On Change and Constraint

Kyle Singh[†]

[†]Harvard University, Cambridge, MA

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ABSTRACT

Traditional arguments against or in favor of continuity rely upon the presupposition that scientific theories can serve as markers of descriptive truth. I argue that such a notion of the term is misguided if we are concerned with the question of how our scientific schemes ought to *develop*. Instead, a reconstruction of the term involves identifying those concepts which guide the development from one successive scheme to the next and labelling those concepts with the status that they are continuous. I explicitly construct an example of this kind of continuity utilizing two formulations of Quantum Field Theory (QFT) and identify what persists from the standard formulation, beginning with an action, to the successive one, making use of spinor helicity variables. Three concepts persist which are responsible for supplying explicit constraints on our expressions which serve to match onto empirical predictions: Lorentz invariance, locality and unitarity. Further extensions of this kind of analysis to models beyond the physical sciences are proposed.

KEYWORDS
Scientific Realism/Anti-realism; Theory Change; Scattering Amplitudes; Conceptual Engineering

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CONTACT: kas0502@g.harvard.edu

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1. Persistence as a Marker of Truth?

Persistence in light of successive theory change has often been thought to hinge upon a pre-ordainment of truth on behalf of scientific schemes. For the realist, persistence is equated to the preservation of truth, as an indicator that scientific schema are all working towards an explanation of the world, and its mechanisms, as they actually are. Likewise, the anti-realist position only gets off the ground if it is centered around the truth content of our scientific theories, namely that their is not sufficient warrant for belief in such schemes beyond their observational implications.

The distinction, as posed, rests on a notion of truth which is *descriptive* in nature. When Psillos states that the anti-realist denies the truth-likeness of a given theory because ”the entities they posited are no longer believed to exist and/or because the laws and mechanisms they postulated are not part of our current theoretical description of the world”, we can ask what kind of theoretical description is to be sought out in the first place (Psillos 1999: 97). In this case, Psillos presents a notion of continuity which is ontological.

What consequence would such a notion, argued in favor of or against, have for the practicing scientist? The first inclination for some to say would be none. Indeed, the instrumentalist that wishes to focus on the utility of any given theory takes on this tact. Moreover, it is certainly the case that even if one could somehow decisively show, for example, that at-bottom reality is tenable *viz* our best current theories, their would be no doubt, given the current state of our affairs, that the scientist would go on with their practices. One might immediately object that this premise is simply begging the question with regards to the value of metaphysical questioning when it comes to the philosophy of science. However, since science is developmental and given that this development is predicated on practice, drawing a clear link between what persists and how the scientist views this persistence is one way philosophical inquiry can latch on to pragmatic advancement. If it is not already apparent, I will argue first that such a notion of continuity would certainly not be of the ontological sort, nor would it rest upon any prior distinction of realism and anti-realism. Instead, to fulfill the function outlined above, it would need to serve as a guide for development rather than act as an account for past theory change. I want to stress that this takes us beyond mere instrumentalism, for questions of development are inherently philosophical ones.

Before I construct this idea of continuity with greater precision, let me begin by considering various arguments constructed on the backdrop of descriptive truth and attempt to illustrate how the lines of reasoning in each example fall short of yielding markers for the scientist to act upon. In particular, assessing various positions in favor of continuity, which for the moment pose as *de facto* realist positions, present the traditional account with greater clarity.

1.1. Traditional Accounts of Continuity

Many contemporary advocates of persistence wage their claims in response to Laudan’s well known pessimistic meta-induction. One of the upshots of Lauden’s argument is to note that empirical adequacy and truth-likeness are not connected. This is due to the notion that although our past theories can be taken to be false given that any current given theory can be taken to be true, those false theories were, nonetheless, successful in making empirical predictions [1] (Laudan, 1981). In making this claim, Laudan relies heavily upon the history of science and famously point to anomalies in the historical record which act as counterexamples to the realists intuition by presenting an explicit list

[1] I rely on Psillos’ reconstruction of Lauden’s argument here, and construction of an explicit pessimistic induction, to spell out the objectionable parts of the argument at stake (Psillos, 1996)

of theories that can now be taken to be completely false.

The strategy to *Divide et impera* is perhaps one of the central realist responses to the pessimistic induction. In particular, the tripartite of Kitcher, Leplin and Psillos (1993, 1997, 1999) have argued that even false theories permit approximate truth in ways that are conducive to the realists cause. For the three of them, Fresnel, for example, could derive his empirical predictions without referring to the concept of the ether. This implies that what carries on is a network of theory, a network of working posits which must carry over even in time of scientific revolution. Moreover, these can be seen as the best description of the world at a given moment. Regardless of ones attitude towards the descriptive content of this strategy, there is a practical concern rooted within those posits that are held onto. Putting aside the question of truth, it is reasonable to suggest that scientists build onto pre existing frameworks and, even more strongly, that this must necessarily be the case, although phrasing the situation in this way deflates the realists stakes.

Indeed, other notions of continuity on the realist front take on a similar tact. Let us go further back in the tradition and take into account Duhem's thesis that "by virtue of a continuous tradition, each theory passes on to the one that follows it a share of the *natural classification* it was able to construct" [Emphasis mine] (Duhem, [1914] 1982: 32). For a theory to tend towards a natural classification means that it gets us closer to describing real relations between things. Duhem presents the belief that science gives us the correct descriptive picture of the world as one that scientists take on faith, with a fair degree of conviction to continue their practice. This implies that Duhem would be in favor of substantive continuity. The specific characteristics of what carries on from theory to theory, for Duhem, rest on his distinction between the representative part and explanatory part of any given scientific scheme. What is retained is the representative part which contains both the various empirical laws and the mathematical formalism accompanying those laws. It is the explanatory part, for him, which reveals the underlying unobservable causes of various phenomena which are left behind. What is clear here is that the very idea that scientific schemes should converge upon the designation of a natural classification at all depends upon Duhem's background concern that continuity explicitly has something to do with truth, for it is the idea of transition to a classification of this sort which provides his criteria for continuity to begin with. Although, Duhem's lines of reasoning predate the current debate, it is illustrative to see how the genealogy of this term has been rooted in presupposition that the question of continuity is one that should ask whether or not scientific theories can serve as markers of truth.

Duhem's distinction is an interesting one nonetheless, in that his notion of what persists, for the practicing scientist, can almost be seen as trivial, if not always straightforward to identify. In this sense it shares substantive commonality with the views of Kitcher, Leplin, and Psillos since the emphasis here is placed upon what is required for the practitioner to construct testable predictions. This is the thread which connects the realist position in light of explicit epistemic limitation and is the intuition which I will take further in §2. First, let me sketch out more recent proposals which take this consideration into greater account.

1.2. Alternative Notions on the Pragmatic Track

There have been some current efforts to construct a notion of persistence that is consistent with pragmatic concerns. A prime example of this is the so-called Theoretical Physics Realism of Gabovich and Kusnetsov. in which they argue that any given current theory is, at the level of concepts which the scientist uses, is continuous with past theories. For example, they write that "many quantum attributes are like corresponding classical attributes" which is certainly the case when one considers, for example the role of Poisson brackets in classical physics, utilized in the Hamiltonian formalism, which played a crucial role in the development of early quantum theory, namely in the construction of the canonical commutation relation (Gabovich and Kusnetsov 2023:53). The emphasis on the reconstruction of physical theory is the distinguishing marker here, for in order to substantiate such a claim, identifying certain classical attributes with quantum ones, one needs to look at the function of the given concepts in question and and classify them given particular functional criteria.

Indeed, the function of physical theory is what Dieks considers when he makes the related claim that ”continuity between theories is essential for preserving empirical success” (Dieks 2023: 2). However, as he aptly notes this does not yield ontological continuity of which the realist desires. In this sense, he sees continuity between what are even usually considered to be disparate schemes. As it relates to Newton and Aristotle's formulation of mechanics he writes that ”even though Aristotle's and Newton's mechanics possess very different structures... they must agree at least approximately on certain predictions” (Dieks 2023, 5). The criteria of empirical success, along with the simple idea that current theories should retain the correct predictions of past ones is enough for Dieks to take this stance.

Prima facie, it seems that one can accept these notions of continuity while remaining agnostic to either the realist or anti-realist camp. A straightforward example of this is the fact that, for example, one can recover the Newton-Poisson equation from general relativity in the weak field approximation. In this case, Newton's theory can be seen as an *effective description* which is only recovered when one assumes that they are working with test particles moving at very slow velocities; it is in this sense that Newton's theory can be seen as being contained in Einstein's. Moreover, many concepts carry over from one formulation to the next chief among them that both are written as classical field theories.

Although these criterion for what persists take us away from mere ontological continuity, there is a sense in which these notions of persistence are trivial. The chief demand of continuous concepts I will argue in favor for is that they serve as a guide for future development. In the case of Gabovich and Kusnetsov, we are identifying concepts which persist retroactively. In the case of Dieks, it is hard to see what, if anything, Newton could have gleaned from Aristotle as he developed his theory in real time. Perhaps more strongly, I will argue that a guide for development needs to be forward looking if it is to be of true consequence to the scientist. Before I spell out this criteria in more detail and sketch an explicit analysis of this, let me begin the next section by framing my construction in a broader metaphysical context.

2. Continuity as a Guide for Development

2.1. Functionalism With Respect to Theoretical Terms

In order to set a ground upon which I can present my notion of persistence, a proper meta-metaphysical background must be established. As I have alluded to until this point, the tenability of deep worldly truths on behalf of metaphysics is a starting point that does not allow for an analysis that can serve the practitioner much use. Underpinning this impulse is what Thomasson refers to as functional monism, which starts with the assumption that our terms are to serve as world-tracking. Only terms which are ”*supposed to serve* the function of tracking and picking out the Fs” should be thought of as such. Here F refers to a given concept or term [Emphasis original] (Thomasson *Forthcoming*: 119). For us, the relevant F to be picked out is some notion of descriptive truth. As Thomasson notes, it is a mistake to think that all of our terms serve this function.

Instead, Thomasson constructs a notion of *pragmatic* conceptual engineering in which one is to make an appeal to the function of a given concept and is to then assess how it stands in relation to prior instantiations of the scheme. In light of the newly developed, or what is in the process of being developed, scheme, we can ask what should be revised, retained or even outright rejected (Thomasson, *Forthcoming*). This two step procedure builds naturally upon the desire to identify continuities which serve as a guide for development.

This is where it is apt to ask what the function of our theoretical terms and concepts are. Theoretical terms roughly feature two functions for our purposes. The first is that they allow us to make testable predictions. The second is that they explicitly place constraints on the structure our expressions can take. Let us consider the theoretical term ”spin”. Stipulating that the electron is spin 1/2 implies a wide range of testable predictions in a given Stern-Gerlach experiment. It also places explicit constraints on how a particular electron state, ψ, can be configured and written down. Furthermore, this leads to a whole set of rules which determine how specific states can be constructed for various physical systems. In this case, the function that ”spin” plays is not to track a worldly at-bottom feature of a given electrons description, but rather serves as a basis upon which theoretical deductions can be made.

I should be careful here and make a further distinction due to the fact that the property of spin is such that we can describe it as a measured quantity of an electron. We can, for example write down an observation sentence which states that ”the electron has spin 1/2”. Can the same be said of any theoretical term, namely that it can be translated in such a fashion? I would claim that they can, for there seems to be no reason to distinguish between theoretical statements and observational ones if we view the function of theoretical terms as spelled out. One could say, perhaps rather naively, that we do not ”measure locality”. However, this would be besides the point since, as I have already identified, locality is a term which allows us to predict the outcomes of dynamics within a physical system and constrains the set of expressions which make that system up; in this case, the canonical commutation relations come to mind in QFT, $[(\phi)_a(\vec{x}), \pi^b(\vec{y})] = i\delta^{(3)}(\vec{x}-\vec{y})\delta^a_b$, which ensure that causality is preserved. In either case, a line can be drawn between a theoretical term and a corresponding observational sentence in this way.

With this methodological backdrop, I will now spell out how non-trivial continuities can be identified.

2.2. *Reconstructing the Concept and a Procedure for Identifying Its Constitutive Parts*

Ultimately, I would wage that most attempts to clarify the nature of what persists involve projects which are *backward looking*, making appeals to structural patterns found throughout the history of science. At the same time, any current theory is both implicitly and explicitly an endorsement of past theories. In order to identify a given continuous concept in this framework, I stipulate that we begin with the current theory and attempt to reconstruct it with and without certain theoretical terms and see if an identical empirical structure can be attained.

I can begin with a current scheme, T, and call the scheme I am constructing T'. We can think of the maps from T to T' as being injective, bijective or surjective. In any construction where $T \equiv T'$, this map is trivially bijective. We can consider removing one of the elements in T. If this is done, we are immediately working with a new theory T'. Once this map is established, a further question needs to be asked, namely whether or not all of the relevant predictions, or empirical facts can be recreated by T'. In this simple case, where one element is removed from the original theory and nothing is added to the new one, it is clear that T' will not recreate the space of all empirically testable predictions, let us call this space E. Elements will need to be added to T' which do not map onto T if T' is to map onto E just as the original theory does.

An analysis of this kind is one by which we can identify suitable continuities. After iteratively forming new T'’s, one is left with a set of elements which map from the original theory and are not replaced. The suitable continuities we will be left with correspond to the bare minimum set of elements required to construct a theory that has the potential to map onto E.

What we are left with is what we cannot do without and is, as outlined, the bare elements of what is required to formulate our conceptual scheme. Are our assumptions simply what are under scrutiny as a result of this process? In a certain sense, they are since we are seeking to build consistent frameworks whilst carrying over the least amount of constraint. Commitment to a particular assumption here is not a feature of that assumption serving as a descriptive mechanism, rather as a tool to find the best operational theory. Any assumption then can be mechanically removed, or toggled, in exactly the way spelled out here. [2]

This set is crucial to identify in the development of any conceptual scheme. When we ask ourselves how many elements we can replace from T to obtain a consistent T', we are indirectly making a statement about how much structure in T is excess or redundant, for whatever is retained is straightforwardly what we cannot do without. If the map from T to T' is really close, for example, than T can be seen as being extended instead of replaced. In other words, the set of continuities is large enough from one theory to the next such that there is no succession. The main point here is that, in carrying out this procedure, we always need to reduce the set of elements which map from the prior theory to the current one as much as possible. Moreover, a non trivial succession must include the addition of at least one new concept into T' if it is to have any chance of reproducing E.

A basic objection one may have is to state that such an account of continuity can only be had *locally*, that is to say, that it is unable to look at the long term and be able to speculate on its unfolding. At a basic level, such an objection does not hold much weight given that the regress of speculation can carry on. However, in the regime where we are actively developing a pre-existing scheme, this speculation becomes less speculative since we have a threshold for what a successive scheme needs to reproduce predicated on the function of any additional, or replaced, concept. The alternative to this, to move to a sufficiently global claim, would take us back to the distinction I have sought out to undermine here. This restricts our analysis to a given scheme in the current moment and forces us to work with the details of it.

A second objection one can levy is with respect to the criteria under which, even in this pragmatic framework, concepts can be tagged as continuous. If, for example, we constrain ourselves to the functions for theoretical terms and concepts stated above, haven't we resigned ourselves to a certain amount of inevitable continuity? This is certainly true, although there is a sense in which certain persistent concepts are trivial whereas others are not. The former would inhabit the most general class of mathematical concepts, numbers and the like. It is simply unimaginable to expect a contemporary scientific theory to be without numbers. Any non-trivial aspect of a given scheme which one purports to carry on in successive iterations would need to be the kind of concept which could be toggled at will. In other words, we would need to see whether we could formulate a consistent version of that scheme by removing that concept from it.

This gives us the degree to which our identification is fine-grained that will become clear by carrying out the following example.

3. Intra-Theoretical Traveling *viz* On-Shell Amplitudes

3.1. Feynman Diagrammatics vs. Spinor Helicity Variables

The last several decades have featured a renaissance with respect to how standard perturbative calculations of the $\mathcal{S}$- matrix are to be thought of in Quantum Field Theory (QFT). Traditionally, gauge symmetry has played the central role in constraining allowable interactions described by the

[2] I should be careful here an clarify what I mean by assumption. Often times, in scientific practice, there is a tendency to equate an assumption with an approximation or corresponding idealization. However, it should be clear that such assumptions are not what I am concerned with here since approximations seem to be a ubiquitous process undertaken by all model builders. Therefore, it would be strange to identify a particular approximation as a suitable continuity of the kind I have outlined. Roughly, I think it is safe to equate a particular assumption with a corresponding principle. More precisely I equate the term with a translation onto an explicit constraint on the formalism that is stronger than mere estimation.

standard model (SM) and particles, which can be described as irreducible representations of the Poincaré group, are to be thought of as fluctuations of quantum fields. Alternatives to the field theoretic picture are being sought out for several reasons. One of these, that has led to recent development, is that the standard picture requires the addition of extra unphysical degrees of freedom. The simplest example of this is the fact that the photon must be described by a four-component Lorentz vector field despite having only two physical, polarizations, degrees of freedom. Rectifying this requires one to select a gauge condition, $\partial_\mu A^\mu = 0$, to maintain Lorentz invariance. This kind of redundancy has long been identified to be a main source of complication when one tries to compute various Feynman diagrams. [3] Let us consider the following general Lagrangian for scalar field theory

$$\mathcal{L} = K(\phi)\partial_\mu\phi\partial^\mu\phi \tag{1}$$

where $K(\phi) = 1+\lambda_1\phi+\frac{1}{2!}\lambda_2\phi^2+\frac{1}{3!}\lambda_2\phi^3+\ldots$. Suffice to say that this would lead to an arbitrarily complicated $\mathcal{S}$-matrix. If one begins to compute amplitudes in this theory, they will find the expressions simply vanish. For example, the four particle amplitude is one such case that is straightforward to calculate

$$\mathcal{A}_4 \propto \sum_{i\neq j} p_i p_j \propto s+t+u = 0 \tag{2}$$

where s, t, and u are the usual Mandelstam invariants. Here, two conditions had to be satisfied. The first is that the total momentum needed to be conserved, $\sum_i p_i = 0$. The second is that the on-shell condition, $p_i^2 = 0$ is held. Indeed, the calculation of many amplitudes in this theory leads to similar results. [4] We can add field redefinitions *ad nauseam* to our action which explicitly describe the same physics. Moreover, this formalism, given its unnecessary complexity, conceals any underlying structures of the QFT that we want to identify.

This difficulty sets the backdrop for the on-shell approach to constructing scattering amplitudes. The starting point is to recast kinematic data, typically characterized by p_μ in terms of another set of variables with the hope of simplifying our calculations and identifying persistent constraints in these alternative formulations. This is where the spinor helicity formalism comes into play in which the components of our momentum four-vector are mapped as follows

$$p_{\alpha\dot{\alpha}} = p_\mu \sigma^\mu_{\alpha\dot{\alpha}} = \begin{pmatrix} p_0+p_3 & p_1-ip_2 \\ p_1+ip_2 & p_0-p_3 \end{pmatrix} \tag{3}$$

Here $\sigma^\mu = (1, \vec{\sigma}$ contains the Pauli matrices. From this, we can only construct one Lorentz invariant quantity

$$\text{det}p = -p^\mu p_\mu = m^2 \tag{4}$$

In the section which follows, and for now, we work in the high energy scattering limit in which the fermion mass goes to zero. Now, $p_{\alpha\dot{\alpha}}$ can be written as the outer product of of two two component

[3] I should be careful here and distinguish between the function of a given physical theory and the theoretical virtues a given physicist wishes to impose on a given construction. From the concerns I have listed here, one make argue that the desire to remove unphysical degrees of freedom in a successive formulation is to impose a virtue of simplicity on a given scheme. Simplicity, by my estimation, should be seen here as a practical virtue given that a simpler theory is easier to calculate with. What stands is that regardless of one's personal aesthetic, the requirement that any successive scheme give us the correct predestines, or match on to pre-existing ones if is to be considered viable at all, is one all theory constructors will agree on.

[4] It may be a good exercise to show that this is the case for the 14 particle amplitude, although it would involve the calculation of close to 5 trillion diagrams!

objects referred to as spinors

$$p_{\alpha\dot{\alpha}} = \lambda_\alpha \tilde{\lambda}_{\dot{\alpha}} \tag{5}$$

The two spinors are referred to a holomorphic and anti holomorphic given their differing explicit transformations under the Lorentz group. We can now write the following expressions for two particles i and j

$$\langle ij \rangle = \lambda_{i\alpha} \lambda_{j\beta} \epsilon^{\alpha\beta} \tag{6a}$$

$$[ij] = \tilde{\lambda}_{i\dot{\alpha}} \tilde{\lambda}_{j\dot{\beta}} \epsilon^{\dot{\alpha}\dot{\beta}} \tag{6b}$$

where ϵ is the 2 index Levi-Civita symbol. this gives us a way to write any function of kinematic data utilizing these so-called "angle" and "square" brackets. Shifting variables in this way leads to an immense simplification of standard calculations in QFT. Perhaps the most well known example of such simplification is expressed by the Parke-Taylor formula which takes 220 diagrams for six particle gluon scattering and, for maximal helicity violating helicity configurations which feature two negative helicity gluons with the rest positive, and reduces the amplitude to the following simple expression (Parke and Taylor 1986)

$$\mathcal{A}(...i^- ...j^- ...) = \frac{\langle ij \rangle^4}{\langle 12 \rangle \langle 23 \rangle ... \langle n1 \rangle} \tag{7}$$

At this point, a natural question to ask is what are the constraints which fix the form of the amplitude. Consider the following ansatz for for 3 particle amplitudes

$$A_3(1^{h_1} 2^{h_2} 3^{h_3}) = c \langle 12 \rangle^{x_{12}} \langle 13 \rangle^{x_{13}} \langle 23 \rangle^{x_{23}} \tag{8}$$

Under little group scaling, under which $|p\rangle \to t\,|p\rangle$ and $|p] \to t^{-1}|p]$, on-shell amplitudes transform in the following way, with helicity h_i

$$A_n(|1\rangle, |1], h_1, ..., t_i\,|i\rangle, t_i^{-1}|i], h_i, ...) = t_i^{-2h_i} A_n(...|i\rangle, |i], h_i ...) \tag{9}$$

This fixes the following

$$-2h_1 = x_{12} + x_{13} \tag{10a}$$

$$-2h_2 = x_{12} + x_{23} \tag{10b}$$

$$-2h_3 = x_{12} + x_{33} \tag{10c}$$

whereupon one can solve the system of equations and rewrite the ansatz as follows

$$A_3(1^{h_1} 2^{h_2} 3^{h_3}) = c \langle 12 \rangle^{h_3 - h_1 - h_2} \langle 13 \rangle^{h_2 - h_1 - h_3} \langle 23 \rangle^{h_1 - h_2 - h_3} \tag{11}$$

Now, we can consider a 3-gluon amplitude with the following helicity configuration

$$A_3(g_1^- g_2^- g_3^+) = g_{\rm YM}\frac{\langle 12\rangle^3}{\langle 12\rangle\langle 23\rangle} \tag{12}$$

where $g_{\rm YM}$ is the Yang-Mills coupling. Little group scaling fixes the form of the amplitude. Moreover, the amplitude is fixed by locality, namely that it is compatible with a term of the form $AA\partial A$ in the Lagrangian $\mathrm{Tr}F_{\mu\nu}F^{\mu\nu}$ and not a term that goes like $g'AA\frac{\partial}{\Box}A$ since both angle and square brackets have mass dimension 1. Therefore, given the scaling properties of the angle brackets under a little group transformation, the momentum dependence is $(mass)^1$.

In this case, we can now spell out the two constraints which are required to uniquely fix the form of the amplitudes in question: **locality** and **little group scaling**. Do these characteristics carry over the standard formulation? Indeed, they do. Little group scaling is directly related to the notion that we wish to maintain Lorentz invariance, $\phi'(x) = \phi(\Lambda^{-1}x)$, since the little group is the subgroup of the Lorentz group which leaves momentum invariant. Locality, on the other hand, is simply manifest in the dynamics of a physical system that depends on the local, and not global, behavior of the fields.

This sketch carries out the analysis spelled out in §2. The move from momentum four-vectors to spinor helicity variables to discuss kinematics constitutes the addition of an additional element added to T', where T' is the new formulation of our theory. Locality and little group scaling are the theoretical terms, concepts which persist. Moreover, the two terms function as we have stipulated in §2.1. They both constrain the form of the amplitude and allow us to obtain the correct structure of kinematic data.

We will see that an additional constraint is required to extend these results to include and reproduce the structure of the Standard Model. First, I will work out an example of how to spinor helicity formalism can reproduce differential cross sections, which are the observables in QFT related to scattering amplitudes, concerning particle processes due to electromagnetic interactions.

3.2. A One-To-One Map Between Cross Sections in QED

Let me illustrate a simple example of this procedure in the context of basic Quantum Electrodynamics (QED) and show how one can obtain the same differential cross section in the standard perturbative approach and, instead by utilizing the spinor-helicity formalism. I will focus on $e^-e^+ \to e^-e^+$ scattering, also referred to as Bhabha scattering in any standard treatment of the subject. Although this is an elementary process but illustrative of explicitly carrying out the process, outlined in §2, by which we can identify non-trivial continuities. The differential cross section, in the center of mass frame, can be written as follows

$$\left(\frac{d\sigma}{d\Omega}\right)_{CM} = \frac{m_1^2}{(2\pi)^2(E_1+E_2)^2}\frac{|\vec{p}_1'|}{[\vec{p}_1]}\left(\frac{1}{2}\right)^2\sum_{S_i,S_f}[\mathcal{M}_{fi}]^2 \tag{13}$$

For this process, there are two relevant Feynman diagrams which contribute to the amplitude at tree level. Applying the Feynman rules for QED, the s-channel and t-channel contributions are expressed as

$$\mathcal{M}_{fi}^1 = -ie^2\bar{v}(p_2)\gamma^\nu v(p_4)\frac{\eta_{\mu\nu}}{(p_3-p_1)^2}\bar{u}(p_3)\gamma^\mu u(p_1) \tag{14a}$$

$$\mathcal{M}^2_{fi} = ie^2\bar{u}(p_3)\gamma^\nu v(p_4)\frac{\eta_{\mu\nu}}{(p_1+p_2)^2}\bar{v}(p_2)\gamma^\mu u(p_1) \tag{14b}$$

The total amplitude is simply $\mathcal{M}_{fi} = \mathcal{M}^1_{fi} + \mathcal{M}^2_{fi}$. Squaring the above result leads to the following expression

$$\begin{aligned}\left(\frac{1}{2}\right)^2 \sum_{S_i,S_f} [\mathcal{M}_{fi}]^2 = \frac{e^4}{4(p_3-p_1)^4}\mathrm{Tr}\left[\gamma^\mu \frac{\not p_4 - m}{2m}\gamma^\nu \frac{\not p_2 - m}{2m}\right]\mathrm{Tr}\left[\gamma_\mu \frac{\not p_1 + m}{2m}\gamma_\nu \frac{\not p_3 + m}{2m}\right] - \\ \frac{e^4}{4(p_1+p_2)^2(p_3-p_1)^2}\mathrm{Tr}\left[\gamma^\mu \frac{\not p_4 - m}{2m}\gamma^\nu \frac{\not p_2 - m}{2m}\gamma_\mu \frac{\not p_1 + m}{2m}\gamma_\nu \frac{\not p_3 + m}{2m}\right] - \\ \frac{e^4}{4(p_3-p_1)^2(p_1+p_2)^2}\mathrm{Tr}\left[\gamma^\mu \frac{\not p_4 - m}{2m}\gamma^\nu \frac{\not p_3 - m}{2m}\gamma_\mu \frac{\not p_1 + m}{2m}\gamma_\nu \frac{\not p_2 - m}{2m}\right] + \\ \frac{e^4}{4(p_1+p_2)^4}\mathrm{Tr}\left[\gamma^\mu \frac{\not p_4 - m}{2m}\gamma^\nu \frac{\not p_3 - m}{2m}\right]\mathrm{Tr}\left[\gamma_\mu \frac{\not p_1 + m}{2m}\gamma_\nu \frac{\not p_2 - m}{2m}\right]\end{aligned} \tag{15}$$

where the standard slash notation, $\not\partial = \gamma_\mu \partial^\mu$, is employed. A straightforward evaluation of the traces leads to the following expression for the total squared amplitude

$$\begin{aligned}\left(\frac{1}{2}\right)^2 \sum_{S_i,S_f} [\mathcal{M}_{fi}]^2 = \frac{e^4}{2m^4}\Big(\frac{(p_4\cdot p_1)^2 + (p_2\cdot p_1)^2 + 2m^2(p_1\cdot p_4 - p_1\cdot p_2)}{(p_3-p_1)^4} - \\ \frac{-2((p_3\cdot p_2)^2) - 2m^2(p_1\cdot p_2) + m^4}{(p_1+p_2)^2(p_3-p_1)^2} + \\ \frac{(p_4\cdot p_1)^2 + (p_3\cdot p_1)^2 + 2m^2(p_1\cdot p_3 - p_1\cdot p_4)}{(p_1+p_2)^4}\Big)\end{aligned} \tag{16}$$

Our goal now is to find an expression for the differential cross section which is our relevant observable. employing the following parametrization for the four-momenta

$$p_{1\mu} = (E, 0, 0, |p|) \tag{17a}$$

$$p_{2\mu} = (E, 0, 0, -|p|) \tag{17b}$$

$$p_{3\mu} = (E, 0, |p|\, sin\theta, |p|\, cos\theta) \tag{17c}$$

$$p_{4\mu} = (E, 0, -|p|\, sin\theta, -|p|\, cos\theta) \tag{17d}$$

it is a matter of simple algebra to show that the cross section becomes takes on the following, well-known, form

$$\left(\frac{d\sigma}{d\Omega}\right)_{CM} = \frac{\alpha^2}{8E^2}\left[\frac{cos^4\left(\frac{\theta}{2}\right)+1}{sin^4\left(\frac{\theta}{2}\right)} - 2\frac{cos^4\left(\frac{\theta}{2}\right)}{sin^2\left(\frac{\theta}{2}\right)} + \frac{1}{2}\left(1+cos^2\theta\right)\right] \tag{18}$$

We can rewrite the above expression in a more familiar way

$$\left(\frac{d\sigma}{d\Omega}\right)_{CM} = \frac{\alpha^2}{2s}\left[\frac{t^2}{s^2} + \frac{s^2}{t^2} + u^2\left(\frac{1}{s} + \frac{1}{t}\right)^2\right] \tag{19}$$

where the Mandelstam invariants are parameterized in the following way

$$s = 4E^2 \tag{20a}$$

$$t = -4E^2 sin^2(\theta/2) \tag{20b}$$

$$u = -4E^2 cos^2(\theta/2) \tag{20c}$$

Now, let us obtain the same result utilizing the spinor-helicity formalism. Recall, that we are working in the ultra relativistic limit. This alters the expression for the differential cross section as follows

$$\left(\frac{d\sigma}{d\Omega}\right)_{CM} = \frac{1}{8(2\pi)^2 4E^2}\left(\frac{1}{2}\right)^2 \sum_{S_i, S_f} [\mathcal{M}_{fi}]^2 \tag{21}$$

Note that we are still working the in the center of mass frame. The total amplitude, after some simplification, becomes

$$\mathcal{M}_{fi} = \frac{ie}{t}\bar{v}(p_2)\gamma^\mu v(p_4)\bar{u}(p_3)u(p_1) - \frac{ie^2}{s}\bar{u}(p_3)\gamma^\mu v(p_4)\bar{v}(p_2)\gamma_\mu u(p_1) \tag{22}$$

To obtain all the expressions which contribute to the total amplitude in the spinor helicity formalism is a straightforward process. One needs to simply apply the Feynman rules in the new formalism and find all non-zero contributions from each combination of helicities for both channels. [5] Going through this procedure, one obtains the following non-zero contributions of the squared amplitude

$$\left|\mathcal{M}_{fi}^{++++}\right|^2 = 4e^4\left(\langle 14\rangle[14]\right)^2\left(\frac{1}{t^2} + \frac{1}{s^2} + \frac{2}{st}\right) \tag{23a}$$

$$\left|\mathcal{M}_{fi}^{--++}\right|^2 = \frac{4e^2}{s^2}\left(\langle 13\rangle[13]\right)^2 \tag{23b}$$

$$\left|\mathcal{M}_{fi}^{-+-+}\right|^2 = \frac{4e^2}{t^2}\left(\langle 12\rangle[12]\right)^2 \tag{23c}$$

Making the following substitutions and rearranging terms accordingly gives back the same result for the cross section, eq. 7.

$$s = (p_1 + p_2)^2 = 2p_1 \cdot p_2 = \langle 12\rangle[12] \tag{24a}$$

[5] If one wishes to recreate this result for themselves, I refer them to (Elvang and Huang, 2015) for a pedagogical introduction to this subject.

$$-t = -(p_1 - p_3)^2 = 2p_1 \cdot p_3 = \langle 13 \rangle [13] \tag{24b}$$

$$-u = -(p_1 - p_4)^2 = 2p_1 \cdot p_3 = \langle 14 \rangle [14] \tag{24c}$$

In considering the use of spinor-helicity variables until this point, we are still tethered to the Lagrangian formalism writ large even though we have simplified what we have identified as often redundant Feynman rules.

3.3. *The Standard Model From an On-Shell Perspective*

At this stage, the obvious question to ask is whether massive particles can be incorporated into this framework and if various phenomena described by the standard model can be recovered. Much progress along this front has been made. In particular Arkani-Hamed, Huang T. and Huang Y., introducing the formalism of the so-called spin spinors, have accounted for particles of all masses and spins in the on-shell formalism (Arkani-Hamed, Huang T., Huang, Y, 2021).

More strikingly, Bachu and others have developed methods by which various mechanisms of the standard model can be reproduced. In particular, Bachu has shown how the mechanism for spontaneous mass generation, along with corresponding arrangement of particles and their dynamics, can be recovered without reference to Lagrangian, gauge symmetries or fields acquiring a non-zero vacuum expectation value. this is done by creating a full spectrum of amplitudes from UV to the IR and demanding that the IR amplitudes map onto the UV ones in the high energy limit.(Bachu, 2024)

Along with the constraints described above, **Unitarity** is the final constraint required to fix all amplitudes in this construction. This is the condition that all residues must be factorizable as a product of lower point amplitudes. For massless particles, we can express this as follows

$$\mathcal{A} = \frac{\mathcal{A}_L^{ah} \mathcal{A}_R^{a-h}}{P^2} \tag{25}$$

The analog of this constraint from QFT is the that Ward identity, $k_\mu \mathcal{M}^\mu = 0$, must hold. Unitarity presents an explicit constraint, once more, on the form of our amplitudes. Since this is all work that is very much in progress, and since the details of this work would take us beyond the scope of the work here, suffice to say that this philosophical methodology can aid in its progress. In identifying these constraints, it may be that we have given a operational set of elements one must start with to construct successive theories. What of the development of T' itself? It may be the case that subsequent developments from T' to an even further development, call it T'' will retain different aspects of T' and perhaps may even shed those elements that are retained from T. Nonetheless, the analysis, as we have undertaken it, serves piratical utility and allows continuity to come through as constraint, for change to be that which creates new possibility under the guise of formal structure.

The fact that all this structure can be recovered while substantially reducing the number of elements in T that are mapped onto T' is compelling precisely because the on-shell formalism leaves many of the elements behind from the original framework it builds off of. As outlined in §2, this is precisely the criterion laid out for the identification of non-trivial continuity.

4. Future Switches to Toggle

Although my focus here has been explicitly on physical theory, it is interesting question to consider whether the formulation of continuity I am developing here can be maintained when one looks at the practice of the biological sciences, for example, or even the construction of various models in the social sciences. For QFTs this process is certainly complicated. However for social scientific models where assumptions can be toggled in the context of simplified models, such a philosophical approach can serve as useful means by which a given assumption, rather than providing an at-bottom characteristic of the world, can have a marked outcome on how a given model maps onto a given empirical result and to outline explicit constraints on the set of models in a given context, taken as a whole.

Additionally, I have tried to begin with the upshot of this kind of approach as one that can serve as a real practical tool for the physicist, biologist, social scientist or any builder of models. Working out this method in specific contexts will surely present us with unique challenges in each case. In any case, the emphasis would be on practice. This explicitly comes through when one toggles the various levers of a given scheme and watches what unfolds.

At a broader scale, such an approach implicitly provides constraints on the scope of a given theory writ large. The question of our epistemic reach has always been a central one in philosophy and has, in many ways, been central to the pragmatist concern. If we adopt it and carry it out to its full capacity, it is inevitable that the tweaking of concepts present in our current frameworks needs to be carried out in such a fashion that it gives us clues for development, and more importantly, clues for correct empirical predictions and fits to given data sets. The conjecture that there must always be one non trivial continuity from an origin theory, T, to subsequent local iterations of T is the statement that traces the very limits of development, for it is only when all terms of an original scheme are shed, that the kind of continuity I view as useful is untenable.

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