)$ symmetries, which were tentatively connected to the conservation laws of electric charge and baryon number:

$$\psi \to \psi e^{i\alpha} \quad \psi \to \psi e^{i\alpha\gamma_5}. \tag{7}$$

These constraints together with appeals to simplicity arguments gave rise to the following interaction term:

$$\mathcal{H}_{int} = g_0 \left[\bar{\psi}\psi\bar{\psi}\psi + \bar{\psi}\gamma_5\psi\bar{\psi}\gamma_5\psi \right]. \tag{8}$$

What Heisenberg ended up with, therefore, was a quartic spinor theory.

Fixing a form for his Hamiltonian was a small victory, however, as the resulting model immediately faced an array of difficult to answer questions. On the one hand, there were a number of foundational difficulties that made it hard to discern what was it that Heisenberg's model said about the world or, indeed, whether it said anything coherent at all. The first, obvious problem was that the interaction term (8) is non-renormalizable. This meant that the renormalization techniques developed a few years earlier could not be used to systematically remove the ultraviolet divergences appearing in its perturbation series, raising the question of whether Heisenberg's model was well behaved in this regime.

Heisenberg's reaction to this problem consisted of arguing that perturbation did not constitute an appropriate tool to spell out the physical content of his Hamiltonian. This raised a number of even thornier issues, however, as perturbative QFT was more or less the only game in town circa 1950. It was far from clear what an exact solution to a QFT model would look like, or how the dynamical equations to be solved should be set up. There was, in fact, an ongoing debate in this period about whether QED, the prime example of a successful QFT model, could even be formulated in a mathematically consistent way outside of the confines of the perturbative approximation scheme (Blum, forthcoming). Heisenberg's program quickly became intertwined with these debates about the mathematical consistency of QFT. Considering the twists and turns of this debate, described in detail in Blum (2019), would take us too far away from our central narrative, but the most important point for our purposes is clear. As a result of the difficulties that we have just described, it was very dubious that any definite physical content could be assigned to Heisenberg's model at all.

On top of these theoretical difficulties, there was the problem of connecting Heisenberg's model to the high energy phenomenology of the time. As we just saw, Heisenberg had envisioned that all the particles known at the time would emerge as bound states of the fundamental field ψ. But could Hamiltonian (8) plausibly be used to describe such a wide variety of different particles? In order to answer this question conclusively Heisenberg would have needed to solve his model exactly and find out if the dynamics of its quartic interaction term allowed for the formation of the required kinds of bound states. As we have just seen, of course, there were no hopes of Heisenberg succeeding at doing so, as it was not even clear how to formulate the relevant equations. But one could perhaps devise some sort of approximation method that told us enough about the dynamics of Heisenberg's model to answer these questions. While this would not resolve the serious foundational difficulties that we have just described, it could perhaps constitute enough of a success to show that his theory was on the right track. Even though Heisenberg could not provide a full account of the physical content of his model, that is to say, he could perhaps clarify enough of it to render the reductionist aspects of his program plausible.

In order to flesh out this strategy, Heisenberg turned to a non-perturbative approximation technique known as the Tamm-Dancoff approximation. This approach had been developed during the 1940s for the study of relativistic bound states in nuclear physics, making it a promising tool for exploring the viability of Heisenberg ambitious program. Attempting to articulate the physics content described by his Hamiltonian, Heisenberg assumed that the dynamics of his model allowed for the formation of bound states, and used the Tamm-Dancoff approximation to determine the characteristics that those entities would have, provided that they existed at all. For the time being then, he set aside the question of why his new fundamental field ψ was never observed in isolation and focused on trying to identify the existing particle zoo with bound states that could be identified via the Tamm-Dancoff method.

In the Tamm-Dancoff method, the quantum state of the bound system under investigation is represented by a state vector $|\Psi\rangle$. This bound system, of course, is understood to arise as a result of the interaction between more elementary constituents. In order to characterize the bound state as a composite system, then, an infinity of "wave functions" $\tau(x_1, ..., x_n, x_{n+1}, ..., x_{n+m})$ is introduced, each determining the probability of finding n interacting particles and m antiparticles at points $x_1, ..., x_n$ and $x_{n+1}, ..., x_{n+m}$ when probing "inside" of it. These wave functions are defined by the following set of expressions

$$\tau_{\alpha_1,...,\alpha_{n+1},...}(x_1, ..., x_{n+1}, ...) = \langle 0|T\psi_{\alpha_1}(x_1)\cdots\overline{\psi}_{\alpha_{n+1}}(x_{n+1})\cdots|\Psi\rangle\,, \tag{9}$$

where $|0\rangle$ is the vacuum state, T the time ordering operator, and the α_i with $i \in [0, n+m]$ are Dirac spinor indices.

The Tamm-Dancoff method allowed one to find approximate expressions for the wave functions τ, which could in turn be used to obtain information about the properties of the bound states. In order to do this, one needed, first of all, to act with Dirac operators on both sides of expression (9) to obtain an infinite set of coupled differential equations for the τ. This set of differential equations could be truncated by setting all wave functions with particle number greater than some fixed number to zero, this number determining the order of approximation to which the Tamm-Dancoff method was being applied. The resulting set of differential equations, having been rendered suitably finite by the application of the

Tamm-Dancoff method, could be solved numerically to obtain approximate expressions for the wave functions.

Finally, the expressions for the wave functions obtained in this manner could be used to extract information about the properties of the systems bound states. This was done by imposing different conditions on the quantum state $|\Psi\rangle$ in (9). If we require that $|\Psi\rangle$ is an energy eigenstate with eigenvalue E, for instance, it is easy to show that the following relation obtains, which resembles the time-independent Schrödinger equation:

$$\sum_{i=1}^{n+m} \frac{\partial}{\partial x_i} \tau_{\alpha_1,\ldots,\alpha_{n+1},\ldots}(x_1,\ldots,x_{n+1},\ldots) = E\ \tau_{\alpha_1,\ldots,\alpha_{n+1},\ldots}(x_1,\ldots,x_{n+1},\ldots). \tag{10}$$

The approximate expressions for the τ obtained by using the Tamm-Dancoff approximation could thus be used to estimate the mass spectrum of the bound states allowed by Heisenberg's theory. Imposing further conditions on $|\Psi\rangle$, one could obtain additional information about the bound states, including relations between their masses and spin values, and a rough estimate of the intensity with which they coupled to each other.

Using this kind of reasoning, Heisenberg was able to reproduce some general features of the particle spectrum known at the time. More precisely, he was able to infer the existence of fermionic bound states that coupled to two different kinds of bosonic states. On the one hand, there were massless vector bosons with a coupling constant that was much smaller than one and, on the other hand, there were massive bosonic states with a mass that was about ten times lighter than that of the lightest of fermions. This seemed to recover quite well the existence of fermionic nucleons that interact with pions and photons via the strong and the electromagnetic interactions. Unfortunately, Heisenberg was unable to account for weak phenomena, or even for the existence of the electron. He consequently adopted the view that Hamiltonian (8) only accounted for the strong and electromagnetic interactions of hadrons, and that the leptons and the weak interactions would arise through further refinements of the model (Ascoli and Heisenberg, 1957, 187) [10]

In spite of this, Heisenberg was quite pleased with these results, which he took to indicate that he was on the right track. In a letter that he sent to Pauli on 11 May, 1955, he wrote:

> Of course I know that the devil called "agreement with experience" can badly lead one astray; but still I just can't believe that all these surprises are mere

[10] Heisenberg intermittently touched upon the possibility that his theory might ultimately also be able to address gravitational phenomena, e.g., in a letter to Pauli of 10 February 1958. But this was never systematically pursued, a clear indication of the still marginal status of quantum gravity, even within programs to construct a final theory of microscopic physics.

coincidences. The impression they made on me were so strong that I got into the mood: "even if the axiomatics cannot be carried through without contradiction, this theory will relate to the correct one in about the same manner as Bohr's theory of atomic structure related to quantum mechanics".

The rest of the scientific community was much less impressed, however. For one thing, doubts about the mathematical consistency of his whole approach continued to fester. To make things worse, the legitimacy of the Tamm-Dancoff calculations with which he had tried to defend his reductionist program was widely questioned. Pauli, for instance, was for some time impressed with Heisenberg's Hamiltonian, but continued to be worried about the consistency of the model and never endorsed his calculations methods:

> The mathematical foundations are not clarified, and one is still calculating with the old bad methods (are those even methods?).[11]

There were at least two reasons to be dubious about Heisenberg's approximation method. On the one hand, Heisenberg had promised that the non-renormalizable divergences in his theory would be dynamically regularized somehow. In his actual Tamm-Dancoff calculations, however, Heisenberg had had to insert a regularization by hand without justification. On the other hand, the Tamm-Dancoff approximation had itself fallen from grace by the late 1950s. By 1957, indeed, most physicists agreed that, although the method could be useful as a tool to improve on perturbative calculations, it was unreliable outside of the perturbative regime. Heisenberg's use of the Tamm-Dancoff approach to support claims about the non-perturbative behaviour of his model was, therefore, viewed with a great deal of suspicion.

These difficulties proved to be fatal for Heisenberg's theory, which was soon discarded. Pauli's objections were publicly voiced in 1958 at the 8th Rochester Conference in High-Energy Physics which was widely taken to settle the matter in a decisive fashion. Heisenberg's attempts to clarify the physical content of his model using the Tamm-Dancoff approximation were unsuccessful, then, and his model quickly faded into obscurity.

4.2 Nambu's and Jona-Lasinio's Approximation Method

After 1958, interest in Heisenberg's theory waned. This is in fact where most accounts of Heisenberg's attempt to develop a theory of everything end (Blum, 2019). Yoichiro Nambu and Giovanni Jona-Lasinio, however, went on to take an active interest in Heisenberg's

[11] *Eine Klärung der mathematischen Grundlage erfolgt aber nicht, es wird nur mit den alten schlechten Methoden (sind das überhaupt Methoden?) weitergerechnet.* Letter to Heisenberg of 1 February 1958, PSC IV-IV

theory in the years between 1959 and 1961. As we shall now see, in fact, the Nambu-Jona-Lasinio model can be taken to constitute the last attempt to determine the particle content of Heisenberg's Hamiltonian.

Although we do not have a full picture of the reasons why Nambu and Jona-Lasinio turned their attention to Heisenberg's Hamiltonian, the motivations of the two physicists appear to have been different. Nambu, on the one hand, arrived at Heisenberg's Hamiltonian in his search for a simple analogue in particle physics of the many-body model equation for superconductors. In 1959, Nambu had indeed provided a reformulation of the Bardeen-Cooper-Schrieffer (BCS) model of superconductivity and realized that a central element of that model—which would eventually become known as spontaneous symmetry breaking—could be relevant for particle physics (Nambu, 1960).

Around the same time, a number of developments in the study of the weak interaction convinced physicists' of the importance of a new kind of invariance, namely chiral invariance. The advent of the $V-A$ theory of the weak force, in particular, raised the question of whether a chirally invariant model could describe massive particles (Feynman and Gell-mann, 1958). Well aware of these parallel developments, Nambu drew on his work on superconductivity to shed light on the question. His idea, as we will see in more detail below, was that a mass could be generated for an otherwise massless field via the spontaneous breaking of the chiral symmetry. Heisenberg's quartic spinor model Hamiltonian was chirally invariant, and Nambu seems to have mainly seen it as a convenient toy model to explore the possibility of generating a mass via spontaneous symmetry breaking in the context of high energy physics.

As for Jona-Lasinio, his interest in Heisenberg's theory appears to have been more sincere. Although the origins of Jona-Lasinio's interest in Heisenberg's theory are not completely clear, it is likely that they can be traced back to Heisenberg's visit to Rome in 1959, where Jona-Lasinio was a student at the time. After obtaining his degree, Jona-Lasinio was awarded a Fulbright scholarship, which he used to spend the years 1959 and 1960 at the University of Chicago (Bonolis, 2003). Nambu was a Professor at Chicago at the time, and it seems likely that Jona-Lasinio learned about the concept of spontaneous symmetry breaking from him. Like Nambu, Jona-Lasino became interested in showing that the chiral symmetry of Heisenberg's Hamiltonian could be spontaneously broken to assign a mass to the fundamental field ψ. Jona-Lasinio's reasons for wanting to do so, however, were different from Nambu's. While Nambu was interested in the process of symmetry breaking itself, Jona-Lasinio's focus was on its outcome, and on what it entailed for the kinds of entities that Heisenberg's Hamiltonian could represent. If the fundamental field ψ could be endowed with a mass, after all, then it could plausibly be identified with the nucleon. Pions, in turn, could be obtained as bound states of this fundamental field.

Jona-Lasinio's motivation for turning to Heisenberg's Hamiltonian, in sum, consisted of completing Heisenberg's project of showing that (8) could account for at least part of the particle spectrum known at the time, though starting from a different interpretation of the model's ψ field.

As we have just gestured at, the key idea behind the Nambu-Jona-Lasinio model consisted of endowing the fundamental field with a mass without explicitly breaking the chiral invariance of Heisenberg's Hamiltonian. In order to understand the logic behind the model, we first need to understand the reasons why this was not an easy task to accomplish. The first thing to realize is that, as is easily seen, explicit mass terms of the form of $m\bar{\psi}\psi$ are not themselves chirally invariant. Heisenberg's Hamiltonian is chirally invariant, and this ruled out the obvious strategy of adding a mass term by hand.

The obvious alternative to adding a mass term to the Hamiltonian directly was to devise a dynamical mechanism of some sort that would result in the attribution of a physical mass to ψ. More precisely, one could hypothesize that the coupling of the fundamental field to itself imparted a certain kind of inertia that amounted to a dynamically generated mass. Similar schemes had been envisioned since the times of Lorentz's theory of the electron, and the renormalization of QED had shown that the mass of the electron was partly due to a self-energy of this sort. On the other hand, it was by no means obvious that a dynamically generated mass would fare any better than adding a mass term by hand as far as preserving chiral invariance was concerned.

What was needed was not just a dynamical mechanism that could generate a mass, but one that could do so without explicitly breaking chiral symmetry. It was at this point that the notion of spontaneous symmetry breaking entered the scene. The idea behind the concept of spontaneous symmetry breaking is that a set of dynamical laws, operating in a strictly symmetric fashion, can give rise to an asymmetric state of affairs. In the BCS theory of superconductivity, for instance, the interactions between the electrons in a metallic lattice give rise to a certain kind of bound systems—known as Cooper pairs—the formation of which violates the conservation of electric charge. This is in spite of the fact the theory governing these interactions is strictly gauge invariant and thus conserves the total electric charge of the system. Nambu's contribution to the study of superconductivity consisted of showing that it was indeed possible for the laws of the Schrieffer-Cooper-Bardeen theory to give rise to this kind of outcome without compromising the gauge invariance of the theory itself.

Could the same be said of the mass of Heisenberg's fundamental field, and of the chiral invariance of his Hamiltonian? Could the quartic interaction described by (8), while operating in a chirally invariant manner, give rise to an outcome—massive particles—that was not itself invariant under chiral transformations? In order to answer these questions one had to acquire a detailed enough knowledge of the manner in which the fundamental fermions described by Heisenberg's Hamiltonian interacted with each other. Once again, approximation methods had to be brought to bear in order to make any progress in resolving this question, and the construction of an appropriate approximation would form the basis of Nambu and Jona-Lasinio's novel contribution.

Unfortunately, perturbation theory, which remained a central part of the high energy theorists tool kit in 1961, promised to be of little help in studying the phenomenon of spontaneous symmetry breaking. In perturbation theory, after all, each term in the expansion can be associated with a Feynman diagram whose nodes represent the exchange of one or more particles. If the interaction under examination exhibits a particular symmetry then the outcome of every one of these exchanges will also respect this symmetry. The application of any finite number of particle exchanges, therefore, will fail to produce an outcome that violates a symmetry of the interaction, meaning that no matter how many terms one calculates, perturbative approximations will obey the symmetries of the Hamiltonian. In the case of Heisenberg's model, for instance, the chirality of the initial and final state of any Feynman diagram appearing in its perturbative expansion will always be the same.

Spontaneous symmetry breaking, therefore, necessarily stood outside of the remit of perturbation theory, and called for a non-perturbative approximation method. Nambu's prior work on superconductivity had featured an approximation method that promised to be helpful in determining whether Heisenberg's model admitted of non-perturbative solutions for which the fundamental field ψ was massive. In his 1959 paper on superconductivity, indeed, Nambu had made use of the so-called Hartree-Fock method to show that the gauge invariance of the BSC model could be spontaneously broken and calculational strategies from this context were now brought to bear on Heisenberg's Hamiltonian (Nambu, 1959).

The Hartree-Fock method had originated in the late 20s in attempts to gain information about the properties of multiple interacting bound electrons in an atom, where exactly solving the Schrodinger equation was out of the question. Hartree (1928) and Fock (1930) had independently introduced a calculational method which proceeded in the following way. First, a guess was made about the outcome that resulted from the aggregation of all of the many interactions between electrons. This guess was then plugged into the equation describing individual interactions between electrons yielding a "self-consistent" expression in which the quantity to be calculated appeared both as part of the original description of the system and as part of the hypothesized effect that resulted from it. This self-consistent expression could then be solved to various orders of approximation, ideally providing an improving approximation to the exact solutions of the many-body problem.

In subsequent decades, this approximation strategy was adapted to many-body problems in a variety of fields, including condensed matter and nuclear physics. From the early 1950s on, Nambu contributed to these developments, setting out the basic calculational approach which he and Jona-Lasinio would apply to Heisenberg's quartic spinor model. Nambu essentially proposed a treatment of a generic many-body system based on the renormalization procedure that Feynman and Dyson had recently used in the context of quantum electrodynamics. This was made possible by regarding quasiparticles as analogous to the renormalized particles of QED. The bulk interactions of each particle with the many other *real* particles surrounding it were treated as if they were self-interactions making up the quasiparticle's self-energy (Gaudenzi, Forthcoming). Since however this self-energy term was in general large and untreatable with perturbative methods, Nambu turned to the Hartree-Fock approximation as an alternative calculational scheme.[12]

A few years after Nambu's (1960) insightful application of this approximation framework to the Bogolyubov quasiparticles in superconductors, Nambu and Jona-Lasinio took an analogous approach to establishing the existence of massive "quasiparticles" in Heisenberg's quartic spinor model (Nambu and Jona-Lasinio, 1961a, 1961b). Following the strategy outlined above, they assumed that the chiral symmetry of Heisenberg's Hamiltonian could be spontaneously broken and then tried to find out if this assumption was consistent with the self-interactions of the spinor field, as described by the Hamiltonian. The use of analogies between condensed matter systems and QFTs was a crucial, and at the time quite novel, element of Nambu and Jona-Lasinio's work which is discussed in detail elsewhere—see Fraser and Koberinski (2016) and Gaudenzi (Forthcoming). Here we largely put this aspect of the story to one side and focus instead on the role that the Hartree-Fock approximation played in their analysis.

The quantity that was to be calculated self-consistently in the case of Heisenberg's Hamiltonian was now, obviously, the mass. Nambu and Jona-Lasinio thus started from the assumption that the interaction generated a mass m for the fermions. Under this assumption, they then perturbatively calculated a particle's self-energy Σ. Since this self-energy was now being calculated using a nonzero mass, it was no longer identically zero from chiral symmetry, and one obtained the usual expression of renormalized perturbation theory:

$$\delta m^{(n)} = \Sigma^{(n)}. \tag{11}$$

In this expression, $\delta m^{(n)}$ represents the contribution that the self-energy of a field makes to its observed physical mass calculated to a perturbative order n, while $\Sigma^{(n)}$ represents the

[12] In generic many-body systems, this occurs simply due to the large number of interacting particles and regardless of the strength of the interparticle coupling. In cases like superconductors, where the collective interactions give rise to a non-analytic self-energy of the Bogolyubov quasiparticles, the non-analyticity constitutes an additional reason for the failure of the perturbative approximation.

sum of all Feynman diagrams with a single incoming and a single outgoing fermion line that appear up to order n. This relation obtains to all orders in perturbation theory, manifesting the key idea of perturbative renormalization, that the interactions of a quantum field give rise to a shift in the observed masses and charges characterizing the model.

In order for the initial assumption of nonzero mass not to be in conflict with chiral symmetry, none of that mass could come from a mass term in the Hamiltonian. It had to be generated entirely due to the self-energy. Nambu and Jona-Lasinio thus equated δm with the physical mass m. Consequently, they re-wrote equation (11) as:

$$m = \Sigma^{(n)}, \tag{12}$$

Equation (12) provides the self-consistent equation that Nambu and Jona-Lasinio were looking for. The reason is that the physical mass m appears on both sides of (12). On the left-hand side of the equation, m represents the *outcome* of the multiple processes of virtual scattering that the fundamental field experiences as it travels through space. On its right side, m appears in Σ, as the Feynman diagrams involved were evaluated under the original *assumption* of a nonzero mass m. The question, of course, is whether the assumption underlying the approximation method is consistent with the outcome.

The answer to this question will of course depend on the specific theory that we choose to consider. Nambu and Jona-Lasinio were interested in Heisenberg's theory and they thus went on to calculate the right-hand side of (12) with Feynman rules derived from the four-point interaction term in (8). To first order, this yields:

$$\Sigma^{(1)}(m, g_0) = -\frac{g_0 i}{2\pi^4} \int \frac{m}{p^2 + m^2 - i\epsilon} d^4p = m, \tag{13}$$

which makes the self-consistent nature of (12) explicit.

The only thing that remains to be done is determining whether (13) can actually be solved. As is easy to see, the equation admits of a trivial solution for which $m = 0$. According to this solution, the interaction of Heisenberg's fundamental field with itself fails to give rise to a mass, and one recovers the results of the usual perturbative approximation. As it turns out, however, the equation also admits of a second branch of massive solutions. To see that, one needs first of all to introduce a cut-off function $\Theta(\Lambda^2 - p^2)$ to make the integral in (13) finite:

$$1 = -\frac{g_0 i}{2\pi^4} \int \frac{d^4p}{p^2 + m^2 - i\epsilon} \Theta(\Lambda^2 - p^2), \tag{14}$$

Carrying out the integration, one obtains:

$$\frac{2\pi^2}{g_0 \Lambda^2} = 1 - \frac{m^2}{\Lambda^2} \ln\left(\frac{\Lambda^2}{m^2} + 1\right). \tag{15}$$

This expression is solved by an implicit function $m(g_0, \Lambda)$, which is non-analytic in the coupling strength g_0 and the cut-off Λ and takes a non-trivial and real value if:

$$0 < 2\pi^2/g_0\Lambda^2 < 1. \tag{16}$$

If we assume that the interaction is attractive and that therefore $g_0 > 0$ then we have that the second branch of solutions with $m > 0$ emerges for $g_0\Lambda^2 > 2\pi^2$.

To complete their analysis, Nambu and Jona-Lasinio examined the vacua associated with each one of these two solutions (13). What they found was that the vacuum associated with the massive solution, $\Omega^{(m)}$, is the one with the lowest energy and that the energy difference between the two vacua is, in fact, infinite. This allowed them to conclude that $\Omega^{(m)}$ constituted the true vacuum of the theory and that only the massive solution of (13) was physical. Having done this, they went on to argue that the fundamental field could feasibly be identified with the nucleon, with the pions being obtained as bound states. The resulting entities, furthermore, were shown to interact with each other in a manner consistent with what was known about strong interactions at the time.

While what became known as the Nambu-Jona-Lasinio model was not widely adopted as a candidate theory of strong nuclear interactions, their work was taken seriously and had an important impact on later developments. Perhaps most importantly, their results were taken to show that the phenomenon of spontaneous symmetry breaking could take place in elementary particle physics—it was not a mere peculiarity of the BCS theory of superconductivity. As we noted, Nambu's motivation all along had apparently been to demonstrate the usefulness of the concept of spontaneous symmetry breaking in high energy physics, whether the Heisenberg Hamiltonian had anything to do with real-world particle interactions or not. This insight turned out to be crucial, as the idea of generating particle masses via spontaneously broken symmetry was the basis of the Higgs mechanism, itself the cornerstone of the standard model of particle physics (Borrelli, 2015). Nambu and Jona-Lasinio's use of a novel approximation method thus paved the way for an extremely fruitful new modelling strategy in high energy physics.

Our analysis reveals another aspect of their achievement which was less recognised by their contemporaries and has also gone unnoticed by historians: it was a continuation, and a sort of conclusion, to Heisenberg's efforts to determine what the representational content of his Hamiltonian was. While Heisenberg's dreams of a monistic final theory were never resurrected, Nambu and Jona-Lasinio's work did finally convince particle physicists that his Hamiltonian could plausibly account for at least part of the particle spectrum known at the time—namely nucleons and pions. The Nambu-Jona-Lasinio model, furthermore, would eventually be recognised as an effective field theory to quantum chromodynamics (Vogl and Weise, 1991). Nambu's and Jona-Lasinio's work thus contributed to transform

Hamiltonian (8) from an object of suspicion to the basis of a respectable (though still difficult to analyse) QFT model, its connection to Heisenberg's ideas being however largely forgotten.

Returning to our general theses about approximation practice, the Nambu-Jona-Lasinio case is especially interesting with regards to **Content Determination**, but we will first touch on how it further reinforces **Distinctness**.

As with the Chew case, we have seen that what Nambu and Jona-Lasinio had really contributed was a new approximation method not a new idealized system. They took an old Hamiltonian and affixed a new procedure for calculating physical quantities from it, creating something in the process which physicists recognised as genuinely new, as evidenced by the fact that the quartic spinor model is nowadays named after them. We, again, see a pattern of different approximation techniques being coupled to a single Hamiltonian, further evidencing the claim that the design of an approximation technique that is appropriate for a particular modelling task is an autonomous activity, which does not flow from either abstract theoretical principles or the form of a particular Hamiltonian. The transfer of the Hartree-Fock approximation from many-body physics to high energy physics is an important aspect of this story—and indeed, we feel that the analogical transfer of approximations between different models is a topic that merits further attention by historians and philosophers of science. However, it would be a mistake to think of the Hartree-Fock approximation as a precisely defined algorithm for generating approximations. It is more aptly characterised as an approximation strategy, and a major achievement of Nambu and Jona-Lasinio was the adaption of this strategy to their particular model and modelling aims. As with the Chew case then, we claim that it is very difficult to make sense of the historical facts of the Nambu-Jona-Lasinio case without accepting something like **Distinctness**.

Moving on to the theme of **Content Determination**, the Nambu-Jona-Lasinio case provides an instructive corrective to common caricatures of the role that approximation methods play in scientific modelling. As was mentioned in section 2, the philosophical literature has often cast approximation as a purely formal activity, in contrast to idealization which is supposed to furnish rich representational resources capable of underwriting deep physical explanations. Furthermore, accounts of scientific representation often write as if a models content is specified all at once in an initial representational act, with approximation methods potentially playing a supporting role in deriving empirical predictions when exact solutions to dynamical equations are unavailable. The Nambu-Jona-Lasinio case shows these naive pictures to be inapplicable to the modelling practice of post-war high energy physics.[13]

[13]This conclusion also seems to be supported by related work on the Nambu-Jona-Lasinio model by

Both the Tamm-Dancoff approximation and Nambu's and Jona-Lasinio's approximation method were deployed in order to answer interpretative rather than empirical questions, namely: What is the physical content of Heisenberg's Hamiltonian? And more precisely: what are the kinds of entities that Heisenberg's Hamiltonian can be taken to describe? It seems to us to be wildly implausible to hold that the answers to these questions were already implicitly contained in Heisenberg's earlier work, only being revealed by Nambu and Jona-Lasinio's approximation method. As we saw, Heisenberg had not even settled on the basic theoretical framework he wanted to use to characterise his model's kinematic and dynamical structure. He neither stated a dynamical equation for his model nor established the existence of solutions to it. It was only the introduction of Nambu and Jona-Lasinio's approximation method which finally brought some clarity to the kind of particles which the quartic spinor model could be used to represent, after Heisenberg's unsuccessful attempt to use the Tamm-Dancoff approximation for that same purpose. We claim, therefore, that the best way to understand what is really going on in the development of the Nambu-Jona-Lasinio model is to accept something like **Content Determination**.

5 Conclusion: The Vanishing Scientific Theory?

At the close of his book on the adoption of Feynman diagram techniques, in a section titled "In Search of the Vanishing Scientific Theory", David Kaiser launches a critique of the way that philosophers of science have tended to view scientific theorising:

> When we inspect the materials with which theoretical physicists have worked, night and day, we see tinkering and appropriation of paper tools—tools fashioned, calculations made, approximations clarified, results compared with data, interpretations advanced, analogies extended to other types of calculations or phenomena, and so on. "Theories" do not appear, nor is it clear where they might be found. (Kaiser 2009, 377)

As Kaiser acknowledges, an exclusive focus on theory as the fundamental unit of analysis for the history and philosophy of science, associated especially with the logical empiricist's "received view of theories", has largely gone out of fashion. He goes on to raise doubts about whether the various model orientated philosophies that replaced the received view do a much better job of capturing the practice of postwar high energy theorising, however.

Philosophers of science have largely ignored Kaiser's complaints, likely in part because

Fraser and Koberinski (2016).

the positive view he wants to replace the prevailing philosophical orthodoxy with is not characterized very precisely. One way to read what Kaiser is proposing is that approximation, in the sense articulated in section 2, is a better category for analysing the outputs of post-war high energy physics. Instead of the axiomatic systems and set-theoretic structures that philosophers of science have focused on under the headings 'theory' and 'model', Kaiser seems to see the field as a complex network of approximation methods and calculational tricks. The most radical version of this view, which takes the traditional concepts of theory and model to be completely inapplicable to the constructs of modern theoretical physics, is most likely too radical, in our view. Still, Kaiser does seem to have a point here, which is further bolstered by our examination of the Chew and Nambu-Jona-Lasinio models: the crafting of approximations dominates the work of the modern theoretical physics, yet philosophical approaches to scientific modelling have had little to say about the subject.

We take our **Distinctness** and **Content Determination** theses to be a more measured way of developing the spirit of Kaiser's critique. To reiterate:

> **Distinctness:** Approximations need to be recognised as a distinct category of theoretical output from idealized systems, which jointly play a role in the modelling of real-world systems. The practice of their construction also needs to be recognised as distinct from the practice of constructing idealized systems.
>
> **Content Determination:** Coupling an approximation to an idealized system is, in at least some cases, instrumental in assigning determinate empirical and physical content to it.

We conclude this paper by offering some final reflections on these theses, drawing further inspiration from Kaiser's discussion.

Distinctness does not require us to abandon the concepts of theory and model, it merely advises us to add a new concept to our historico-philosophical tool kit. It is thus a good deal weaker than the most radical reading of Kaiser's claims about the "vanishing" of scientific theory. We see it as a crucial first step, however, establishing the role of approximation methods in scientific modelling practice as a topic that stands in need of further analysis. Indeed, one of the most important consequences of **Distinctness** is that it allows us to pose a gamut of further questions that would not otherwise arise. To mention two that we have only touched on obliquely thus far:[14] philosophers of science have focused on the confirmation of theories and theoretical claims, but how are the epistemic credentials of approximation methods established—that is how do scientists go about determining

[14]Both of these issues feature centrally in the stories of the Chew and Nambu-Jona-Lasinio models and our case studies may well provide a good starting point for honing potential answers to these questions.

when an approximation method can be trusted and when it cannot? Similarly, debate surrounding the role of analogy in science has focused on the transfer of propositional claims between different domains; can we give an account of how approximation methods are analogically transferred between models?[15] **Distinctness** can thus be understood as an opening gambit in an expansive research program that responds to the neglect of approximations driving Kaiser's critique. As was mentioned in the introduction, recent work in the philosophy of science has already started to contribute to this project in various ways (Batterman, 2000, 2002, 2006; Wilson 2017; Fillion, 2021; James Fraser, 2020; Koberinski 2021, Miller 2021): we see **Distinctness** as a foundation and rallying point for further work in this direction.

Recognising approximation construction as a distinct stream of theoretical development also opens up new historical lines of investigation. An interesting question suggested by **Distinctness** is whether, and how, the role of approximation in scientific modelling practice has changed throughout the history of mathematized science. This brings us to another theme of Kaiser's discussion: the idea that something important has changed in the modelling strategies of post-war high energy physics, with approximation methods coming to play a more fundamental role than had previously been the case. Addressing this question responsibly would require a detailed survey of the mathematical modelling strategies which have been employed throughout the history of physics, a massive undertaking. Here we offer some speculative thoughts on this topic, which hopefully shed further light on our stronger, and perhaps more controversial, claim **Content Determination**.

Contra Kaiser, we do not think that the concept of scientific theory is completely inapplicable to the post-war development of high energy physics. In fact, in the same time period as the Chew and Nambu-Jona-Lasinio models were being developed, sustained discussions about how QFT should be characterised as an abstract scientific theory were taking place in the work of figures like Rudolf Haag and Arthur Wightman—the approach which would come to be called axiomatic QFT. It is not so much that theory talk vanishes in the post-war period then; part of what is going on is that these questions are being delegated to a new sub-community of mathematical physicists.[16]

During the 50s and 60s the gap between this nascent axiomatic tradition and the phenomenological orientated wing of the discipline, which largely bracketed questions about high theory, continued to widen, however. Theorists like Chew, Nambu and Jona-Lasinio did employ abstract theoretical principles of various kinds in their modelling practice,

[15] There are connections to be explored here between our analysis of the Nambu-Jona-Lasinio model and Doreen Fraser's work on formal analogies between QFTs and condensed matter systems (Fraser, 2020).

[16] It is interesting to note that mathematical physics itself was emerging as a disciplinary identity around this time, a broader development which is likely an important part of this story.

but flexibly and unsystematically.[17] And as the role of theory in high-energy modelling practice became more attenuated so too did the characterization of idealized systems. There was very little clarity in this period, for instance, about what a QFT system really was—what mathematical structures were needed to characterize its kinematics and how to implement its dynamics. Amongst those physicists who cared about these questions, there was debate in the 50s and 60s about whether quantum electrodynamics, or indeed any QFT, could even be defined in a mathematically consistent way (Blum, forthcoming). Eventually, an offshoot of the axiomatic QFT tradition known as constructive QFT would emerge which focused specifically on the problem of constructing QFT systems with full mathematical rigour. To this day most of the key questions in this field pertaining to empirically supported QFTs remain open.

It is this background context that makes it implausible to hold that Chew, Nambu and Jona-Lasinio merely gave us epistemic access to claims about the world that were already contained within the strong coupling and Heisenberg Hamiltonians. It is not just that high energy theorists did not know how to solve the dynamical equations posited by their models, in most cases they did not really know how to precisely formulate the problem. This suggests the following story; as the question of how to associate mathematical structures with theories and particular systems became increasingly intractable in the post-war period, approximation methods stepped in to fill the void, allowing representational content to be associated with a Hamiltonian without explicitly solving these problems. This could be one way of spelling out Kaiser's idea that there is an important shift in the mode of post-war high energy theorising.[18] On this view, **Content Determination** can be read as describing a new, more fundamental, role for approximations that comes to the fore in this period and area of physics.

We freely admit that this historical picture is both impressionistic and speculative. It will likely have to be modified substantially in response to more detailed investigation of the historical development of high energy physics modelling practice. Furthermore, on taking

[17]Koberinski (2019) makes similar claims about the pragmatic use of general theoretical principles in high energy physics in the 50s and 60s, emphasising the extent to which experimental discoveries outstripped the scope of established theories during this period.

[18]Kaiser (2009) examines the uptake of Feynman diagram methods and his account thus primarily documents the rise to dominance of renormalized perturbation theory. Our discussion in this paper can thus be read as extending the "vanishing scientific theory" narrative into the 50s and 60s, where, as we have seen, theorists were attempting to move beyond perturbation theory in various ways. Having said this, Kaiser emphasises in his book that Feynman diagrams were not exclusively associated with perturbative calculations and our discussion of the Chew and Nambu-Jona-Lasinio models can be read as supporting this point. Diagramatic calculations continue to play a role in both of their approximation methods. In other words, they created something new using raw materials drawn from the perturbative tradition rather than starting from a completely blank slate.

a broader view of the history of physics, it may turn out that **Content Determination** is widely applicable in many other periods and disciplines, and does not mark out post-war high energy physics as special. Indeed, the work of Mark Wilson and Robert Batterman on approximations in classical physics might be taken to support asserting something like **Content Determination** across much larger swathes of applied mathematics. We hope that these more provocative concluding remarks will stimulate future discussion and debate about the role of approximation methods throughout the history of physics.

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